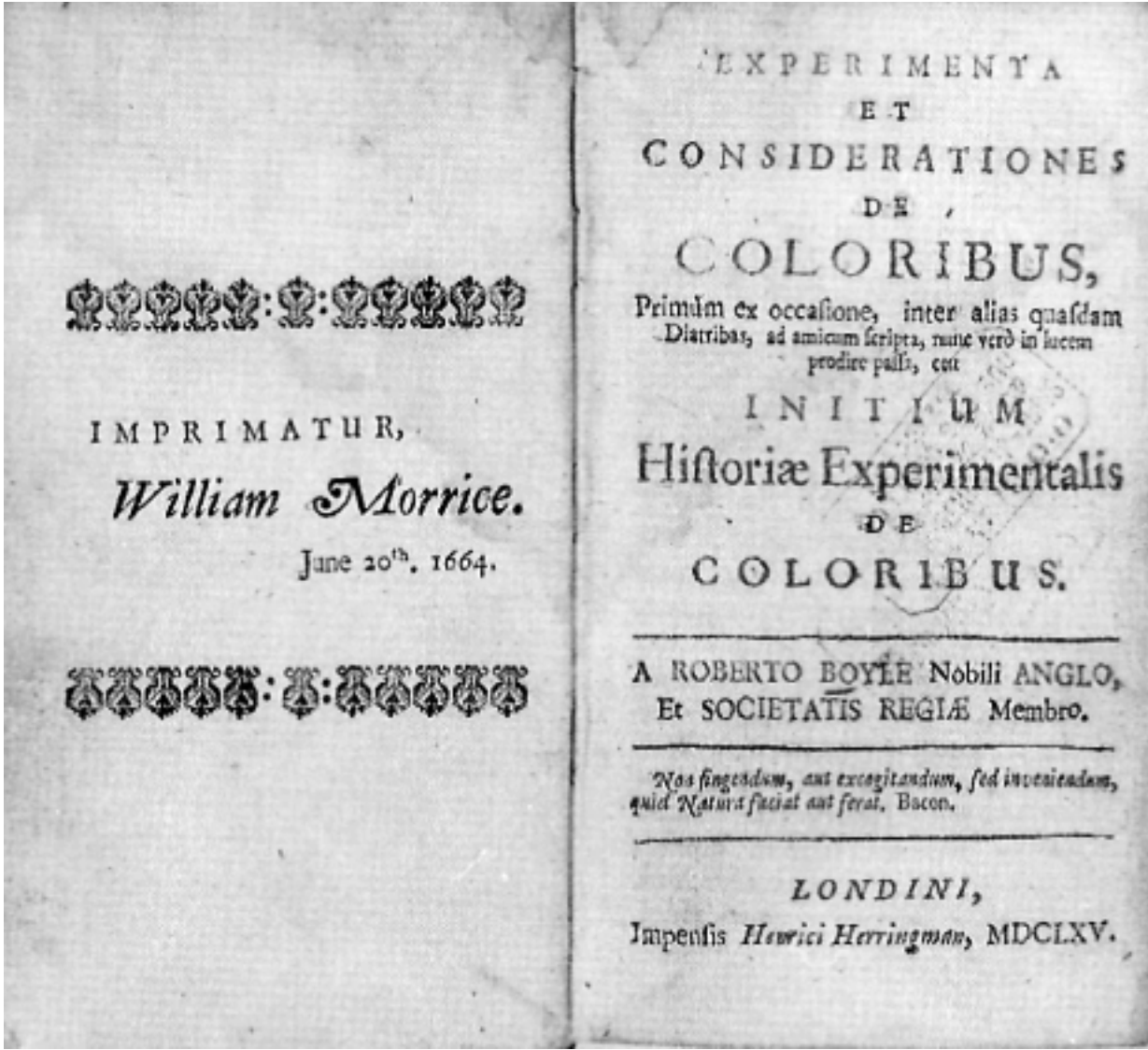


Frontmatter



The title-page of the Latin edition of *Colours* 1665). Facing it is the imprimatur of Sir William Morice, Secretary of State (1602-76). For details of this edition, which contains substantial passages not previously published in English, see below, pp. xiv-xv. (Cambridge University Library)

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Title Page

The Works of  
Robert Boyle  
Edited by  
Michael Hunter and  
Edward B. Davis  
volume 4  
*Colours and Cold*, 1664-5



LONDON

# Copyright Page

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## List of Abbreviations

Bartholin, <i>De nivis usu</i>	Thomas Bartholin, <i>De nivis usu medico observationes</i> (Copenhagen, 1661)
Birch, <i>Royal Society</i>	Thomas Birch, <i>The History of the Royal Society of London</i> (4 vols., London 1756-7)
BP	Royal Society Boyle Papers
<i>Correspondence</i>	Michael Hunter and Antonio Clericuzio (eds.), <i>The Correspondence of Robert Boyle</i> (6 vols., London, forthcoming)
	Gerrit de Veer, <i>The True and Perfect Description of Three Voyages ... by the Ships of Holland and Zeeland</i> (1598; Eng. trans., 1609), reprinted in Samuel Purchas, <i>Hakluytus Posthumus: or, Purchas his Pilgrimes: Contayning a History of the World in Sea Voyages and Lande Travells by Englishmen and Others</i> (London, 1625), pt. 2, bk. 3.
de Veer, <i>Three Voyages</i>	

F (in F1, etc.)	Reference no. in Fulton, <i>Bibliography</i> (see next item)
Fulton, <i>Bibliography</i>	J. F. Fulton, <i>A Bibliography of the Hon. Robert Boyle</i> (2nd edn., Oxford, 1961)
James, <i>Voyage</i>	Thomas James, <i>The Strange and Dangerous Voyage of Captain Thomas James in his intended Discovery of the Northwest Passage into the South-Sea</i> (London, 1633)
Lat.	Latin edition (see Introductory Note for elucidation as to which is referred to)
Magnus, <i>Historia</i>	Olaus Magnus, <i>Historia de gentibus septentrionalibus</i> (Rome, 1555)
<i>NRRS</i>	<i>Notes and Records of the Royal Society</i>
Olearius, <i>Relation</i>	Adam Olearius, <i>Relation du voyage de Moscovie, Tartarie, et de Perse</i> (Paris, 1656)
<i>Phil. Trans.</i>	<i>Philosophical Transactions</i>
Purchas, <i>Pilgrimes</i>	Samuel Purchas, <i>Hakluytus Posthumus: or, Purchas his Pilgrimes: Contayning a History of the World in Sea Voyages and Lande Travells by Englishmen and Others</i> (London, 1625)
<i>RBHF</i>	Michael Hunter (ed.), <i>Robert Boyle by Himself and his Friends</i> (London, 1994)

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## Stationers' Company

<i>Register</i>	G. E. B. Eyre and C. R. Rivington (eds.), <i>A Transcript of the Registers of the Worshipful Company of Stationers: from 1640 to 1708</i> (3 vols., London, privately printed, 1914)
<i>Term Catalogues</i>	Edward Arber (ed.), <i>The Term Catalogues, 1668-1709</i> (3 vols., London, 1903-6)

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## Introductory Notes

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## *Experiments and Considerations touching Colours* (1664)

### *Composition*

Boyle states in the preface that this work, 'written to a private Friend', was compiled 'by snatches, at several times, and places, and (after my manner) in loose sheets'; he added how, because he had 'carelesly laid by the loose Papers, for several years after they were written, when I came to put them together to dispatch them to the Press, I found some of those I reckon'd upon, to be very unseasonably wanting' (p. 5). He added how 'I did divers years ago shew some of them to a Learned Company of *Virtuosi*', which seems likely to be an allusion to the Oxford group of natural philosophers in the late 1650s (p. 6). Beyond this, the exact date of the work's composition is unclear. In the 'Burnet Memorandum', Boyle implies that his work on colours was inspired by the work on air published in *Spring of the Air*: 'The Affinity of Air and Colour led him to write of that'.<sup>†\*</sup> But there is reason to believe that an element of retrospective rationalisation entered into that section of the Memorandum, and such a timing is in any case rather late for the reference in the

preface to a gap of 'several years' prior to publication. What seems fairly clear is that research for the book was in progress alongside that for *Cold*. Not only are many of the same authorities cited in both, particularly travellers and explorers; in addition, Boyle's comments to Burnet seem to bear this out, since the sentence just quoted continues: 'he next writ of Cold which was a great prejudice to his health for the cold steams of ice and snow were too severe for so weak a body'. The one surviving section of the manuscript of the work, BP 17, fols. 65-6, is in a hand which is atypical, though it could be of 1650s date.

A more precise clue is offered by the author's note on p. 177, which refers to Christopher Merrett's translation of Antonio Neri's *Art of Glass* (1662) as published '6 or 7 years since the Writing of this 49th Experiment', which would give a composition date of 1655-6. In addition, references to a work on colours as already in existence appear in Robert Sharrock's *History of the Propagation and Improvement of Vegetables* (1660), in *The Sceptical Chymist* (1661), and in *The Usefulness of Natural Philosophy* (1663).<sup>†\*</sup> On the other hand, Boyle clearly continued to add material thereafter; one passage is apparently

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based on an observation by Dr Samuel Collins that appears in a work-diary of the early 1660s (BP 27, p. 93). 'The Publisher to the Reader' is signed by Henry Oldenburg, who evidently played a part in consigning the manuscript to the press, though this is not documented by extant correspondence.

The annexed 'Observations' on Clayton's diamond, dated 27 October 1663, take the form of a letter from Boyle to Sir Robert Moray, and were presented to the Royal Society on 28 October 1663.<sup>†\*</sup> Two manuscript copies of this survive, one, in the hand of Henry Oldenburg, in the Royal Society's Classified Papers; the other, in the hand of the Society's amanuensis, Richard Shortgrave, among the papers of Christiaan Huygens at Leiden, to whom Boyle intended Moray to forward this tract (this is shown by the covering letter which forms part of the book though it does not otherwise survive). These have been collated with the printed text, and minor variants (evidently due largely to compositorial error) have been noted.

## ***Publication***

It has sometimes been claimed - notably by J. F. Fulton - that a preliminary version of this work, entitled 'The History of Colours Begun' (a title echoing the sub-title of the work as published in 1664, together with the title to part 1, 'The Experimental History of Colours Begun'), was printed at London in 1663; Fulton gave this item the number 56 in his bibliography, despite the fact that no copy of it has ever turned up.<sup>†\*</sup> The authority he gives for this are 'frequent references' in 'contemporary booksellers' catalogues'. By this, he evidently means the various printed lists of Boyle's works issued in conjunction with editions of writings by him in his later years, tabulated in section 5 of the General Introduction, the most important of which are printed in various volumes below. It is in the list published in the *Second Continuation to Spring of the Air* (1680) that a '1663' edition of *Colours* first appears, entitled 'Experimentalis Colorum Historia inchoata: anno 1663', translated in the 1682 English version of this text as 'The Experimental History of Colours begun, A. 1663':<sup>†\*</sup> however, since there is no reference to a further edition of 1664 in either text, this is almost certainly simply a dating error, of which others also appear in this list.<sup>†\*</sup> Subsequently, in the list of Boyle's works published by Edward Jones in Latin in 1688, this item is elaborated as: 'Historia

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Manuscript material relating to *Colours*

Approx Location Location

MS location	Identity and nature of MS; comments	Hand(s) of MS	date of MS	in 1st edition	in this edition
BP 8, fols. 72v-3	'Addenda to the Booke of Colours', in 'Promiscuous Addenda to my severall Treatises', a work-diary from the 1660s. Unpublished observations on shining water and other phenomena in Jamaica, collected from 'Dr Stubbs', apparently intended for use in a later edition, should there have been one.	F	1660s		
BP 17, fols. 65-6	Fragment of MS version of text dealing with Olearius on climate and black skin, published in part II, experiment XI. The long sentence in square brackets on p. 155 and the following sentence are not found in the manuscript.	?1650s	1650s or early 1660s	151-7	84-7
BP 27, 93a	Paragraph about the colour of hares in Russia, one in a series of observations from Samuel Collins that are part of 'Promiscuous Experiments, Observations, & Notes', a work-diary from the 1660s. Probably the basis for published comments in part II, experiment XI, paragraph 1.		early 1660s	157	87
Royal Society, Cl. P. 9 (1) 11	Manuscript version of 'Observations about Mr. Clayton's Diamond'.	Oldenburg	1660s?	413-23	197-201
Royal Society Register Book, 2, 213-7 (RBC, 2, 105-11)	Manuscript copy of 'Observations about Mr. Clayton's Diamond'.		1663	413-23	197-201
Biblio-theek der Rijksuniversiteit Leiden, Huy 1194	Manuscript version of 'Observations about Mr. Clayton's Diamond'.	Richard Short-grave	1660s?	389-423	185-201

Published in Huygens, *Oeuvres complètes*, iv (The Hague, 1889), 495-8.

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Experimentalis de Coloribus incoepta, 8°, A. 1663, Sive Initium Historiae Experimentalis de Coloribus', translated in the 1689 English version of the list as 'An Experimental History of Colours begun, 8° 1663'.<sup>†\*</sup> However, this is probably a repetition of the 1680 dating error, the more elaborate title simply being based on components of that of the published work of 1664.

As far as we can establish, *Experiments and Considerations touching Colours* was in fact first published in 1664, 'Printed for *Henry Herringman* at the *Anchor* in the Lower walk of the New-Exchange' (F57). Herringman had registered the title in the Stationers' Register on 11 December 1663 (it is there given as 'The experimentall History of Colors in English and Lattin', and it is noted that it was entered under the hand of the Secretary of State, Sir William Morice, as well as the then warden of the company, Luke Fawne).<sup>†\*</sup> The title-page is printed in red and black. In the list of Boyle's works in *Cold* (p. 517) it is stated that this edition came out in May 1664. However, copies were evidently available for several weeks before this, since Boyle presented a copy to the Royal Society at a meeting on 30 March 1664.<sup>†\*</sup> An erratum following the contents leaf alerts the reader to the fact that a section is misplaced, and this has been moved to its correct position in the present edition.



A second edition was published in octavo in 1670 in London, also printed for Henry Herringman (F58). This edition is completely reset, but follows the 1664 edition sheet for sheet and page for page, not even correcting all of the errata; it even repeats the note concerning the misplaced section without implementing it. It was advertised in the Term Catalogues for November 1669; the price was 4s bound.<sup>†\*</sup>

### *Latin editions*

A Latin edition was published in duodecimo at London in 1665, printed for Henry Herringman; it was entitled *Experimenta et considerationes de coloribus, primum ex occasione, inter alias quasdam diatribas, ad amicum scripta, nunc vero in lucem prodire passa, ceu initium historiae experimentalis de coloribus* (F59). The imprimatur for the book is signed by William Morice, and dated 20 June 1664. The translator is not identified. Though it is conceivable that it was Henry Oldenburg, it might as easily have been an Oxford figure, as is suggested particularly by a letter from Oldenburg to Boyle of 10 September 1664, in which, noting Boyle's intimation to him of the point that the translation had by then reached, he continued: 'I cannot but conclude, there is somewhat more of the translation in your hands; which I intreat you to resolve me speedily in; that, in case there should be something yet wanting, I

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may presently go about it to perfect it'.<sup>†\*</sup> In this edition, quite significant additions are made to the text at various points, namely on pp. 159-60, 166-8, 170-1, 268-9, 273-8, 294-6 and 366-7. These are clearly the work of Boyle. Although these were included in all subsequent Latin editions, which are derived from this, they fail to appear either in the 1670 English edition or in Birch's text and have therefore hitherto been virtually unknown. They are reproduced here in footnotes to the text at the appropriate point, together with an English translation.

A further Latin edition was published in duodecimo by Gerbrandus Schagen at Amsterdam in 1667 (F60): this is in fact a reprint of the 'English' Latin edition of 1665, although in that year Boyle and Oldenburg had shown their apprehensions that it might be independently translated due to Herringman's delay in bringing the edition out.<sup>†\*</sup> Another edition of the same Latin text was brought out in Rotterdam in duodecimo in 1671 by Arnold Leers (F61).<sup>†\*</sup>

The same Latin text of *Experiments and Considerations touching Colours* was published in quarto by Samuel de Tournes in Geneva in 1676 (F62); in fact this was the earliest reprint of a work by Boyle that de Tournes brought out.<sup>†\*</sup> This was reissued in 1677 (F62A). Another edition was published in 1680; this has two issues, one with the imprint: 'Coloniae Allobrogum' (F63); the other with the imprint: 'Genevae' (F63A).

### *Impact and sequels*

A letter from Nathaniel Highmore to Boyle survives in which he writes: 'I have with a greate deale of pleasure reade over youre booke of coloures' and goes on to comment on various matters arising from the book. This is unfortunately undated, and has been linked in the *Correspondence* to the known date of publication of the edition.<sup>†\*</sup> What is evidently a commentary on the book by Benjamin Worsley is to be found in Evelyn Papers in the British Library, Add. MS 78685, no. 103. There are a handful of references to the Latin edition and its rather disappointing sale and impact in Boyle's correspondence with Oldenburg in 1666-7. A highly adulatory account of the work appears in Glanvill's *Plus Ultra*, and this set the tone for contemporary acclaim.<sup>†\*</sup> The reader of the work whose reaction is perhaps best known is Isaac Newton, who made extensive notes on it and who was apparently stimulated by it to

record his first known optical experiments.<sup>†\*</sup> Samuel Pepys' more equivocal response is familiar from his diary.<sup>†\*</sup>

A brief memorandum, 'Addenda to the Booke of Colours', survives as BP 8, fols. 72v-3, in one of Boyle's work diaries dating from the 1660s (see Table).

Modern scholarly attention to the book has particularly focused on Boyle's interest in colour indicators, which forms a significant component of the book.<sup>†\*</sup> A facsimile edition of the book with an introduction by M.B. Hall, in the series 'The Sources of Science', was published by Johnson Reprint Corporation in 1964.

### *The present text*

This is taken from the first edition of 1664. However, this has been collated with the Latin edition of 1665, and with the surviving manuscript texts, notably of the account of Clayton's diamond, and variants noted. The unusually substantial passages in the Latin text which fail to appear in the English one are included with translations in the footnotes at the relevant point in the text.

## *New Experiments and Observations Touching Cold, or an Experimental History of Cold, Begun (1665)*

### *Composition*

'Of Cold' is the subject of one of the essays in Boyle's 1649/50 list of writings. In addition, it appears in the further list of his writings of c. 1654, and he refers to his essay on this subject in his 'Essay of the Holy Scriptures' of c. 1652-4.<sup>†\*</sup> Such writings may have comprised the 'Old Observations, I made divers years ago' to which he refers in the letter to Viscount Brouncker dated 14 February 1663 that introduces the main section of the work (p. 263), while a further allusion to such earlier work is to be found on p. 301 of the published book, where he speaks of his consideration of ways of glaciation 'when I was scarce more than a Boy'. As to when the book as we have it was begun, the dating clues in the preface to the published work are not entirely consistent. Boyle refers to the dialogue on the history of heat and flame which he had written six or seven years earlier (i.e., approx. 1657-8, assuming that the preface precedes publication by about a year), implying that it seemed to him appropriate to write this account of 'the contrary quality' as a sequel at about the same time (p. 209): it is perhaps significant that the 'Examen of Antiperistasis' (pp. 459ff.) has the same interlocutors as that work.<sup>†\*</sup> There is also the evidence of parallelism with his work on colours, a substantial amount of which clearly dates from the late 1650s, as was shown in the previous Introductory Note. On the other hand, in the preface Boyle also alludes to his work on the properties of the air in *Spring of the Air*, together with his *Defence* of this book against Linus, and refers to the orders of the Royal Society in connection with the inception of the work (pp. 209-10). The latter probably relate particularly to the first section of the book, concerning thermometry: one of Robert Hooke's earliest tasks at the behest of the Royal Society had been to produce an accurately calibrated thermometer based on the sealed spirit thermometer brought from Italy by Sir Robert Southwell, and it was such an instrument that Boyle deployed in the research on which the book was based.<sup>†\*</sup>

Probably the book took shape in stages from the late 1650s onwards. The few manuscript fragments



of the text of *Cold* that survive are compatible with a date around 1660, since they are written in the hands of amanuenses that Boyle is known to have employed at that time. In one of these (BP 10, fols. 157-8), *Certain Physiological Essays* (1661) is referred to as already published. That other sections date from c. 1662 is suggested by such internal dating clues in the published text as the reference to King Charles X of Sweden's marching his army across frozen seas in January 1658 as occurring 'within these three years' (p. 354). One experiment dated 20 November 1662 appears in square brackets (p. 383), and, like other such passages scattered through the work, it evidently represents a relatively late addition. Boyle's reference in the preface to being forced to make experiments in a village, away from his apparatus (p. 213), is compatible with a date in the early 1660s, though this could also have occurred earlier.

As already noted, the letter introducing the main section of the work is dated 14 February 1663 (pp. 263-4). Yet, even if the book was almost complete by the date of this letter, Boyle clearly continued to add to it thereafter. Thus he included significant quantities of material that he obtained from Samuel Collins, physician to Czar Alexis Romanov from 1660 to 1669 and author of the posthumously and anonymously published *Present State of Russia* (1671), whom Boyle evidently first approached in this connection in the summer of 1663.<sup>†\*</sup> A further late addition was the opening section, 'New Thermometrical Experiments and Thoughts', which precedes the main body of the work which opens on p. 263. This was evidently the 'discourse concerning weather glasses' by Boyle that was 'read in great part' at the Royal Society on 3 February 1664, which Boyle excused himself from communicating on the grounds that its publication was imminent.<sup>†\*</sup> The lateness of the addition of this section to the book is indicated by its failure to appear in the list of contents, and it apparently reached Oldenburg subsequently to the main body of the book (though at least one component of it is in fact of earlier date, as Boyle's remarks on p. 253 make clear). From what is said in the 'Advertisement' to the 'Examen of the Doctrine of Antiperistasis', it also appears that work on that section continued after the text of the main *History* had been consigned to the press: it evidently reached Oldenburg only in mid-November 1664 (p. 461). Even after that, additions continued to be made. Work on the text was still in juxtaposition with a very severe winter, which must have been that of 1664-5, while a further very late addition is the information that Boyle includes in a note to p. 496 from the Earl of Carlisle, 'newly returned' from an embassy to Russia: he is known to have arrived home on 30 January 1665.<sup>†\*</sup>

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The final stages of the book's preparation for publication are well-evidenced in Oldenburg's letters to Boyle from September 1664 onwards.<sup>†\*</sup> It is apparent that it was intended to include Merrett's 'Account of Freezing' as a separately paginated annexe from the outset, though the relationship of this to the remainder of the work was a matter for discussion. The two plates included in the published work, one a folding one showing 'several figures of the thermoscope', the other a snow pit, were executed by Robert Hooke; in addition to being mentioned in a letter from Oldenburg to Boyle of 17 November, they are discussed in letters from Hooke to Boyle of 24 November and 13 and 15 December 1664.<sup>†\*</sup>

Manuscript material relating to *Cold* and the 'Appendix' (1683)

MS location	Identity and nature of MS; comments	Hand(s) of MS	Approx date of MS	Location in 1st edition	Location in this edition
BP 8, fol. 72v	'Collections for the Addenda to the Booke of Cold', in 'Promiscuous Addenda to my severall Treatises', a work-diary from the 1660s. Unpublished observations on an experiment in a bolt-head,	F	1660s		

collected from 'Dr Stubbs'.

BP 10, fol. 89v	MS version of the 21 titles for this work, as printed in list of contents.	early 1660s?	sigs. e1-2	226-7
BP 10, fols. 157-8	MS version of two passages about ice assuming the shapes of plants, if the water is first impregnated with 'the Saline parts of Plants'.	1650s	early 1660s	666-7; 447-8; 674-5 450-1

Final paragraph not published.

Cites *Certain Physiological Essays* as already published, so MS cannot predate 1661.

BP 26, fol. 204	MS version of opening paragraph, title I, as printed.	1650s	1650s or early 1660s	108-10	265-6
BP 27, p. 93b	A paragraph about thawing winds at Archangel, one in a series of observations from Samuel Collins that are part of 'Promiscuous Experiments, Observations, & Notes', a work-diary from the 1660s. Published (in much shortened form) as point 12 under 'Particulars referable to the XVIIIth Title' in the 'Appendix'.	?	1660s	Appendix, 23	569

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BP 28, pp. 259-63	Fragments of a set of notes on cold, many with information provided by Samuel Collins, all published in 'Appendix' to 2nd edition.	Bacon or N?	1660s or later	Appendix, 2-4, 23 and 27-8	551-3, 569, 573
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Numbered '44' to '68' in manuscript; numbered quite differently in published text.

May be copies of lost originals from 1660s onwards.

BP 29, fols. 76	Unpublished Latin translation of 'Examen of Antiperistasis', incl. 'Postscript'.	L	?		
BP 36, fol. 94	MS version of the 21 titles for this work, including subtitles to Title XXI, which missing from published contents list; one of the latter mistakenly has 'To the XXI Title' rather than XI title.		1660s?	sigs. e1-2	226-7
BP 38, fol. 74	Fragment of an experiment with cold temperatures and a glass egg, probably an early version of the experiment in section 11 of Title IX (dated there '4 Feb 1661').	1650s	early 1660s	265-70	314-16

Royal Society Register Book, 4, 112-17 (RBC, 3, 189-	Text of 'The Phaenomena of an Experiment about Freezing', as read at the Royal Society on 23 Nov. 1671 (and again on 14 Jan. 1675), and printed in Birch, <i>Royal Society</i> , ii, 492-5.	Richard Shortgrave	1671	Appendix, 6-9	555-8
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## Publication

The *Experimental History of Cold* was first published in octavo at London in 1665, 'Printed for John Crook, at the Sign of the Ship in St. Pauls Church-yard'; the title-page is printed in red and black (F70). It is introduced by an epistle from 'the Publisher to the Ingenious Reader', signed by Henry Oldenburg and dated 10 March 1665, which, in addition to praising the work, explains about some of the difficulties that had attended its publication, not least due to the severity of the weather in the winter of 1664-5. Much of what Oldenburg says there is also borne out by his letters to Boyle: indeed, since the book was printed in London whereas Boyle was in Oxford for much of the relevant period, its publishing history can be followed in more detail than is the case with almost any other of Boyle's works.

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Oldenburg's preface also explains how, partly due to the delay caused by the weather, the actual publication of the book was preceded by what effectively constituted a trial issue, and this is borne out by the minutes of the Royal Society. On 4 January 1665, informing the Society that the work 'was printing', Boyle promised to present the Society 'with about twenty heads concerning cold and congelation, to choose out of them experiments for trial in this frosty season'; a week later, he proved as good as his word, bringing in 'some printed copies of a part of his *Experimental History of Cold*, with a desire, that they might be recommended to the perusal of some of the society, to collect from thence such experiments as are there proposed and wished to be made, or such as were by him made but imperfectly'.<sup>†\*</sup>

All this also tallies with references to the book in *Philosophical Transactions* in these months. First, in number 1 (6 March 1665), the titles of the components of the work were given, and it was stated that it would by then have been published 'if the extremity of the late Frost had not stopt the Press'; thereafter, in issue no. 3, a summary of some of the main findings of the book and the matters dealt with in it is given in a manner that looks forward to Oldenburg's later reviews of Boyle's books, though at this point such reviews had not been formalised.<sup>†\*</sup>

Thereafter, a copy of the complete book was produced at a meeting of the Society on 12 April 1665.<sup>†\*</sup> John Crook entered the title in the Stationers' Register on the following day, 13 April.<sup>†\*</sup> On p. 517 of the published work is a catalogue of Boyle's published natural philosophical writings from *Spring of the Air* to *Colours*; this gives the month of publication of each work. It is followed by a list of the unpublished philosophical writings by Boyle that he had referred to in his publications; some of the latter were published between 1666 and 1671, but others were never published. An asterisked note disavows the intention of dealing with Boyle's theological works.

The first edition of the book appears to have sold quite well, and in a letter to Boyle of 30 December 1665, Oldenburg reported that there were 'not above 200 copies left'. However, it was not until 1683 that a second edition was brought out, a quarto printed for Richard Davis in Oxford (F71). It was advertised in the *Term Catalogues* for February 1683.<sup>†\*</sup> This contains a different catalogue of books from that given in F70, in a different position (generally it appears prior to p. 267 of that edition). This has not been reproduced here, since it is a straight reprint of the catalogue that had appeared in the English edition of *Second Continuation of Spring of the Air* the previous

year, except

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that it is updated to include the *Aerial* and *Icy Noctiluca*. It includes the paragraph that had appeared in *Second Continuation* disavowing the intention of dealing with theological books or *Philosophical Transactions* articles, together with asterisks denoting which books were available in Latin; however, the accompanying 'Advertisement' which explained these is missing.<sup>†\*</sup>

More significantly, although the text of the first edition is reprinted in the second edition without change (except for the addition of random error), an 'Appendix' is added, 'containing some promiscuous *Experiments* and *Observations* relating to the precedent History of *Cold*'. This is introduced by a new preface, 'The Publisher to the Reader', signed by 'J.W.', i.e. Boyle's servant and amanuensis, John Warr, which, though found in some copies at the beginning of the work as a whole, was not intended to introduce the entire book but the appendix. This preface is printed with the 1683 appendix below, following the 1665 text of the book; it explains the circumstances in which the appendix was compiled from Boyle's scattered notes while the new edition was in progress. The source and date of the material used is indicated partly by references within the text, since a number of the experiments that it contains bear dates from the 1660s, and partly by manuscript evidence, since several pages of the notes that were printed here survive, including further information from Dr Samuel Collins that Boyle evidently acquired in the 1660s. One section printed here, 'The Phaenomenon of an Experiment about Freezing' (pp. 555-8), had been read at a meeting of the Royal Society on 23 November 1671; it was then read again at the meeting on 14 January 1675.<sup>†\*</sup>

### ***Latin and continental editions***

Oldenburg states in his preface to the first edition that Boyle 'has already taken care of having it put into Latine', and correspondence between Oldenburg and Boyle makes it clear that the translation of the main body of the work was begun in November 1664, while the printing of the English edition was in progress. Initially, the intention appears to have been to translate only the first section of the work, the 'New Thermometrical Experiments and Thoughts', and printing of the Latin edition of this may have begun at a different press while the English edition was in progress in November 1664. It was certainly proceeding in June 1665, but was then postponed pending Oldenburg's translation, first, of two other sections of the book, 'An Examen of Antiperistasis' and 'An Examen of Mr Hobs' Doctrine touching Cold', and then of the remaining sections of the book. A further complication occurred in September, when Boyle authorised an Italian translation, which Oldenburg saw as a potential threat to the Latin one, though in fact it failed to materialise. Oldenburg completed the Latin version by mid-November

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1665, and over the next months there are further references to attempts to get the work printed: indeed, in a letter of 24 March 1666 Oldenburg reported to Boyle that the printer, Crooke, had printed the title-page, and that his corresponding booksellers in Holland had offered to 'take off the whole Latin impression'. But in fact the translation was never published, despite the fact that an isolated reference in a later letter shows that Crooke was still promising it in July 1668. It is possibly significant that Crooke was never given any subsequent work of Boyle's to print.<sup>†\*</sup>

In 1683, it is apparent from a letter from Thomas Hyde to Boyle of 14 July that year that the Oxford scholar, Christopher Wase, was translating the material added to the second edition into Latin.<sup>†\*</sup> However, this, too, never got into print.

### ***Impact and sequels***



This was the first of Boyle's works to be given coverage in Oldenburg's newly-founded *Philosophical Transactions*, which, as already noted, gave two accounts of the book, one an announcement prior to publication and the other a lengthy summary after the work had appeared. A similar adulatory account appeared in Glanvill's *Plus Ultra*.<sup>†\*</sup> The preface to the second edition speaks of the encouragement for this given by the book's favourable reception both at home and abroad, citing passages from books by Du Hamel and Conradt, while the receipt of copies of the book is confirmed by letters to Boyle, for instance one from John Wallis dated 29 April 1665 or one from Henry More dated 5 June 1665.<sup>†\*</sup> A more critical reaction was apparently represented by the Duchess of Newcastle's *Observations upon Natural Philosophy* (1666); though this does not cite Boyle's book by name, it is clear that it was one of the principal targets of some of her critical remarks concerning 'deluding Glasses and Experiments'.<sup>†\*</sup>

Apart from the material on the subject which was used in the Appendix to the second edition of 1683, Boyle collected further relevant information that was not deployed there, which is listed in the 'Table'. However, there is no evidence that he planned a further edition, as against a section of his 'Paralipomena'.

Modern attention to the book has been respectful but muted, perhaps

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partly due to its length. However, an important study of the work on thermometry that Boyle published here was made by M. Grilli, while Boyle's investigation of cold forms the principal case-study in Rose-Mary Sargent's account of Boyle's experimental work.<sup>†\*</sup> The book has also attracted attention because of the problems of credibility raised by Boyle's reliance on the testimony of others for his data concerning extreme climatic conditions, a matter which is discussed at length in his preface.<sup>†\*</sup>

### ***The present text***

The copy text for *Cold* is the first edition of 1665; variant readings (which are very scarce) are taken from the second (1683) edition. In addition to the errata listed on p. 225, which have been silently corrected except in the one case where authorial interference seems likely (p. 351), we have also introduced various emendations given by Boyle in a letter to Oldenburg of 23 December 1665, evidently in connection with the Latin translation of the work then about to be published (see above), together with one suggested by Oldenburg in a letter to Boyle of 8 July 1665; attention has been drawn to these at the appropriate point in the notes.<sup>†\*</sup> It is perhaps worth noting a feature of one part of the original text which we have silently ignored, namely where the compositor evidently ran out of Roman 'I's and used italic instead; this occurs in a rather haphazard manner at various points in the second and near the start of the third discourse of 'New Thermometrical Experiments and Thoughts'.



WORKS OF BOYLE

Volume 4

*COLOURS AND COLD, 1664-5*

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*Experiments and Considerations Touching Colours (1664)*

EXPERIMENTS  
and  
CONSIDERATIONS  
Touching  
COLOURS.

First occasionally Written, among some other  
*Essays*, to a Friend; and now suffer'd to  
come abroad as

THE  
BEGINNING  
Of An  
Experimental History  
OF  
COLOURS.

By the Honourable *ROBERT BOYLE*,  
Fellow of the ROYAL SOCIETY.

*Non fingendum, aut excogitandum, sed inveniendum,  
quid Natura faciat, aut ferat. Bacon.*

1664

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'What Nature may do or bring forth is not to be contrived or imagined, but rather uncovered.' This quotation is from *Novum organum* (1620), ii, 10, by Francis Bacon (1561-1626), statesman and philosopher. The same quotation is used on the title-page of *Cold* (below, p. 203).

In the original edition, this title-page has the imprint: '*LONDON*, Printed for *Henry Harringman*, at the *Anchor* in the Lower walk of the *New Exchange*. MDCLXIV.'

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Having in convenient places of the following Treatise, mention'd the Motives, that induc'd me to Write it, and the Scope I propos'd to my self in it, I think it superfluous to entertain the Reader now, with what he will meet with hereafter. And I should judge it needless, to trouble others, or my self, with any thing of Preface: were it not that I can scarce doubt, but this Book will fall into the hands of some Readers, who being unacquainted with the difficulty of attempts of this nature, will think it strange that I should publish any thing about Colours, without a particular Theory of them. But I dare expect that Intelligent and Equitable Readers will consider on my behalf: That the professed Design of this Treatise is to deliver things rather *Historical* than / *Dogmatical*, and consequently if I have added divers new *speculative* Considerations and hints, which perhaps may afford no despicable Assistance towards the framing of a solid and comprehensive Hypothesis, I have done at least as much as I promis'd, or as the nature of my Undertaking exacted. But another thing there is, which if it should be objected, I fear I should not be able so easily to answer it, and that is; That in the following Treatise (especially in the Third part of it) the Experiments might have been better Marshall'd, and some of them deliver'd in fewer words. For I must confess that this Essay was written to a private Friend,<sup>†\*</sup> and that too, by snatches, at several times, and places, and (after my manner) in loose sheets, of which I oftentimes had not all by me that I had already written, when I was writing more, so that it needs be no wonder if all the Experiments be not rang'd to the best Advantage, and if some connections and consecutions of them might easily have been mended. Especially since having carelessly laid by the loose Papers, for several years after they were written, when I came to put them together to dispatch them to the Press, I found some of those I reckon'd upon, to be very unseasonably wanting. And to make any great change in the order of the rest, was more than the Printers importunity, and that, of my own avocations / (and perhaps also considerabler sollicitations) would permit. But though some few preambles of the particular Experiments might have (perchance)

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been spar'd, or shorten'd, if I had had all my Papers under my View at once; Yet in the most of those Introductory passages, the Reader will (I hope) find hints, or Advertisements, as well as Transitions. If I sometimes seem to insist long upon the circumstances of a Tryall, I hope I shall be easily excused by those that both know, how nice divers experiments of Colours are, and consider, that I was not barely to *relate* them, but so as to teach a young Gentleman to make them. And if I was not sollicitous, to make a nicer division of the whole Treatise, than into three parts, whereof the One contains some Considerations about Colours in general. The Other exhibits a specimen of an Account of particular Colours, Exemplifi'd in Whiteness and Blackness. And the Third, promiscuous Experiments about the remaining Colours (especially Red) in order to a Theory of them. If, I say, I contented my self with this easie Division of my Discourse, it was perhaps because I did not think it so necessary to be Curious about the Method or Contrivance of a Treatise, wherein I do not pretend to present my Reader with a compleat Fabrick, or so much as Modell; but only to bring in Materials proper for the Building; And if I / did not well know how Ingenious the Curiosity and Civility of Friends makes them, to perswade Men by specious allegations, to gratifie their desires; I should have been made to believe by persons very well qualify'd to judge of matters of this nature, that the following Experiments will not need the addition of accurate Method and speculative Notions to procure Acceptance for the Treatise that contains them: For it hath been represented, That in most of them, as the Novelty will make them surprizing, and the Quickness of performance, keep them from being tedious; so the sensible changes, that are effected by them, are so manifest, so great, and so sudden, that scarce any will be displeased to see them, and those that are any thing Curious will scarce be able to see them, without finding themselves excited, to make Reflexions upon Them. But though with me, who love to measure Physical things by their *use*, not their *strangeness*, or *prettiness*, the partiality of others prevails not to make me over value these, or look upon them in themselves as other than Trifles: Yet I confess, that ever since I did divers years ago shew some of them to a Learned Company of *Virtuosi*:<sup>†\*</sup> so many persons of differing Conditions, and ev'n Sexes, have been Curious to see them, and pleas'd not to Dislike them, that I cannot Despair, but that by complying with those that urge the Publication / of them, I may both

gratify and excite the Curious, and lay perhaps a Foundation whereon either others or my self may in time superstruct a substantial Theory of Colours. And if *Aristotle*, after his Master *Plato*, have rightly observ'd Admiration to be the *Parent of Philosophy*,<sup>†\*</sup> the wonder, some of these Trifles have been wont to produce in all sorts of Beholders, and the access they have sometimes gain'd ev'n to the Closets of Ladies, seem to promise, that since the subject is so pleasing, that the Speculation appears as Delightfull as Difficult, such easie

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and recreative Experiments, which require but little time, or charge, or trouble in the making, and when made are sensible and surprizing enough, may contribute more than others, (far more important but as much more difficult) to recommend those parts of Learning (Chymistry and Corpuscular Philosophy) by which they have been produc'd, and to which they give Testimony ev'n to such kind of persons, as value a pretty Trick more than a true Notion, and would scarce admit Philosophy, if it approach'd them in another Dress: without the strangeness or endearments of pleasantness to recommend it. I know that I do but ill consult my own Advantage in the consenting to the Publication of the following Treatise: For those things, which, whilst men knew not how they were perform'd, / appear'd so strange, will, when the way of making them, and the Grounds on which I devis'd them, shall be Publick, quickly lose all that their *being Rarities*, and their *being thought Mysteries*, contributed to recommend them. But 'tis fitter for Mountebanks than Naturalists to desire to have their discoveries rather admir'd than understood, and for my part I had much rather deserve the thanks of the Ingenious, than enjoy the Applause of the Ignorant. And if I can so farr contribute to the discovery of the nature of Colours, as to help the Curious to it, I shall have reach'd my End, and sav'd my self some Labour which else I may chance be tempted to undergo in prosecuting that subject, and adding to this Treatise, which I therefore call a *History*, because it chiefly contains matters of fact, and which History the Title declares me to look upon but as *Begun*: Because though that above a hundred, not to say a hundred and fifty Experiments, (some loose, and others interwoven amongst the discourses themselves) may suffice to give a *Beginning* to a History not hitherto, that I know, begun by any; yet the subject is so fruitfull, and so worthy, that those that are Curious of these Matters will be farr more wanting to themselves than I can suspect, if what I now publish prove any more than a *Beginning*. For, as I hope my Endeavours / may afford them some assistance towards this work, so those Endeavours are much too Unfinish'd to give them any discouragement, as if there were little left for others to do towards the History of Colours.

For (first) I have been willing to leave unmention'd the *most part* of those Phænomena of Colours, that Nature presents us of her own accord, (that is, without being guided or overruled by man) such as the different Colours that several sorts of Fruites pass through before they are perfectly ripe, and those that appear upon the fading of flowers and leaves, and the putrifaction (and its several degrees) of fruits, &c. together with a thousand other obvious Instances of the changes of colours. Nor have I *much* medled with those familiar Phænomena wherein man is not an Idle spectator; such as the Greenness produc'd by salt in Beef much powder'd, and the Redness produc'd in the shells of Lobsters upon the boyling of those fishes; For I was willing to leave the *gathering of Observations* to those that have not the Opportunity to *make Experiments*. And for the same Reasons, among others, I did purposely omit the Lucriforous practise of Trades-men about colours; as

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the ways of making Pigments, of Bleaching wax, of dying Scarlet, &c. though to divers of them I be not a stranger, and of some I have my self made Tryall. /

Next; I did purposely pass by divers Experiments of other Writers that I had made Tryall of (and that not without registering some of their Events) unless I could some way or other improve them, because I wanted leasure to insert them, and had thoughts of prosecuting the work once begun of laying together those I had examin'd by themselves in case of my not being prevented by others

diligence. So that there remains not a little, among the things that are already published, to imploy those that have a mind to exercise themselves in repeating and examining them. And I will not undertake, that *none* of the things deliver'd, ev'n in this Treatise, though never so faithfully set down, may not prove to be thus farr of this Sort, as to afford the Curious somewhat to add about them. For I remember that I have somewhere in the Book it self acknowledged, that having written it by snatches, partly in the Countrey, and partly at unseasonable times of the year, when the want of fit Instruments, and of a competent variety of flowers, salts, Pigments, and other materials made me leave some of the following Experiments, (especialy those about Emphatical Colours) far more unfinished than they should have been, if it had been as easie for me to *supply* what was wanting to compleat them, as to *discern*. Thirdly to avoyd discouraging the young Gentleman I call

Pyrophilus,<sup>†\*</sup> whom / the less Familiar, and more Laborious operations of Chymistry would probably have frighted, I purposely declin'd in what I writ to him, the setting down any Number of such Chymicall Experiments, as, by being very elaborate or tedious, would either require much skill, or exercise his patience. And yet that this sort of Experiments is exceedingly Numerous, and might more than a little enrich the History of Colours, those that are vers'd in Chymical processes, will, I presume, easily allow me.

And (Lastly) for as much as I have occasion more than once in my several Writings to treat either purposely or incidentally of matters relating to Colours; I did not, perhaps, conceive my self oblig'd, to deliver in one Treatise *all* that I would say concerning that subject.

But to conclude, by summing up what I would say concerning what I *have* and what I *have not* done, in the following Papers, I shall not (*on the one side*) deny, that considering that I pretended not to write an accurate Treatise of Colours, but an Occasional Essay to acquaint a private friend with what then occurrd to me of the things I had thought or try'd concerning them; I might presume I did enough for once, if I did clearly and faithfully set down, though not all the Experiments I could, yet at least such a / variety of them, that an attentive Reader that shall consider the Grounds on which they have been made, and the hints that are purposely (though dispersedly) couched in them, may easily *compound* them, and otherwise vary them, so as very much to increase their Number. And yet (*on the other side*) I am so sensible both of

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how much I have, either out of necessity or choice, left undone, and of the fruitfullness of the subject I have begun to handle; that though I had performed far more then 'tis like many Readers will judge I have, I should yet be very free to let them apply to my Attempts that of *Seneca*, where having spoken of the Study of Natures Mysteries, and Particularly of the Cause of Earth-Quakes, he subjoins. *Nulla res consummata est dum incipit. Nec in hâc tantum re omnium maximâ ac involutissimâ, in quâ etiam cum multum actum erit, omnis ætas, quod agat inveniet; sed in omni alio Negotio, longè semper à perfecto fuere Principia.*<sup>†@</sup> /

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### ***The Publisher to the Reader.***

*Friendly Reader,*

Here is presented to thy view one of the Abstrusest as well as the Gentilest Subjects of Natural Philosophy, the *Experimentall History of Colours*; which though the Noble Author be pleased to think but *Begun*, yet I must take leave to say, that I think it so well begun, that the work is more than half dispatcht. Concerning which I cannot but give this advertisement to the Reader, that I have heard the Author express himself, that it would not surprise him, if it should happen to be objected, that some of these Experiments have been already published, partly by Chymists, and partly by two or three very fresh Writers upon other Subjects. And though the number of these Experiments be



but very small, and though they be none of the considerablest, yet it may on this occasion be further represented, that it is easie for our Author to name several men, (of whose number I can truly name my / self) who remember either their having seen him make, or their having read, his Accounts of the Experiments delivered in the following Tract several years since, and long before the publication of the Books, wherein they are mentioned. Nay in divers passages (where he could do it without any great inconvenience) he hath struck out Experiments, which he had tryed many years ago, because he since found them divulged by persons from whom he had not the least hint of them; which yet is not touched, with design to reflect upon any Ingenious Man, as if he were a Plagiary: For, though our Generous Author were not reserved enough in shewing his Experiments to those that expressed a Curiosity to see them (amongst whom a very Learned Man hath been pleased publickly to acknowledge it several years ago;<sup>†@</sup> yet the same thing may be well enough lighted on by persons that know nothing of

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one / another. And especially Chymical Laboratories may many times afford the same *Phænomenon* about Colours to several persons at the same or differing times. And as for the few *Phænomena* mentioned in the same Chymical writers, as well as in the following Treatise, our Author hath given an account, why he did not decline rejecting them, in the Anotations upon the 47th Experiment of the third part.<sup>†\*</sup> Not here to mention, what he elsewhere saith, to shew what use may be Justifiably made of Experiments not of his own devising by a writer of Natural History, if, what he employes of other mens, be well examined or verified by himself.

In the mean time, this Treatise is such, that there needs no other invitation to peruse it, but that tis composed by one of the Deepest & Most indefatigable searchers of Nature, which, I think the World, as far as I know it, affords. For mine own part, I feel a secret Joy within me, to see such beginings upon such *Themes*, it being demonstratively true, *Mota facilius moveri*,<sup>†\*</sup> which causeth me to entertain strong hopes, that this Illustrious *Virtuoso* and Restless Inquirer into Nature's Secrets will not stop here, but go on and prosper in the Disquisition of the other principal Colours, *Green, Red, and Yellow*. The Reasoning faculty set / once a float, will be carried on, and that with ease, especially, when the productions thereof meet, as they do here, with so greedy an Entertainment at home and a broad. I am confident, that the *ROYAL SOCIETY*, lately constituted by his *MOST EXCELLENT MAJESTY for improving Natural knowledge*, will Judge it their interest to exhort our Author to the prosecution of this Argument, considering, how much it is their design and business to accumulate a good stock of such accurate Observations and Experiments, as may afford them and their Offspring genuine Matter to raise a Masculine Philosophy upon, whereby the Mind of Man may be enobled with the Knowledge of solid Truths, and the Life of Man benefited with ampler accommodations, than it hath been hitherto.

Our Great Author, one of the Pillars of that Illustrious Corporation, is constantly furnishing large *Symbola*'s to this work, and is now falln, as you see, upon so comprehensive and important a theme, as will, if insisted on and compleated, prove one of the considerablest peeces of that structure. To which, if he shall please to add his Treatise of *Heat and Flame*,<sup>†\*</sup> as he is ready to publish his / Experimental Accounts of *Cold*, I esteem, the World will be obliged to Him for having shewed them both the *Right* and *Left Hand* of Nature, and the Operations thereof.<sup>†\*</sup>

The considering Reader will by this very Treatise see abundant cause to solicit the Author for more; sure I am, that of whatever of the Productions of his Ingeny comes into *Forein parts* (where I am happy in the acquaintance of

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many intelligent friends) is highly valued; And to my knowledge, there are those among the French,



that have lately begun to learn English, on purpose to enable themselves to read his Books, being impatient of their Traduction into Latin. If I durst say all, I know of the Elogies received by me from abroad concerning Him, I should perhaps make this Preamble too prolix, and certainly offend the modesty of our Author.

Wherefore I shall leave this, and conclude with desiring the Reader, that if he meet with other faults besides those, that the Errata take notice of (as I believe he may) he will please to consider both the weakness of the Authors eyes, for not reviewing, and the manifold Avocations / of the Publisher for not doing his part; who taketh his leave with inviting those, that have also considered this Nice subject experimentally, to follow the Example of our Noble Author, and impart such and the like performances to the now very inquisitive world. *Farewell.*

*H. O.<sup>†\*</sup> /*

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**The Contents.**

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- and why he has partly made use of it in the History of Whiteness and Blackness.* (3.)

Chap. 2.

- Some general Considerations are premis'd, first of the Insignificancy of the Observation of Colours in many Bodies* (4, 5.)
- and the Importance of it in others* (5.)
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- The reason why other particular Instances are in that place omitted* (9.)
- A necessary distinction about Colour premis'd* (10, 11.)
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<i>and about the Asperities of several other Colour'd Bodies</i>	(53.)
<i>And from these, and some premis'd Considerations, are propos'd some Conjectures; That the reason of the several Phænomena of Colours, afterwards to be met with, depends upon the Disposition of the Seen parts of the Object</i>	(54.)
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*Explain'd by Experiments* (65,  
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*That the Colour of Bodies may be Chang'd by the concurrence of two or more of these ways* (67.)  
*And besides all these, Eight Reflective causes of Colours, there may be in Transparent* (68,  
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#### Chap. 4.

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*and partly, that Genuine Colours are produc'd in Opacous Bodies by Reflection, and* (78.)  
*Emphatical in Transparent by Refraction*  
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*and other Emphatical Colours, and the suddain fading of Flowers, and other Bodies of Real* (80.)  
*ones*  
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*soever the Eye be*  
*which proceeds from the specular Reflection of the Wall* (82.)

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*Colours.*

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*Of the Nature of Whiteness and Blackness.*

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<i>from other Bodies</i>	



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*and by the Whiteness of Froth, rais'd from whites of Eggs beaten; that this Whiteness* (109,  
*comes not from the Air, shew'd by Experiments* 110.)

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*That it seems not necessary the Reflecting Surfaces should be Sphaerical, confirm'd by* (112,  
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*A Recital of some Opinions about Blackness, and which the Author inclines to* (117.)  
*which he further insists on and explicates* (118,  
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<i>That which follows about Inks was misplac'd by an Errour of the Printer, for it belongs to what has been formerly said of Galls</i>	(142, 143.) <sup>†*</sup>
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<i>The fifth Experiment, of the black Smoak of Camphire</i>	(144.)
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<i>The eighth Experiment, with Tin-glass, and Sublimate</i>	(147, 148.)
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<i>The ninth Experiment, of a Black powder of Gold in the bottom of Aqua-fortis, and of the Blacking of Refin'd Gold and Silver</i>	(148, 149.)
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<i>The tenth Experiment, of the staining Hair, Skin, Ivory, &amp;c. Black, with Crystals of Silver</i>	(150, 151.)
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<i>The eleventh Experiment, about the Blackness of the Skin, and Hair of Negroes, and Inhabitants of Hot Climates. Several Objections are made, and the whole Matter more fully discours'd, / and stated from several notable Histories and Observations<sup>†*</sup></i>	(from the 151 to the 167.)
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*The twelfth Experiment, of the white Powders, afforded by Precipitating several Bodies, as Crabs Eyes, Minium, Coral, Silver, Lead, Tin, Quick-silver, Tin-glass, Antimony, Benzoin, and Resinous Gumms out of Spirit of Wine, &c. but this is not Universal, since other Bodies, as Gold, Antimony, Quick-silver, &c. may be Precipitated of other Colours* (168, 169, 170.)

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*and of Whitening what would be Minium, and Copper, with Tin, and of Copper with Arsnick, which with Coppilling again Vanishes; of covering the Colour of that of 1/3 of Gold with 2/3 of Silver melted in a Mass together* (173, 174.)

*The fourteenth Experiment, of turning the black Body of Horn into a White immediately with Scraping, without changing the Substantial form, or without the Intervention of Salt, Sulphur, or Mercury* (176.)

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*Experiment the fifth, Of the Refracting and Reflecting Prismatical Colours in a light Room* (193.)

*Experiment the sixth, Of the Vanishing of the Iris of the Prism, upon the access of a greater adventitious Light* (194.) /

*Experiment the seventh, Of the appearances of the same Colour'd Papers by Candle-light* (195, 196.)

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Experiment <i>the ninth</i> , <i>Of the Greenish Blew transparency of Leaf Gold</i>	(198.)
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<i>The twenty seventh Experiment, of the changes of the Colour of Jasmin flowers, and Snow drops, by Alcalizate and Sulphureous Salts</i>	(263, 264.)
<i>The twenty eighth Experiment, of other differing Effects on Mary-golds, Prim-roses, and fresh Madder</i>	(265.)
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*Juices, instanced in Black-berries, and the Juices of Roses* 270.)

*Two reasons, why the Author added this twenty ninth  
Experiment, the last of which is confirm'd by an Instance of Mr. Parkinson, consonant to  
the Confession of the Makers of such Colours* (272.)

*The thirtieth Experiment, of several changes in Colours by*

*Digestion, exemplify'd by an Amalgam of ☉ [gold] and ☿*

*[mercury] and by Spirit of Harts-horn. And (to such as believe*

*it) by the changes of the Elixir. /*

*The thirty first Experiment, shewing that most Tinctures drawn by Digestion incline to a  
Red, instanc'd in Jalap, Guaicum, Amber, Benzoin, Sulphur, Antimony, &c.* (276,  
277.)

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*The thirty second Experiment, That some Reds with Diluting turn Yellow, others not,  
exemplify'd by the Tincture of Cochineel, and by Balsam of Sulphur, Tinctures of Amber,  
&c.* (277,  
278,  
279.)

*The thirty third Experiment, of a Red Tincture of Saccharum ꝑ and Oyl of Turpentine made  
by Digestion* (279.)

*The thirty fourth Experiment, of drawing a Volatile red Tincture of Mercury, whose Steams  
were white, but it would Tinge the Skin black* (279,  
280.)

*The thirty fifth Experiment, of a suddain way of making a Blood red Colour with Oyl of  
Vitriol, and Oyl of Anniseeds, two transparent Liquors* (280,  
281.)

*The thirty sixth Experiment, of the Degenerating of several Colours exemplify'd in the last  
mention'd Blood red, and by Mr. Parkinsons relation of Turnsol, by some Trials with the  
Juice of Buck-thorn Berries, and other Vegetables, to which several notable Considerations  
and Advertisements back'd with Experiments are adjoyn'd* (from  
281 to  
288.)  
/

*The thirty seventh Experiment, Of Varying the Colour of the Tinctures of Cochineel, Red-  
cherries, and Brasil, with Acid and Sulphureous Salts, and divers Considerations thereon* (from  
288 to  
290.)

*The thirty eighth Experiment, About the Red fumes of some, and White of other distill'd  
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291.)

*And of the various Colours of dry Sublimations, exemplify'd with several Experiments* (292,  
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<i>The thirty ninth Experiment, Of Varying the Decoction of Balaustiums with Acid and Urinous Salts</i>	(294, 295.)
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<i>The difference between a Chymical and Philosophical Solution of a Phænomenon</i>	(307, 308.)
<i>The Author's Chymical Explication of the Phænomena, confirm'd by several Experiments made on Mercury, with several Saline Liquors</i>	(from 308 to 310.)
<i>An Improvement of the fortieth Experiment, by a fresh Decoction of Antimony in a Lixivium</i>	(311, 312, 313.)
<i>Reflections on the tenth, twentieth, and fortieth Experiments, compar'd together, shewing a way with this Tincture / of Sublimate to distinguish whether any Saline Body to be examin'd be of a Urinous or Alcalizate Nature</i>	(from 314 to 317.)
<i>The Examination of Spirit of Sal-armoniack, and Spirit of Oak by these Principles</i>	(from 316 to 319.)
<i>That the Author knows ways of making highly Operative Saline Bodies, that produce none of the before mention'd Effects</i>	(319, 320.)

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<i>Some notable Experiments about Solutions and Precipitations of Gold and Silver</i>	(320, 321.)
<i>The one and fortieth Experiment, Of Depriving a deep Blew Solution of Copper of its Colour</i>	(322.)
<i>to which is adjoyn'd the Discolouring or making Transparent a Solution of Verdigrease &amp;c. and another of Restoring or Increasing it</i>	(322, 323.)
<i>The forty second Experiment, Of changing Milk white Precipitate of Mercury into a Yellow, by Affusion of fair Water, with several Considerations thereon</i>	(from 323 to 326.)

<i>The forty third Experiment, Of Extracting a Green Solution with fair Water out of imperfectly Calcin'd Vitriol</i>	(327.)
<i>The forty fourth Experiment, Of the Deepening and Diluting of several Tinctures, by the Affusions of Liquors, and by Conical Glasses that contain'd them, Exemplify'd in the Tinctures of Cochineel, Brasil, Verdigrease, Glass, Litmus, of which last on this occasion several pleasant Phænomena are related</i>	(from / 328 to 335.)
<i>To which are adjoyn'd certain Cautional Corollaries</i>	(335, 336.)
<i>The Water-drinker and some of his Legerdemain tricks related</i>	(337.)
<i>The forty fifth Experiment, Of the turning Rhenish and White Wine into a lovely Green, with a preparation of Steel</i>	(338, 339.)
<i>Some further Trial made about these Tinctures, and a Similar Experiment of Olaus Wormius</i>	(340.)
<i>The forty sixth Experiment, Of the Internal Colour of Metalls exhibited by Calcination</i>	(341, 342, 343.)
<i>Annotation the first, That several degrees of Fire may disclose a differing Colour</i>	(343.)
<i>Annotation the second, That the Glasses of Metalls may exhibit also other Kinds of Colours</i>	(344.)
<i>Annotation the third, That Minerals by several degrees of Fire may disclose several Colours</i>	(345.)
<i>Experiment the forty seventh, Of the Internal Colours of Metalls disclos'd by their Dissolutions in several Menstruums</i>	(from 345 to 350.)
<i>Annotation the first, The Authors Apology for Recording some already known Experiments, without mentioning their Authors</i>	(from 350 to 352.)
<i>Annotation the second, That some Minerals also by Dissolutions in Menstruums may exhibit divers Colours</i>	(353.)
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<i>The forty eighth Experiment, Of Tinging Glass Blew with Leaf Silver, and with Calcin'd</i>	(from

*Copper, and White with Putty* 355 to 358.)

*Annotation the first, That this white Glass is the Basis of Ammels* (358.)

*Annotation the second, That Colour'd Glasses may be Compounded like Colour'd Liquors in Dying Fats* (359.)

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*Annotation the third, Of Tinging Glass with Mineral Substances, and of trying what Metalls they contain by this means* (from 360 to 362.)

*Annotation the fourth, That Metalls may be Ting'd by Mineralls* (362, 363.)

*Annotation the fifth, Of making several Kinds of Amanses or Counterfeit Stones* (from 363 to 365.)

*Annotation the sixth, Of the Scarlet Dye, of the Stains of dissolv'd Gold and Silver* (366, 367.)

*Of the Greenness of Salt Beef, and Redness of Neats Tongues from Salts; of Gilding Silver with Bathe Water* (368, 369.)

*And Tinging the Nails and Skin with Alcanna* (369.)

*The forty ninth Experiment, Of making Lakes* (369.)

*A particular example in Turmerick* (370, 371.)

*Annotation the first, That in Precipitations wherein Allum is a Co-efficient, a great part of them may consist of the Stony particles of that Compound Body* (from 372 to 375.)

*Annotation the second, That Lakes may be made of other Substances, as Madder, Rue, &c. but that Alcalizate Salts do not always Extract the same Colour of which the Vegetable appears* (from 376 to 378.)

*Annotation the third, That the Experiments / related may Hint divers others* (378.)

*Annotation the fourth, That Alum is usefull for the preparing other than Vegetable Pigments* (379.)

*The fiftieth Experiment, Of the Similar effects of Saccarum ꝑ [Saturni] and Alkalies, of Precipitating with Oyl of Vitriol out of Aqua-fortis, and Spirit of Vinegar; and of divers Varyings of the Colours, with these Compounded* (from 380 to 384.)

*Another very pretty Experiment, with a Solution of Minium* (384,

*That these Experiments Skilfully digested may hint divers matters about Colours* (386.)

*The Authors Apologetick conclusion, in which is Cursorily hinted the Bow or Scarlet Dye* (387.)

*The Authors Letter to Sir Robert Moray, concerning his Observations on the Shining Diamond* (391. &c.)

*And the Observations themselves./*

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## Errata.<sup>†\*</sup>

Pag. 142. l. 20. These words, *And to manifest*, with the rest of what is by a mistake further printed in this fourth Experiment, belongeth, and is to be referred to the end of the second Experiment, p. 137 [see below, pp. 79, 81]. pag. 145. l. 1. leg. *matter* [for *Matters*]. 146. l. 4. leg. *Bolts-head* [for *Colts-head*]. pag. 161. in the marginal note l. 2. dele *de* [after *Hist.*]. ib. l. 3. lege *lib. I.* [for *lib. in fine*] p. 163. l. ult. insert *where* between the words *places* and *the*. p. 164. l. 1. dele *that* [after *Tumors*]. ibid. l. 8. leg. *Epidermis* [for *Epiderms*]. ibid. l. 19. leg. 300. *for* 200. p. 169 l. 22. leg. *into it* [for *in it*] p. 170. l. 23. & 24. leg. *Some Solutions hereafter to be mentioned, for the Solution of Potashes, and other Lixivate Salts.* p. 171. l. 6. insert *part of* between the words *most* and *dissolved*. p. 176. l. ult. insert the particle *it* between the words *Judged* and *not* p. 234. l. 4. leg. *Woud-wax* or *Wood-wax* [for *Luteola* or *Wood-wax*]. p. 320. l. 29. leg *urine* for *Urne*./

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## *The Experimental History of Colours Begun.*

### The First Part.

#### Chap. I.

1 I have seen you so passionately addicted, *Pyrophilus*,<sup>†\*</sup> to the delightful Art of Limning and Painting, that I cannot but think my self obliged to acquaint you with some of those things that have occurred to mee concerning the changes of Colours. And I may expect that I shall as well serve the *Virtuosi* in general, as gratifie you in particular, by furnishing a person, who, I hope, will both improve my Communications, and communicate his Improvements, with such Experiments and Observations as may both invite you to enquire seriously into the Nature of Colours, and assist you in the Investigation of it. This being the principal scope of the following Tract, I should do that which might prevent my own design, / if I should here attempt to deliver you an accurate and particular Theory of Colours; for that were to present you with what I desire to receive from you; and, as farr as in mee lay, to make that study needless, to which I would engage you.

2 Wherefore my present work shall be but to divert and recreate, as well as excite you by the delivery of matters of fact, such as you may for the most part try with much *ease*, and possibly not without some *delight*: And lest you should expect any thing of Elaborate or Methodical in what you will meet with here, I must confess to you before-hand, that the seasons I was wont to chuse to devise and try Experiments about Colours, were those daies, wherein having taken Physick, and finding my self as unfit to speculate, as unwilling to be altogether idle, I chose this diversion, as a kind of Mean betwixt the one and the other. And I have the less scrupled to set down the following



Experiments, as some of them came to my mind, and as the Notes wherein I had set down the rest, occur'd to my hands, that by declining a Methodical way of delivering them, I might leave you and my self the greater liberty and convenience to add to them, and transpose them as shall appear expedient. /

3 Yea, that you may not think mee too reserv'd, or look upon an Enquiry made up of meer Narratives, as somewhat jejune, I am content to

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*premise* a few considerations, that now offer themselves to my thoughts, which relate in a more general way, either to the Nature of Colours, or to the study of it. And I shall *insert* an *Essay*, as well Speculative as Historical, of the Nature of Whiteness and Blackness, that you may have a *Specimen* of the History of Colours I have sometimes had thoughts of; and if you dislike not the Method I have made use of, I hope, you, and some of the *Virtuosi*, your friends, may be thereby invited to go thorow with *Red, Blew, Yellow*, and the rest of the particular Colours, as I have done with *White* and *Black*, but with farr more sagacity and success. And if I can invite Ingenious men to undertake such Tasks, I doubt not but the Curious will quickly obtain a better Account of Colours, than as yet we have, since in our Method the Theorical part of the Enquiry being attended, and as it were interwoven with the Historical, whatever becomes of the disputable Conjectures, the Philosophy of Colours will be promoted by the indisputable Experiments./

## Chap. II.

1 To come then in the first place to our more general Considerations, I shall begin with saying something as to the Importance of examining the Colours of Bodies. For there are some, especially *Chymists*, who think, that a considerable diversity of Colours does constantly argue an equal diversity of Nature, in the Bodies wherein it is conspicuous; but I confess I am not altogether of their mind; for not to mention changeable Taffaties, the blew and golden necks of Pidgeons, and divers Water-fowl, Rainbows Natural and Artificial, and other Bodies, whose Colours the Philosophers have been pleased to call not Real, but Apparent and Phantastical; not to insist on these, I say, (for fear of needlesly engaging in a Controversie) we see in Parrots, Goldfinches, and divers other Birds, not only that the contiguous feathers which are probably as near in properties as place, are some of them Red, and others White, some of them Blew, & others Yellow, &c. but that in the several parts of the self-same feather there may often be seen the greatest disparity of Colours; and so in the leaves of Tulips, July-flowers, and some other Vegetables / the several leaves, and even the several parts of the same leaf, although no difference have been observed in their other properties, are frequently found painted with very different Colours. And such a variety we have much more admired in that lovely plant which is commonly, and not unjustly call'd the *Marvayl of Peru*; for of divers scores of fine Flowers, which in its season that gaudy Plant does almost daily produce, I have scarce taken notice of any two that were dyed perfectly alike. But though *Pyro*: such things as these, among others, keep mee from daring to affirm, that the Diversity and change of Colours does *alwaies* argue any great difference or alteration, betwixt, or in, the Bodies, wherein it is to be discerned, yet that *oftentimes* the Alteration of Colours does signifie considerable Alterations in the disposition of parts of Bodies, may appear in the Extraction of Tinctures, and divers other Chymical Operations, wherein the change of Colours is the chief, and sometimes

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the only thing, by which the Artist regulates his proceeding, and is taught to know when 'tis seasonable for him to leave off. Instances of this sort are more obvious in divers sorts of fruits, as Cherries, Plums, &c. wherein, according as the Vegetable sap is sweetned, or otherwise / ripened, by passing from one degree to another of Maturation, the external part of the fruit passes likewise from one to another Colour. But one of the noblest Instances I have met with of this kind, is not so obvious; and that is the way of tempering Steel to make Gravers, Drills, Springs, and other

Mechanical Instruments, which we have divers times both made Artificers practise in our presence, and tryed our selves, after the following manner. First, the slender Steel to be tempered is to be hardened by heating as much of it as is requisite among glowing Coals, till it be glowing hot, but it must not be quenched as soon as it is taken from the fire (for that would make it too brittle, and spoil it) but must be held over a bason of water, till it descend from a White heat to a Red one, which as soon as ever you perceive, you must immediately quench as much as you desire to harden in the cold water. The Steel thus hardened, will, if it be good, look somewhat White and must be made bright at the end, that its change of Colours may be there conspicuous; and then holding it so in the flame of a Candle, that the bright end may be, for about half an inch, or more, out of the flame, that the smoak do not stain or sully the brightness of it, you shall after a / while see that clean end, which is almost contiguous to the flame, pass very nimbly from one Colour to another, as from a brighter Yellow, to a deeper and reddish Yellow, which Artificers call a *sanguine*, and from that to a fainter first, and then a deeper Blew. And to bring home this Experiment to our present purpose, it is found by daily Experience, that each of these succeeding Colours argue such a change made in the texture of the Steel, that if it be taken from the flame, and immediately quenched in the tallow (whereby it is settled in whatever temper it had before) when it is Yellow, it is of such a hardness as makes it fit for Gravers Drills, and such like tools; but if it be kept a few minutes longer in the flame till it grow Blew, it becomes much softer, and unfit to make Gravers for Metalls, but fit to make Springs for Watches, and such like Instruments, which are therefore commonly of that Colour; and if the Steel be kept in the flame, after that this deep Blew hath disclosed it self, it will grow so soft, as to need to be new hardened again, before it can be brought to a temper, fit for Drills or Penknives. And I confess *Pyro*. I have taken much pleasure to see the Colours run along from the parts of the Steel contiguous to the flame, to the end of the Instrument, / and succeed one another so fast, that if a man be not vigilant, to thrust the Steel into the tallow at the very nick of time, at which it has attain'd its due Colour, he shall miss of giving his tool the right temper. But because the flame of a Candle is offensive to my weak eyes, and because it is apt to either black or sully the contiguous part of the Steel which is held in it, and thereby hinder the change of Colours from being so long and clearly discern'd, I have sometimes made this Experiment by laying the Steel to be tempered upon a heated bar of Iron, which we finde also to be

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employ'd by some Artificers in the tempering of such great Instruments, as are too big to be soon heated sufficiently by the flame of a Candle. And you may easily satisfie your self *Pyro*: of the differing hardness and toughness, which is ascribed to Steel temper'd at different Colours, if you break but some slender wires of Steel so temper'd, and observe how they differ in brittleness, and if with a file you also make tryal of their various degrees of hardness.

2 But *Pyrophilus*, I must not at present any further prosecute the Consideration of the importance of Experiments about Colours, not only because you will in the following papers finde some Instances, that would here / be presented you out of their due place, of the use that may be made of such Experiments, in discovering in divers bodies, what kind the salt is, that is predominant in them; but also because a speculative Naturalist might justly enough allege, that as Light is so pleasing an object, as to be well worth our looking on, though it discover'd to us nothing but its self; so modifi'd Light called Colour, were worth our contemplation, though by understanding its Nature we should be taught nothing else. And however, I need not make either you or my self excuses for entertaining you on the subject I am now about to treat of, since the pleasure *Pyro*: takes in mixing and laying on of Colours, will I presume keep him, and will (I am sure) keep mee from thinking it troublesome to set down, especially after the tedious processes (about other matters) wherewith I fear I may have tyr'd him, some easie, and not unpleasant Experiments relating to that subject.

3 But, before we descend to the more particular considerations, we are to present you concerning Colours, I presume it will be seasonable to propose at the very entrance a Distinction; the ignorance or neglect of which, seems to mee to have frequently enough occasioned either mistakes or

confusion / in the Writings of divers Modern Philosophers; for Colour may be considered, either as it is a quality residing in the body that is said to be coloured, or to modify the light after such or such a manner; or else as the Light it self, which so modifi'd, strikes upon the organ of sight, and so causes that Sensation which we call Colour; and that this latter may be look'd upon as the more proper, though not the usual acception of the word Colour, will be made probable by divers passages in the insuing part of our discourse; and indeed it is the Light it self, which after a certain manner, either mingled with shades, or some other waies troubled, strikes our eyes, that does more immediately produce that motion in the organ, upon whose account men say they see such or such a Colour in the object; yet, because there is in the body that is said to be coloured, a certain disposition of the superficial particles, whereby it sends the Light reflected, or refracted, to our eyes thus and thus alter'd, and not otherwise, it may also in some sense be said, that Colour depends upon the visible body; and therefore we shall not be against that way of speaking of Colours that is most us'd among the Modern Naturalists, provided we be allowed to have recourse when occasion shall / require to the premis'd distinction, and to take the more immediate cause of Colour to be

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the modifi'd Light it self, as it affects the Sensory; though the disposition also of the colour'd body, as that modifies the Light, may be call'd by that name Metonimically (to borrow a School term) or Efficiently, that is in regard of its turning the Light, that rebounds from it, or passes thorow it, into this or that particular Colour.

4 I know not whether I may not on this occasion add, that Colour is so far from being an Inherent quality of the object in the sense that is wont to be declar'd by the Schools, or even in the sense of some Modern Atomists, that, if we consider the matter more attentively, we shall see cause to suspect, if not to conclude, that though Light do more immediately affect the organ of sight, than do the bodies that send it thither, yet Light it self produces the sensation of a Colour, but as it produces such a determinate kind of local motion in some part of the brain; which, though it happen most commonly from the motion whereinto the slender strings of the *Retina* are put, by the appulse of Light; yet if the like motion happen to be produc'd by any other cause, wherein the Light concurs not at / all, a man shall think he sees the same Colour. For proof of this, I might put you in mind, that 'tis usual for dreaming men to think they see the Images that appear to them in their sleep, adorn'd some with this, and some with that lively Colour, whilst yet, both the curtains of their bed, and those of their eyes are close drawn. And I might add the confidence with which distracted persons do oftentimes, when they are awake, think, they see black fiends in places, where there is no black object in sight without them. But I will rather observe, that not only when a man receives a great stroak upon his eye, or a very great one upon some other part of his head, he is wont to see, as it were, flashes of lightning, and little vivid, but vanishing flames, though perhaps his eyes be shut: But the like apparitions may happen, when the motion proceeds not from something without, but from something within the body, provided the unwonted fumes that wander up and down in the head, or the propagated concussion of any internal part in the body, do cause about the inward extremities of the Optick Nerve, such a motion as is wont to be there produc'd, when the stroak of the Light upon the *Retina* makes us conclude, that we see either Light, or such and such a / Colour: This the most ingenious *Des Cartes* hath very well observ'd,<sup>†\*</sup> but because he seems not to have exemplifi'd it by any unobvious or peculiar observation, I shall indeavour to illustrate this doctrine by a few Instances.

5 And first, I remember, that having, through Gods goodness, been free for several years, from troublesome Coughs, being afterwards, by an accident, suddenly cast into a violent one, I did often, when I was awaked in the night by my distempers, observe, that upon coughing strongly, it would seem to mee, that I saw very vivid, but immediately disappearing flames, which I took particular notice of, because of the conjecture I am now mentioning.

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6 An excellent and very discreet person, very near ally'd both to you and mee,<sup>†\*</sup> was relating to mee, that some time since, whilst she was talking with some other Ladies, upon a sudden, all the objects, she looked upon, appeared to her dyed with unusual Colours, some of one kind, and some of another, but all so bright and vivid, that she should have been as much delighted, as surpriz'd with them, but that finding the apparition to continue, she fear'd it portended some very great alteration as to her health: As indeed the day after she was assaulted / with such violence by Hysterical and Hypochondrical Distempers, as both made her rave for some daies, and gave her, during that time, a Bastard Palsey.

7 Being a while since in a Town, where the Plague had made great havock, and inquiring of an ingenious man, that was so bold, as without much scruple to visit those that were sick of it, about the odd symptomes of a Disease that had swept away so many there; he told mee, among other things, that he was able to tell divers Patients, to whom he was called, before they took their beds, or had any evident symptomes of the Plague, that they were indeed infected, upon peculiar observations, that being asked, they would tell him that the neighbouring objects, and particularly his cloths, appear'd to them beautifi'd with most glorious Colours, like those of the Rainbow, oftentimes succeeding one another; and this he affirm'd to be one of the most usual, as well as the most early symptomes, by which this odd Pestilence disclos'd it self: And when I asked how long the Patients were wont to be thus affected, he answered, that it was most commonly for about a day; and when I further inquired whether or no Vomits, which in that Pestilence were usually given, did not remove this symptome / (For some used the taking of a Vomit, when they came ashore, to cure themselves of the obstinate and troublesome giddiness caus'd by the motion of the ship) reply'd, that generally, upon the evacuation made by the Vomit, that strange apparition of Colours ceased, though the other symptomes were not so soon abated, yet he added (to take notice of that upon the by, because the observation may perchance do good) that an excellent Physician, in whose company he was wont to visit the sick, did give to almost all those to whom he was called, in the beginning before Nature was much weakened, a pretty odd Vomit consisting of eight or ten drammes of Infusion of *Crocus Metallorum*, and about half a dramm, or much more, of White Vitriol, with such success, that scarce one of ten to whom it was seasonably administred, miscarried.

8 But to return to the consideration of Colours: As an apparition of them may be produced by motions from within, without the assistance of an outward object, so I have observed, that 'tis sometimes possible that the Colour that would otherwise be produced by an outward object, may be chang'd by some motion, or new texture already produced in the Sensory, as long as that unusual motion, or new disposition / lasts; for I have divers times try'd, that after I have through a Telescope look'd upon the Sun, though

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thorow a thick, red, or blew glass, to make its splendor supportable to the eye, the impression upon the *Retina*, would be not only so vivid, but so permanent, that if afterwards I turned my eye towards a flame, it would appear to mee of a Colour very differing from its usual one. And if I did divers times successively shut and open the same eye, I should see the adventitious Colour (if I may so call it) changed or impair'd by degrees, till at length (for this unusual motion of the eye would not presently cease) the flame would appear to mee, of the same hew that it did to other beholders; a not unlike effect I found by looking upon the Moon, when she was near full, thorow an excellent Telescope, without colour'd Glass to screen my eye with; But that which I desire may be taken notice of, because we may elsewhere have occasion to reflect upon it, and because it seems not agreeable to what Anatomists and Optical Writers deliver, touching the relation of the two eyes to each other, is this circumstance, that though my Right eye, with which I looked thorow the Telescope, were thus affected by the over-strong impression of the light, yet when the flame / of a Candle, or some other bright object appear'd to me of a very unusual Colour, whilst look'd upon with the Discompos'd Eye, or (though not so notably) with both eyes at once; yet if I shut that Eye, and looked upon the same object with the other, it would appear with no other than its usual Colour,



though if I again opened, and made use of the Dazled eye, the vivid adventitious Colour would again appear. And on this occasion I must not pretermitt an Observation which may perswade us, that an over-vehement stroak upon the Sensory, especially if it be naturally of a weak constitution, may make a more lasting impression than one would imagine, which impression may in some cases, as it were, mingle with, and vitiate the action of vivid objects for a long time after.

For I know a Lady of unquestionable Veracity, who having lately, by a desperate fall, receiv'd several hurts, and particularly a considerable one upon a part of her face near her Eye, had her sight so troubl'd and disorder'd, that, as she hath more than once related to me, not only when the next morning one of her servants came to her bed side, to ask how she did, his cloaths appear'd adorn'd with such variety of dazling Colours, that she was fain presently to / command him to withdraw, but the Images in her Hangings, did, for many daies after, appear to her, if the Room were not extraordinarily darken'd, embellish'd with several offensively vivid Colours, which no body else could see in them; And when I enquir'd whether or no White Objects did not appear to her adorn'd with more luminous Colours than others, and whether she saw not some which she could not now well describe to any, whose eyes had never been distemper'd, she answer'd mee, that sometimes she thought she saw Colours so new and glorious, that they were of a peculiar kind, and such as she could not describe by their likeness to any she had beheld either before or since, and that White Objects did so much disorder her sight, that if several daies after her fall, she look'd upon the inside of a Book, she fanci'd she saw there Colours like those of the Rain-bow, and even when she thought her self pretty well recover'd, and made bold to leave her Chamber,

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the coming into a place where the Walls and Ceeling were whited over, made those Objects appear to her cloath'd with such glorious and dazling Colours, as much offended her sight, and made her repent her venturousness, and she added, that this Distemper of her Eyes lasted no less / than five or six weeks, though, since that, she hath been able to read and write much without finding the least Inconvenience in doing so. I would gladly have known, whether if she had shut the Injur'd Eye, the *Phænomena* would have been the same, when she employ'd only the other, but I heard not of this accident early enough to satisfie that Enquiry.

9 Wherefore, I shall now add, that some years before, a person exceedingly eminent for his profound Skil in almost all kinds of Philological Learning, coming to advise with mee about a Distemper in his Eyes, told me, among other Circumstances of it, that, having upon a time looked too fixedly upon the Sun, thorow a Telescope, without any coloured Glass, to take off from the dazling splendour of the Object, the excess of Light did so strongly affect his Eye, that ever since, when he turns it towards a Window; or any White Object, he fancies, he seeth a Globe of Light, of about the bigness the Sun then appeared of to him, to pass before his Eyes: And having inquir'd of him, how long he had been troubled with this Indisposition, he reply'd, that it was already nine or ten years, since the Accident, that occasioned it, first befel him. /

10 I could here subjoyn, *Pyrophilus*, some memorable Relations that I have met with in the Account given us by the experienc'd *Epiphanius Ferdinandus*, of the Symptomes he observ'd to be incident to those that are bitten with the Tarantula,<sup>†\*</sup> by which (Relations) I could probably shew, that without any change in the Object, a change in the Instruments of Vision may for a great while make some Colours appear Charming, and make others Provoking, and both to a high degree, though neither of them produc'd any such Effects before. These things, I say, I could here subjoyn in confirmation of what I have been saying, to shew, that the Disposition of the Organ is of great Importance in the Dijudications we make of Colours, were it not that these strange Stories belonging more properly to another Discourse, I had rather, (contenting my self to have given you an Intimation of them here) that you should meet with them fully deliver'd there.<sup>†\*</sup>



1 But, *Pyrophilus*, I would not by all that I have hitherto discours'd, be thought to have forgotten the Distinction / (of Colour) that I mentioned to you about the beginning of the third Section of the former Chapter; and

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therefore, after all I have said of Colour, as it is modifi'd Light, and immediately affects the Sensory, I shall now re-mind you, that I did not deny, but that Colour might in some sense be consider'd as a Quality residing in the body that is said to be Colour'd; and indeed the greatest part of the following Experiments referr to Colour principally under that Notion, for there is in the bodyes we call Colour'd, and chiefly in their Superficial parts, a certain disposition, whereby they do so trouble the Light that comes from them to our Eye, as that it there makes that distinct Impression, upon whose Account we say, that the Seen body is either White or Black, or Red or Yellow, or of any one determinate Colour. But because we shall (God permitting) by the Experiments that are to follow some Pages hence, more fully and particularly shew, that the Changes, and consequently in divers places the Production and the appearance of Colours depends upon the continuing or alter'd Texture of the Object, we shall in this place intimate (and that too but as by the way) two or three things about this Matter.

2. And first it is not without some Reason, / that I ascribe Colour (in the sense formerly explan'd) *chiefly* to the Superficial parts of Bodies, for not to question how much Opacous Corpuscles may abound even in those Bodies we call Diaphanous, it seems plain that of Opacous bodies we do indeed see little else than the Superficies, for if we found the beams of Light that rebound from the Object to the Eye, to peirce deep into the Colour'd body, we should not judge it Opacous, but either Translucid, or at least Semi-diaphanous, and though the Schools seem to teach us that Colour is a Penetrative Quality, that reaches to the Innermost parts of the Object, as if a piece of Sealing-wax be broken into never so many pieces, the Internal fragments will be as Red as the External surface did appear, yet that is but a Particular Example that will not overthrow the Reason lately offer'd, especially since I can alleage other Examples of a contrary Import, and two or three Negative Instances are sufficient to overthrow the Generality of a Positive Rule, especially if that be built but upon One or a Few Examples. Not (then) to mention Cherries, Plums, and I know not how many other Bodies, wherein the skin is of one Colour, and what it hides of another, I shall name a couple of Instances drawn from the Colours / of Durable bodies that are thought far more Homogeneous, and have not parts that are either Organical, or of a Nature approaching thereunto.

3 To give you the first Instance, I shall need but to remind you of what I told you a little after the beginning of this Essay, touching the Blew and Red and Yellow, that may be produc'd upon a piece of temper'd Steel, for these Colours though they be very Vivid, yet if you break the Steel they adorn, they will appear to be but Superficial; not only the innermost parts of the Metall, but those that are within a hairs breadth of the Superficies, having not any of these Colours, but retaining that of the Steel it self. Besides that, we may as well confirm this Observation, as some other particulars we elsewhere deliver concerning Colours; by the following Experiment which we purposely made.

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4. We took a good quantity of clean Lead, and melted it with a strong Fire, and then immediately pouring it out into a clean Vessel of a convenient shape and matter, (we us'd one of Iron, that the great and sudden Heat might not injure it) and then carefully and nimbly taking off the Scum that floated on the top, we perceiv'd, as we expected, the smooth and / glossie Surface of the melted matter, to be adorn'd with a very glorious Colour, which being as Transitory as Delightfull, did almost immediately give place to another vivid Colour, and that was as quickly succeeded by a third, and this as it were chas'd away by a fourth, and so these wonderfully vivid Colours successively appear'd and vanish'd, (yet the same now and then appearing the second time) till the Metall ceasing to be hot enough to afford any longer this pleasing Spectacle, the Colours that

chanc'd to adorn the Surface, when the Lead thus began to cool, remain'd upon it; but were so Superficial, that how little soever we scrap'd off the Surface of the Lead, we did in such places scrape off all the Colour, and discover only that which is natural to the Metall it self, which receiving its adventitious Colours, only when the heat was very Intense, and in that part which was expos'd to the comparatively very cold Air, (which by other Experiments seems to abound with subtil Saline parts, perhaps not uncapable of working upon Lead so dispos'd:) These things I say, together with my observing that whatever parts of the so strongly melted Lead were expos'd a while to the Air, turn'd into a kind of Scum or Litharge, / how bright and clean soever they appear'd before, suggested to me some Thoughts or Ravings, which I have not now time to acquaint You with. One that did not know me, *Pyrophilus*, would perchance think I endeavour'd to impose upon You by relating this Experiment, which I have several times try'd, but the Reason why the *Phænomena* mention'd have not been taken notice of, may be, that unless Lead be brought to a much higher degree of Fusion or Fluidity than is usual, or than is indeed requisite to make it melt, the *Phænomena* I mention'd will scarce at all disclose themselves; And we have also observ'd that this successive appearing and vanishing of vivid Colours, was wont to be impair'd or determin'd whilst the Metal expos'd to the Air remain'd yet hotter than one would readily suspect. And one thing I must further Note, of which I leave You to search after the Reason, namely, that the same Colours did not always and regularly succeed one another, as is usually in Steel, but in the diversify'd Order mention'd in this following Note, which I was scarce able to write down, the succession of the Colours was so very quick, whether that proceeded from the differing degrees of Heat in the Lead expos'd to the cool Air, or from some / other Reason, I leave you to examine.

[*Blew, Yellow, Purple, Blew; Green, Purple, Blew, Yellow, Red; Purple, Blew, Yellow and Blew, Yellow, Blew, Purple, Green mixt, Yellow, Red, Blew, Green, Yellow, Red, Purple, Green.*]<sup>†</sup>

5. The *Atomists* of Old, and some Learned men of late, have attempted to explicate the variety of Colours in Opacous bodies from the various Figures of their Superficial parts; the attempt is Ingenious, and the Doctrine

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seems partly True, but I confess I think there are divers other things that must be taken in as concurrent to produce those differing forms of Asperity, whereon the Colours of Opacous bodies seem to depend. To declare this a little, we must assume, that the Surfaces of all such Bodies how Smooth or polite soever they may appear to our Dull Sight and Touch, are exactly smooth only in a popular, or at most in a Physical sense, but not in a strict and rigid sense.

6. This, excellent *Microscopes* shew us in many Bodies, that seem Smooth to our naked Eyes; and this not only as to the little Hillocks or Protuberancies that swell / above that which may be conceiv'd to be the Plain or Level of the consider'd Surface, for it is obvious enough to those that are any thing conversant with such Glasses, but as to numerous Depressions beneath that Level, of which sort of Cavities by the help of a *Microscope*, which the greatest Artificer that makes them, judges to be the greatest Magnifying Glass in *Europe*, except one that equals it, we have on the Surface of a thin piece of Cork that appear'd smooth to the Eye, observ'd about sixty in a Row, within the length of less then an 31 and 32 part of an Inch, (for the Glass takes in no longer a space at one view) and these Cavities (which made that little piece of Cork look almost like an empty Honey-comb) were not only very distinct, and figur'd like one another, but of a considerable bigness, and a scarce credible depth; insomuch that their distinct shadows as well as sides were plainly discern'd and easily to be reckon'd, and might have been well distinguish'd, though they had been ten times lesser than they were; which I thought it not amiss to mention to you *Pyrophilus* upon the by, that you may thence make some Estimate, what a strange Inequality, and what a multitude of little Shades, there may really be, in a / scarce sensible part of the Physical superficies, though the naked Eye sees no such matter. And as Excellent *Microscopes* shew us this Ruggedness in many Bodies that pass for Smooth, so there are divers Experiments, though we must not now stay to urge them,

which seem to perswade us of the same thing as to the rest of such Bodies as we are now treating off; So, that there is no sensible part of an Opacous body, that may not be conceiv'd to be made up of a multitude of singly insensible Corpuscles, but in the giving these Surfaces that disposition, which makes them alter the Light that reflects thence to the Eye after the manner requisite to make the Object appear Green, Blew, &c. the Figures of these Particles have *a great*, but not *the only* stroak. 'Tis true indeed that the protuberant Particles may be of very great variety of Figures, Sphærical, Elliptical, Conical, Cylindrical, Polyedrical, and some very irregular, and that according to the Nature of these, and the situation of the Lucid body, the Light must be variously affected, after one manner from Surfaces (I now speak of Physical Surfaces) consisting of Sphærical, and in another from those that are made up of Conical or Cylindrical Corpuscles; some / being fitted to reflect more of the incident Beams of Light, others less, and some towards one part, others towards another. But besides this difference of Shape, there may be divers other things that may eminently concur to vary the forms of Asperity that

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Colours so much depend on. For, willingly allowing the Figure of the Particles in the first place, I consider secondly, that the superficial Corpuscles, if I may so call them, may be bigger in one Body, and less in another, and consequently fitted to allay the Light falling on them with greater shades. Next, the protuberant Particles may be set more or less close together, that is, there may be a greater or a smaller number of them within the compass of one, than within the compass of another small part of the Surface of the same Extent, and how much these Qualities may serve to produce Colour may be somewhat guess'd at, by that which happens in the Agitation of Water; for if the Bubbles that are thereby made be Great, and but Few, the Water will scarce acquire a sensible Colour, but if it be reduc'd to a Froth, consisting of Bubbles, which being very Minute and Contiguous to each other, are a multitude of them crowded into a narrow Room, the Water (turned to / Froth) does then exhibit a very manifest White Colour, (to which these last nam'd Conditions of the Bubbles do as well as their Convex figure contribute) and that for Reasons to be mention'd anon.<sup>†@</sup> Besides, it is not necessary that the Superficial particles that exhibit one Colour, should be all of them Round, or all Conical, or all of any one Shape, but Corpuscles of differing Figures may be mingled on the Surface of the Opacous Body, as when the Corpuscles that make a Blew colour, and those that make a Yellow, come to be Accurately and Skilfully mix'd, they make up a Green, which though it seem one simple Colour, yet in this case appears to be made by Corpuscles of very differing Kinds, duely commix'd. Moreover the Figure and Bigness of the little Depressions, Cavities, Furrows or Pores intercepted betwixt these protuberant Corpuscles, are as well to be consider'd as the Sizes and Shapes of the Corpuscles themselves: For we may conceive the Physical superficies of a Body, where (as we said) its Colour does as it were reside, to be cut Transversly by a Mathematical plain, which you know is conceiv'd to be without any Depth of Thickness at all, and then as / some parts of the Physical superficies will be Protuberant, or swell above this last plain, so others may be depress'd beneath it, as (to explaine my self by a gross Comparison) in divers places of the Surface of the Earth, there are not only Neighbouring Hills, Trees, &c. that are rais'd above the Horizontal Level of the Valley, but Rivers, Wells, Pits and other Cavities that are depress'd beneath it; and that such Protuberant and Concave parts of a Surface may remit the Light so differingly, as much to vary a Colour, some examples and other things, that we shall hereafter have occasion to take notice off in this Tract, will sufficiently declare, till when, it may suffice to put you in mind, that of two Flat-sides of the same piece of, for example, red Marble, the one being diligently Polished, and the other left to its former Roughness, the differing degrees or sorts of Asperity, for the side that is smooth to the Touch wants not its Roughness, will so diversifie the Light reflected from the several Plains to the Eye, that a Painter would employ two differing Colours to represent them.

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7. And I hope, *Pyrophilus*, you will not think it strange or impertinent, that I employ in divers passages of these Papers, / examples drawn from Bodies and Shadows far more Gross, than those

minute Protuberances and shady Pores on which in most cases the Colour of a Body as 'tis an Inherent Quality or Disposition of its Surface, seems to depend. For sometimes I employ such Examples, rather to declare my Meaning, than prove my Conjecture; things, whom their Smallness makes Insensible, being better represented to the Imagination by such familiar Objects, as being like them enough in other respects, are of a Visible bulk. And next, though the Beams of Light are such subtil Bodies, that in respect of them, even Surfaces that are sensibly Smooth, are not exactly so, but have their own degree of Roughness, consisting of little Protuberances and Depressions; and though consequently such Inequalities may suffice to give Bodies differing Colours, as we see in Marble that appears White or Black, or Red or Blew, even when the most carefully Polish'd, yet 'tis plain by the late Instance of Red Marble, and many others, that even bigger Protuberances and greater Shades may likewise so Diversifie the Roughness of a Bodies Superficies, as manifestly to concur to the varying of its Colour, whereby such Examples appear to be proper enough / to be employ'd in such a Subject as we have now in hand. And having hinted thus much on this Occasion, I now proceed.

8. The Situation also of the Superficial particles is considerable, which I distinguish into the Posture of the single Corpuscles, in respect of the Light, and of the Eye, and the Order of them in reference also to one another; for a Body may otherwise reflect the Light, when its Superficial particles are more erected upon the Plain that may be conceiv'd to pass along their Basis, and when the Points or Extremes of such Particles are Obverted to the Eye, than when those Particles are so Incl'in'd, that their Sides are in great part Discernable, as the Colour of Plush or Velvet will appear Vary'd to you, if you carefully stroak part of it one way, and part of it another, the posture of the particular Thrids, in reference to the Light, or the Eye, becoming thereby different. And you may observe in a Field of ripe Corn blown upon by the Wind, that there will appear as it were Waves of a Colour (at least Gradually) differing from that of the rest of the Field, the Wind by Depressing some of the Ears, and not at the same time others, making the one Reflect more from / the Lateral and Strawy parts, than do the rest. And so, when Doggs are so angry, as to Erect the Hairs upon their Necks, and upon some other parts of their Bodies, those Parts seem to acquire a Colour vary'd from that which the same Hairs made, when in their usual Posture they did farr more stoop. And that the Order wherein the Superficial Corpuscles are Rang'd is not to be neglected, we may guess by turning of Water into Froth, the beating of Glass, and the scraping of Horns, in which cases the Corpuscles that were before so marshall'd as to be Perspicuous, do by the troubling of that Order become Dispos'd to terminate and reflect more Light, and thereby to appear Whitish. And there are other ways in which the Order of the Protuberant parts, in reference to the Eye, may much contribute to the appearing of a

particular Colour, for I have often observ'd, that when Pease are Planted, or Set in Parallel Lines, and are Shot up about half a Foot above the Surface of the Ground, by looking on the field or Plot of Ground from that part towards which the Parallel Lines tended, the greater part of the Ground by farr would appear of its own dirty Colour, but if I look'd upon it Transversly, the Plot / would appear very Green, the upper parts of the Pease hindering the intercepted parts of the Ground, which as I said retain'd their wonted Colour, from being discover'd by the Eye. And I know not, *Pyrophilus*, whether I might not add, that even the Motion of the Small Parts of a Visible Object may in some cases contribute, though it be not so easie to say how, to the Producing or the Varying of a Colour; for I have several times made a Liquor, which when it has well settled in a close Vial, is Transparent and Colourless, but as soon as the Glass is unstopp'd, begins to fly away very plentifully in a White and Opacous fume; and there are other Bodies, whose Fumes, when they fill a Receiver, would make one suspect it contains Milk, and yet when these Fumes settle into a Liquor, that Liquor is not White, but Transparent; And such White Fumes I have seen afforded by unstopping a Liquor I know, which yet is it self Diaphanous and Red; Nor are these the only Instances of this Kind, that our Tryals can supply us with. And if the Superficial Corpuscles be of the Grosser sort, and be so Framed, that their differing Sides or Faces may exhibit differing Colours, then the Motion or Rest

of those Corpuſcles may be / conſiderable, as to the Colour of the Superficies they compoſe, upon this account, that ſometimes more, ſometimes fewer of the Sides diſpos'd to exhibit ſuch a Colour may by this means become or continue more Obverted to the Eye than the reſt, and compoſe a Physical Surface, that will be more or leſs ſenſibly interrupted; As, to explaine my meaning, by propoſing a groſs Example, I remember, that in ſome ſorts of Leavy Plants thick ſet by one another, the two ſides of whoſe Leaves were of ſomewhat differing Colours, there would be a notable Diſparity as to Colour, if you look'd upon them both when the Leaves being at Reſt had their upper and commonly expos'd ſides Obverted to the Eye, and when a breath of Wind paſſing thorow them, made great Numbers of the uſually Hidden ſides of the Leaves become conſpicious. And though the Little Bodies, we were lately ſpeaking of, may Singly and Apart ſeem almoſt Colourleſs, yet when Many of them are plac'd by one another, ſo near, that the Eye does not eaſily diſcern an Interruption, within a ſenſible ſpace, they may exhibit a Colour; as we ſee, that though a Slenderest Thrid of Dy'd Silk do's, whiſt look'd on Single, ſeem almoſt quite Devoyd of Redneſs, (for Inſtance) / yet when numbers of theſe Thrids are brought together into one Skein, their Colour becomes notorious.

9. But the ſame Occaſion that invited me to ſay what I have mention'd concerning the Leaves of Trees, invites me alſo to give you ſome account of what happens in Changeable Taffities, where we ſee differing Colours, as it were, Emerge and Vanish upon the Ruffling of the ſame piece of Silk:  
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have divers times with Pleaſure obſerv'd, by the help of ſuch a *Microscope*, as, though it do not very much Magnifie the Object, has in recompence this great Conveniency, that you may eaſily, as faſt as you pleaſe, remove it from one part to another of a Large Object, of which the Glass taking a great part at once, you may thereby preſently Survey the Whole. Now by the help of ſuch a *Microscope* I could eaſily (as I began to ſay) diſcern, that in a piece of Changeable Taffity, (that appear'd, for Inſtance, ſometimes Red, and ſometimes Green) the Stuff was compos'd of Red thrids and Green, paſſing under and over each other, and crossing one another in almoſt innumerable points; and if I look'd through the Glass upon any conſiderable portion of the Stuff, that (for example ſake) to the / naked Eye appear'd to be Red, I could plainly ſee, that in that Poſition, the Red thrids were Conſpicious, and reflected a vivid Light; and though I could alſo perceive, that there were Green ones, yet by reaſon of their diſadvantagious Poſition in the *Physical Surface* of the Taffity, they were in part hid by the more Protuberant Thrids of the other Colour; and for the ſame cauſe, the Reflection from as much of the Green as was diſcover'd, was comparatively but Dim and Faint. And if, on the contrary, I look'd through the *Microscope* upon any part that appear'd Green, I could plainly ſee that the Red thrids were leſs fully expos'd to the eye, and obſcur'd by the Green ones, which therefore made up the Predominant Colour. And by obſerving the Texture of the Silken Stuff, I could eaſily ſo expoſe the Thrids either of the one Colour or of the other to my Eye, as at pleaſure to exhibit an apparition of, Red or Green, or make thoſe Colours ſucceed one another: So that, when I obſerv'd their Succeſſion by the help of the Glass, I could mark how the Predominant Colour did as it were ſtart out, when the Thrids that exhibited it came to be advantageouſly plac'd; And by making little Folds in the Stuff after a certain manner, / the Sides that met and terminated in thoſe Folds, would appear to the naked Eye, one of them Red, and the other Green. When Thrids of more than two differing Colours chance to be Interwoven, the reſulting changeableneſs of the Taffity may be alſo ſomewhat different. But I chooſe to give an Inſtance in the Stuff I have been ſpeaking off; becauſe the mixture being more Simple, the way whereby the Changeableneſs is produc'd, may be the more eaſily apprehended: and though Reaſon alone might readily enough lead a conſidering Man to gueſs at the Explication, in caſe he knew how Changeable Taffities are made: yet I thought it not impertinent to mention it, becauſe both Scholars and Gentlemen are wont to look upon the Inquiry into Manufactures, as a *Mechanick* imployment, and conſequently below Them; and becauſe alſo with ſuch a *Microscope* as I have been mentioning, the diſcovery is as well Pleaſant as Satisfactory, and may afford Hints of the Solution



of other *Phænomena* of Colours. And it were not amiss, that some diligent Inquiry were made, whether the *Microscope* would give us an account of the Variableness of Colour, that is so Conspicuous and so Delightfull in Mother of Pearl, in Opalls, and some / other resembling Bodies: For though I remember I did formerly attempt something of that Kind (fruitlessly enough) upon Mother of

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Pearl, yet not having then the advantage of my best *Microscope*, nor some Conveniences that might have been wish'd, I leave it to you, who have better Eyes, to try what you can do further; since 'twill be *Some* discovery to find, that, in this case, the best Eyes and *Microscopes* themselves can make *None*.

10. I confess, *Pyrophilus*, that a great part of what I have deliver'd, (or propos'd rather) concerning the differing forms of Asperity in Bodies, by which Differences the incident Light either comes to be Reflected with more or less of Shade, and with that Shade more or less Interrupted, or else happens to be also otherwise Modify'd or Troubl'd, is but Conjectural. But I am not sure, that if it were not for the Dulness of our Senses, either these or some other Notions of Kin to them, might be better Countenanc'd; for I am apt to suspect, that if we were Sharp sighted enough, or had such perfect *Microscopes*, as I fear are more to be wish'd than hop'd for, our promoted Sense might discern in the Physical Surfaces of Bodies, both a great many latent Ruggidnesses, and the particular / Sizes, Shapes, and Situations of the extremely little Bodies that cause them, and perhaps might perceive among other Varieties that we now can but imagine, how those little Protuberances and Cavities do Interrupt and Dilate the Light, by mingling with it a multitude of little and singly undiscernable Shades, though some of them more, and some of them less Minute, some less, and some more Numerous; according to the Nature and Degree of the particular Colour we attribute to the Visible Object; as we see, that in the Moon we can with Excellent *Telescopes* discern many Hills and Vallies, and as it were Pits and other Parts, whereof some are more, and some less Vividly illustrated, and others have a fainter, others a deeper Shade, though the naked Eye can discern no such matter in that Planet. And with an Excellent *Microscope*, where the *Naked* Eye did see but a Green powder, the *Assisted* Eye as we noted above, could discern particular Granules, some of them of a Blew, and some of them of a Yellow colour, which Corpuscles we had beforehand caus'd to be exquisitly mix'd to compound the Green.

11. And, *Pyrophilus*, that you may not think me altogether extravagant in what I / have said of the Possibility, (for I speak of no more) of discerning the differing forms of Asperity in the Surfaces of Bodies of several Colours, I'll here set down a Memorable particular that chanc'd to come to my Knowledge, since I writ a good part of this *Essay*; and it is this. Meeting casually the other Day with the deservedly Famous Dr. *J. Finch*, Extraordinary *Anatomist* to that Great Patron of the *Virtuosi*, the now Great Duke of *Toscany*,<sup>†@</sup> and enquiring of this Ingenious Person, what might be the chief Rarity he had seen in his late return out of *Italy* into *England*, he told me, it was a Man at *Maestricht* in the Low-Countrys, who at certain times can *distinguish Colours*

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by the Touch with his Fingers.<sup>†\*</sup> You'll easily Conclude, that this is farr more strange, than what I propos'd but as *not Impossible*; since the Sense of the *Retina* seeming to be much more Tender and quick than that of those Grosser Filaments, Nerves or Membranes of our Fingers, wherewith we use to handle Gross and Hard Bodies, it seems scarce credible, that any Accustomance, or Diet, or peculiarity of Constitution, should enable a Man to distinguish / with such Gross and Unsuitable Organs, such Nice and Subtile Differences as those of the forms of Asperity, that belong to differing Colours, to receive whose Languid and Delicate Impressions by the Intervention of Light, Nature seems to have appointed and contexed into the *Retina* the tender and delicate Pith of the Optick Nerve. Wherefore I confess, I propos'd divers Scruples, and particularly whether the Doctor had taken care to bind a Napkin or Hankerchief over his Eyes so carefully, as to be sure he could make

no use of his Sight, though he had but Counterfeited the want of it, to which I added divers other Questions, to satisfy my Self, whether there were any Likelihood of Collusion or other Tricks. But I found that the Judicious Doctor having gone farr out of his way, purposely to satisfy Himself and his Learned Prince about this Wonder, had been very Watchfull and Circumspect to keep *Himself* from being Impos'd upon. And that he might not through any mistake in point of Memory mis-inform *Me*, he did me the Favour at my Request, to look out the Notes he had Written for his Own and his Princes Information, the summ of which Memorials, as far as we shall mention them here, was this, That the Doctor / having been inform'd at *Utrecht*, that there Lived one at some Miles distance from *Maestricht*, who could distinguish Colours by the Touch, when he came to the last nam'd Town, he sent a Messenger for him, and having Examin'd him, was told upon Enquiry these Particulars:

That the Man's name was *John Vermaasen*, at that time about 33 Years of Age; that when he was but two years Old, he had the Small Pox, which rendred him absolutely Blind: That at this present he is an *Organist*, and serves that Office in a publick Quire.

That the Doctor discoursing with him over Night, the Blind man affirm'd, that he could distinguish Colours by the Touch, but that he could not do it, unless he were Fasting; Any quantity of Drink taking from him that Exquisitness of Touch, which is requisite to so Nice a Sensation.

That hereupon the Doctor provided against the next Morning seven pieces of Ribbon, of these seven Colours, Black, White, Red, Blew, Green, Yellow, and Gray, but as for *mingled* Colours, this *Vermaasen* would not undertake to discern them, though if offer'd, he would tell that they were *Mixed*.

That to discern the Colour of the Ribbon, / he places it betwixt the

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Thumb and the Fore-finger, but his most exquisite perception was in his Thumb, and much better in the right Thumb than in the left.

That after the Blind man had four or five times told the Doctor the several Colours, (though Blinded with a Napkin for fear he might have some Sight) the Doctor found he was twice mistaken, for he call'd the White Black, and the Red Blew, but still, he, before his Errour, would lay them by in Pairs, saying, that though he could easily distinguish them from all others, yet those two Pairs were not easily distinguish'd amongst themselves, whereupon the Doctor desir'd to be told by him what kind of Discrimination he had of Colours by his Touch, to which he gave a reply, for whose sake chiefly I insert all this Narrative in this place, namely, That all the difference was more or less Asperity, for says he, (I give you the Doctor's own words) Black feels as if you were feeling Needles points, or some harsh Sand, and Red feels very Smooth.

That the Doctor having desir'd him to tell in Order the difference of Colours to his Touch, he did as follows;

Black and White are the most asperous / or unequal of all Colours, and so like, that 'tis very hard to distinguish them, but Black is the most Rough of the two, Green is next in Asperity, Gray next to Green in Asperity, Yellow is the fifth in degree of Asperity, Red and Blew are so like, that they are as hard to distinguish as Black and White, but Red is somewhat more Asperous than Blew, so that Red has the sixth place, and Blew the seventh in Asperity.

12. To these Informations the Obliging Doctor was pleas'd to add the welcome present of three of those very pieces of Ribbon, whose Colours in his presence the Blind man had distinguished, pronouncing the one Gray, the other Red, and the third Green, which I keep by me as Rarities, and the rather, because he fear'd the rest were miscarry'd.

13. Before I saw the Notes that afforded me the precedent Narrative, I confess I suspected this man might have thus discriminated Colours; rather by the Smell than by the Touch; for some of the Ingredients imployed by Dyers to Colour things, have Sents, that are not so Languid, nor so near of Kin, but that I thought it not impossible that a very Critical Nose might distinguish them, and this I the rather suspected, because he requir'd, that the Ribbons, / whose Colours he was to Name, should be offer'd him Fasting in the morning; for I have observ'd in Setting Doggs, that the feeding of them (especially with some sorts of Aliments) does very much impair the exquisite sent of their Noses. And though some of the foregoing particulars would have prevented that Conjecture, yet I confess to you (*Pyrophilus*) that I would gladly have had the Opportunity of Examining this Man my self, and of Questioning him about divers particulars which I do not find to have been yet thought upon. And though it be not incredible to me, that since the Liquors that Dyers imploy to tinge, are qualifi'd to do so by multitudes of little Corpuscles of the Pigment or Dying stuff, which are dissolved and extracted by the Liquor, and swim to and fro in it, those Corpuscles of Colour (as the *Atomists* call them) insinuating themselves into, and filling all

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the Pores of the Body to be Dyed, may Asperate its Superficies more or less according to the Bigness and Texture of the Corpuscles of the Pigment; yet I can scarce believe, that our Blind man could distinguish all the Colours he did, meerly by the Ribbons having more or less of Asperity, so that I cannot but think, notwithstanding this History, that the Blind man / distinguish'd Colours not only by the *Degrees* of Asperity in the Bodies offer'd to him, but by *Forms* of it, though this (latter) would perhaps have been very difficult for him to make in Intelligible mention of, because those Minute disparities having not been taken notice of by men for want of touch as Exquisite as our Blind Mans, are things he could not have Intelligibly express'd, which will easily seem Probable, if you consider, that under the name of Sharp, and Sweet, and Sour, there are abundance of, as it were, immediate peculiar Relishes or Tasts in differing sorts of Wine, which though Critical and Experienc'd Palats can easily discern themselves cannot make them be understood by others, such Minute differences not having hitherto any Distinct names assign'd them. And it seems that there was something in the Forms of Asperity that was requisite to the Distinction of Colours, besides the Degree of it, since he found it so difficult to distinguish Black and White from one another, though not from other Colours. For I might urge, that he seems not consonant to himself about the *Red*, which as you have seen in one place, he represents as somewhat more Asperous than the *Blew*; and in another, very Smooth: But because he speaks of this Smoothness in that place, / where he mentions the Roughness of *Black*, we may favourably presume that he might mean but a *comparative Smoothness*; and therefore I shall not Insist on this, but rather Countenance my Conjecture by this, that he found it so Difficult, not only, to Discriminate Red and Blew, (though the first of our promiscuous Experiments will inform you, that the Red reflects by great Odds more Light than the other) but also to distinguish Black and White from one another, though not from other Colours. And indeed, though in the Ribbons that were offer'd him, they might be almost equally Rough, yet in such slender Corpuscles as those of Colour, there may easily enough be Conceiv'd, not only a greater Closeness of Parts, or else Paucity of Protuberant Corpuscles, and the little extant Particles may be otherwise Figur'd, and Rang'd in the White than in the Black, but the Cavities may be much Deeper in the one than the other.

14. And perhaps, (*Pyrophilus*) it may prove some *Illustration of what I mean*, and help you to conceive how *this may* be, if I Represent, that where the Particles are so exceeding Slender, we may allow the Parts expos'd to the Sight and Touch to be a little Convex in comparison of the Erected / Particles of Black Bodies, as if there were Wyres I know not how many times Slenderer than a Hair: whether you suppose them to be Figur'd like Needles, or Cylindrically, like the Hairs of a Brush, with Hemisphærical (or at least Convex) Tops, they will be so very Slender, and consequently the Points both of the one sort and the other so very Sharp, that even an exquisite Touch will be able to distinguish no greater Difference between them, than that which our

Blind man allow'd, when comparing Black and White Bodies, he said, that the latter was the less Rough of the two. Nor is every Kind of Roughness, though Sensible enough, Inconsistent with Whiteness, there being Cases, wherein the Physical Superficies of a Body is made by the same Operation both *Rough* and *White*, as when the Level Surface of clear Water being by agitation Asperated with a multitude of Unequal Bubbles, do's thereby acquire a Whiteness; and as a Smooth piece of Glass, by being Scratch'd with a Diamond, do's in the Asperated part of its Surface disclose the same Colour. But more (perchance) of this elsewhere.

15. And therefore, we shall here pass by the Question, whether any thing might / be consider'd about the Opacity of the Corpuscles of Black Pigments, and the *Comparative* Diaphanëity of those of many White Bodies, apply'd to our present Case; and proceed, to represent, That the newly mention'd Exiguity and Shape of the extant Particles being suppos'd, it will then be considerable what we lately but Hinted, (and therefore must now somewhat Explane) That the Depth of the little Cavities, intercepted between the extant Particles, without being so much greater in Black Bodies than in White ones, as to be perceptibly so to the Gross Organs of Touch, may be very much greater in reference to their Disposition of Reflecting the imaginary subtile Beams of Light. For in Black Bodies, those Little intercepted Cavities, and other Depressions, may be so Figur'd, so Narrow and so Deep, that the incident Beams of Light, which the more extant Parts of the Physical Superficies are dispos'd to Reflect inwards, may be Detain'd there, and prove unable to Emerge; whilst in a White Body, the Slender Particles may not only by their Figure be fitted to Reflect the Light copiously outwards, but the intercepted Cavities being not Deep, nor perhaps very Narrow, the Bottoms of them may be so Constituted, as to / be fit to Reflect outwards much of the Light that falls even upon Them; as you may possibly better apprehend, when we shall come to treat of Whiteness and Blackness. In the mean time it may suffice, that you take Notice with me, that the Blind mans Relations import no necessity of Concluding, that, though, because, according to the Judgment of his Touch, Black was the Roughest, as it is the Darkest of Colours, therefore White, which (according to us) is the Lightest, should be also the Smoothest: since I observe, that he makes Yellow to be two Degrees more Asperous than Blew, and as much less Asperous than Green; whereas indeed, Yellow do's not only appear to the Eye a Lighter Colour than Blew, but (by our first Experiment hereafter to be mention'd) it will appear, that Yellow reflected much more Light than Blew, and manifestly more than Green, (which we need not much wonder at, since in this Colour and the two others (Blew and Yellow) 'tis not *only* the *Reflected Light* that is to be considered, since to produce both these, *Refraction* seems to Intervene, which by its Varieties may much alter the Case;) which both seems to strengthen the Conjecture I was formerly proposing, that there was something else / in the *Kinds* of Asperity, as well as in the *Degrees* of it, which enabled our Blind man to Discriminate Colours, and do's at least show, that

we cannot in all Cases from the bare Difference in the Degrees of Asperity betwixt Colours, safely conclude, that the Rougher of any two always Reflects the least Light.

16. But this notwithstanding, (*Pyrophilus*) and what ever Curiosity I may have had to move some Questions to our Sagacious Blind man, yet thus much I think you will admit us to have gain'd by his Testimony, that since many Colours may be felt with the Circumstances above related, the Surfaces of such Coloured Bodies must certainly have differing *Degrees*, and in all probability have differing *Forms* or *Kinds* of Asperity belonging to them, which is all the Use that my present attempt obliges me to make of the History above deliver'd, that being sufficient to prove, *that* Colour do's much depend upon the Disposition of the Superficial parts of Bodies, and to shew in general, *wherein* 'tis probable that such a Disposition do's (principally at least) consist.

17. But to return to what I was saying before I began to make mention of our Blind *Organist*, what

we have deliver'd / touching the causes of the several Forms of Asperity that may Diversifie the Surfaces of Colour'd Bodies, may perchance somewhat assist us to make some Conjectures in the general, at several of the ways whereby 'tis possible for the Experiments hereafter to be mention'd, to produce the suddain changes of Colours that are wont to be Consequent upon them; for most of these *Phænomena* being produc'd by the Intervention of Liquors, and these for the most part abounding with very Minute, Active, and Variously Figur'd Saline Corpuscles, Liquors so Qualify'd may well enough very Nimble alter the Texture of the Body they are imploy'd to Work upon, and so may change the form of Asperity, and thereby make them Remit to the Eye the Light that falls on them, after another manner than they did before, and by that means Vary the Colour, so farr forth as it depends upon the Texture or Disposition of the Seen Parts of the Object, which I say, *Pyrophilus*, that you may not think I would absolutely exclude all other ways of Modifying the Beams of Light between their Parting from the Lucid Body, and their Reception into the common Sensory.

18. Now there seem to me divers ways, / by which we may conceive that Liquors may Nimble alter the Colour of one another, and of other Bodies, upon which they Act, but my present haste will allow me to mention but some of them, without Insisting so much as upon those I shall name.

19. And first, the Minute Corpuscles that compose a Liquor may easily insinuate themselves into those Pores of Bodies, whereto their Size and Figure makes them Congruous, and these Pores they may either exactly Fill, or but Inadequately, and in this latter Case they will for the most part alter the Number and Figure, and always the Bigness of the former Pores. And in what capacity soever these Corpuscles of a Liquor come to be Lodg'd or Harbour'd in the Pores that admit them, the Surface of the Body will for the most part have its Asperity alter'd, and the Incident Light that meets with a Grosser Liquor in the little Cavities that before contain'd nothing but Air, or some yet Subtiler Fluid, will have its Beams either Refracted, or Imbib'd, or

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else Reflected more or less Interruptedly, than they would be, if the Body had been Unmoistned, as we see, that even fair Water falling on white Paper, or Linnen, and divers other Bodies / apt to soak it in, will for some such Reasons as those newly mention'd, immediately alter the Colour of them, and for the most part make it Sadder than that of the Unwetted Parts of the same Bodies. And so you may see, that when in the Summer the High-ways are Dry and Dusty, if there falls store of Rain, they will quickly appear of a much Darker Colour than they did before, and if a Drop of Oyl be let fall upon a Sheet of White Paper, that part of it, which by the Imbibition of the Liquor acquires a greater Continuity, and some Transparency, will appear much Darker than the rest, many of the Incident Beams of Light being now Transmitted, that otherwise would be Reflected towards the Beholders Eyes.

20. Secondly, A Liquor may alter the Colour of a Body by freeing it from those things that hindred it from appearing in its Genuine Colour; and though this may be said to be rather a Restauration of a Body to its own Colour, or a Retection of its native Colour, than a Change, yet still there Intervenes in it a change of the Colour which the Body appear'd to be of before this Operation. And such a change a Liquor may work, either by Dissolving, or Corroding, or by some such way of / carrying off that Matter, which either Veil'd or Disguis'd the Colour that afterwards appears. Thus we restore Old pieces of Dirty Gold to a clean and nitid Yellow, by putting them into the Fire, and into *Aqua-fortis*, which take off the adventitious Filth that made that pure Metall look of a Dirty Colour. And there is also an easie way to restore Silver Coyns to their due Lustre, by fetching off that which Discolour'd them. And I know a *Chymical* Liquor, which I employ'd to restore pieces of Cloath spotted with Grease to their proper Colour, by Imbibing the Spotted part with this Liquor, which Incorporating with the Grease, and yet being of a very Volatile Nature, does easily carry it away with it Self. And I have sometimes try'd, that by Rubbing upon a good Touch-stone a certain *Metalline* mixture so Compounded, that the Impression it left upon the Stone appear'd of a very differing Colour from that of Gold, yet a little of *Aqua-fortis* would in a Trice make the Golden



Colour disclose it self, by Dissolving the other *Metalline* Corpuscles that conceal'd those of the Gold, which you know that *Menstruum* will leave Untouch'd.

21. Thirdly, A Liquor may alter the / Colour of a Body by making a Comminution of its Parts, and that principally two ways, the first by Disjoyning and Dissipating those Clusters of Particles, if I may so call them, which stuck more Loosely together, being fastned only by some more easily Dissoluble Ciment, which seems to be the Case of some of the following Experiments, where you'll find the Colour of many Corpuscles brought to cohere by having been Precipitated together, Destroy'd by the Affusion of very peircing and incisive Liquors. The other of the two ways I was speaking of, is, by Dividing the Grosser and more Solid Particles into Minute ones, which will be always Lesser, and for the most part otherwise Shap'd than the Entire Corpuscle so Divided, as it will happen in a piece of Wood reduc'd

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into Splinters or Chips, or as when a piece of Chrystal heated red Hot and quench'd in Cold water is crack'd into a multitude of little Fragments, which though they fall not asunder, alter the Disposition of the Body of the Crystal, as to its manner of Reflecting the Light, as we shall have Occasion to shew hereafter.

22. There is a fourth way contrary to the third, whereby a Liquor may change the Colour of another Body, especially of / another Fluid, and that is, by procuring the Coalition of several Particles that before lay too Scatter'd and Dispers'd to exhibit the Colour that afterwards appears. Thus sometimes when I have had a Solution of Gold so Dilated, that I doubted whether the Liquor had really Imbib'd any true Gold or no, by pouring in a little *Mercury*, I have been quickly able to satisfie my Self, that the Liquor contain'd Gold, that Mettall after a little while Cloathing the Surface of the *Quick-silver*, with a Thin Film of its own Livery. And chiefly, though not only by this way of bringing the Minute parts of Bodies together in such Numbers as to make them become Notorious to the Eye, many of these Colours seem to be Generated which are produc'd by Precipitations, especially by such as are wont to be made with fair Water, as when Resinous Gumms dissolv'd in Spirit of Wine, are let fall again, if the Spirit of Copiously diluted with that weakning Liquor. And so out of the Rectify'd and Transparent Butter of *Antimony*, by the bare Mixture of fair Water, there will be plentifully Precipitated that Milk-white Substance, which by having its Looser Salts well wash'd off, is turn'd into that Medicine, which Vulgar *Chymists* are pleas'd to call *Mercurius Vitæ*. /

23. A fifth way, by which a Liquor may change the Colour of a Body, is, by Dislocating the Parts, and putting them out of their former Order into another, and perhaps also altering the Posture of the single Corpuscles as well as their Order or Situation in respect of one another. What certain Kinds of Commotion or Dislocation of the Parts of a Body may do towards the Changing its Colour, is not only evident in the Mutations of Colour observable in *Quick-silver*, and some other Concretes long kept by *Chymists* in a Convenient Heat, though in close Vessels, but in the Obvious Degenerations of Colour, which every Body may take notice of in Bruis'd Cherries, and other Fruit, by comparing after a while the Colour of the Injur'd with that of the Sound part of the same Fruit. And that also such Liquors, as we have been speaking of, may greatly Discompose the Textures of many Bodies, and thereby alter the Disposition of their Superficial parts, the great Commotion made in Metalls, and several other Bodies by *Aqua-fortis*, Oyl of *Vitriol*, and other Saline *Menstruums*, may easily perswade us, and what such Vary'd Situations of Parts may do towards the Diversifying of the manner of their Reflecting the Light, may / be Guess'd in some Measure by the Beating of Transparent Glass into a White Powder, but farr better by the Experiments lately Pointed at, and hereafter Deliver'd, as the Producing and Destroying Colours by the means of subtil Saline Liquors, by whose Affusion the Parts of other Liquors are manifestly both Agitated, and likewise Dispos'd after

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another manner than they were before such Affusion. And in some *Chymical* Oyls, as particularly that of Lemmon Pills, by barely Shaking the Glass, that holds it, into Bubbles, that Transposition of the Parts which is consequent to the Shaking, will shew you on the Surfaces of the Bubbles exceeding Orient and Lively Colours, which when the Bubbles relapse into the rest of the Oyl, do immediately Vanish.

24. I know not, *Pyrophilus*, whether I should mention as a Distinct way, because it is of a somewhat more General Nature, that Power, whereby a Liquor may alter the Colour of another Body, by putting the Parts of it into Motion; For though possibly the Motion so produc'd, does, as such, seldome suddenly change the Colour of the Body whose Parts are Agitated, yet this seems to be one of the most General, however not Immediate causes of / the Quick change of Colours in Bodies. For the Parts being put into Motion by the adventitious Liquor, divers of them that were before United, may become thereby Disjoyn'd, and when that Motion ceases or decays, others of them may Stick together, and that in a new Order, by which means the Motion may sometimes produce Permanent changes of Colours, as in the Experiment you will meet with hereafter, of presently turning a Snowy White Body into a Yellow, by the bare Affusion of fair Water, which probably so Dissolves the Saline Corpuscles that remain'd in the *Calx*, and sets them at Liberty to Act upon one another, and the Metall, far more Powerfully than the Water without the Assistance of such Saline Corpuscles could do. And though you rubb Blew *Vitriol*, how Venereal and Unsophisticated soever it be, upon the Whetted Blade of a Knife, it will not impart to the Iron its Latent Colour, but if you moisten the *Vitriol* with your Spittle, or common Water, the Particles of the Liquor disjoyning those of the *Vitriol*, and thereby giving them the Various Agitation requisite to Fluid Bodies, the Metalline Corpuscles of the thus Dissolv'd *Vitriol* will Lodge themselves in Throngs in the Small and Congruous / Pores of the Iron they are Rubb'd on, and so give the Surface of it the Genuine Colour of the Copper.

25. There remains yet a way, *Pyrophilus*, to be mention'd, by which a Liquor may alter the Colour of another Body, and this seems the most Important of all, because though it be nam'd but as One, yet it may indeed comprehend Many, and that is, by Associating the Saline Corpuscles, or any other Sort of the more Rigid ones of the Liquor, with the Particles of the Body that it is employ'd to Work upon. For these Adventitious Corpuscles Associating themselves with the Protuberant Particles of the Surface of a Colour'd Body, must necessarily alter their Bigness, and will most commonly alter their Shape. And how much the Colours of Bodies depend upon the Bulk and Figure of their Superficial Particles, you may Guess by this, that eminent antient *Philosophers*, and divers *Moderns*, have thought that all Colours might in a general way be made out by these two;†\* whose being Diversify'd, will in our Case be attended with these two Circumstances, the

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One, that the Protuberant Particles being Increas'd in Bulk, they will oftentimes be Vary'd as to the Closeness or Laxity of / their Order, fewer of them being contain'd within the same Sensible (though Minute) space than before; or else by approaching to one another, they must Straighten the Pores, and it may be too, they will by their manner of Associating themselves with the Protuberant Particles, intercept new Pores. And this invites me to consider farther, that the Adventitious Corpuscles, I have been speaking of, may likewise produce a great Change as well in the Little Cavities or Pores as in the Protuberances of a Colour'd Body; for besides what we have just now taken notice of, they may by Lodging themselves in those little Cavities, fill them up, and it may well happen, that they may not only fill the Pores they Insinuate themselves into, but likewise have their Upper Parts extant above them; and partly by these new Protuberances, partly by Increasing the Bulk of the former, these Extraneous Corpuscles may much alter the Number and Bigness of the Surfaces Pores, changing the Old and Intercepting new ones. And then 'tis Odds, but the Order of the Little Extancies, and consequently that of the Little Depressions in point of Situation will be alter'd likewise: as if you dissolve *Quick-silver* in some kind of *Aqua-fortis*, / the Saline Particles of the *Menstruum*, Associating themselves with the Mercurial Corpuscles, will make a Green Solution,

which afterwards easily enough Degenerates. And Red Lead or *Minium* being Dissolv'd in Spirit of Vinegar, yields not a Red, but a Clear Solution, the Redness of the Lead being by the Liquor Destroy'd. But a better Instance may be taken from Copper, for I have try'd, that if upon a Copper-plate you let some Drops of weak *Aqua-fortis* rest for a while, the Corpuscles of the *Menstruum* joyning with those of the Metall, will produce a very sensible Asperity upon the Surface of the Plate, and will Concoagulate that way into very minute Grains of a Pale Blew *Vitriol*; whereas if upon another part of the same Plate you suffer a little strong Spirit of Urine to rest a competent time, you shall find the Asperated Surface adorn'd with a Deeper and Richer Blew. And the same *Aqua-fortis*, that will quickly change the Redness of Red Lead into a Darker Colour, will, being put upon Crude Lead, produce a Whitish Substance, as with Copper it did a Blewish. And as with Iron it will produce a Reddish, and on White Quills a Yellowish, so much may the Coalition of the Parts of the same / Liquor, with the differinglly Figur'd Particles of Stable Bodies, divers ways Asperate the differinglly Dispos'd Surfaces, and so Diversifie the Colour of those Bodies. And you'l easily believe, that in many changes of Colour, that happen upon the Dissolutions of Metalls, and Precipitations made with Oyl of *Tartar*, and the like Fix'd Salts, there may Intervene a Coalition of Saline Corpuscles with the Particles of the Body Dissolv'd or Precipitated, if you examine how much the *Vitriol* of a Metall may be Heavier than the Metalline part of it alone, upon the Score of the Saline parts Concoagulated therewith, and, that in several Precipitations the weight of the *Calx* does for the same Reason much exceed that of the Metall, when it was first put in to be Dissolv'd.

26. But, *Pyrophilus*, to consider these Matters more particularly would

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be to forget that I declar'd against Adventuring, at least for this time, at particular Theories of Colours, and that accordingly you may justly expect from me rather Experiments than Speculations, and therefore I shall Dismiss this Subject of the Forms of Superficial Asperity in Colour'd Bodies, as soon as I shall but have nam'd to you by way of Supplement to what we have / hitherto Discours'd in this Section, a Couple of Particulars, (which you'l easily grant me) The one, That there are divers other ways for the speedy Production even of True and Permanent Colours in Bodies, besides those Practicable by the help of Liquors; for proof of which Advertisement, though several Examples might be alleged, yet I shall need but Re-mind you of what I mention'd to you above, touching the change of Colours suddenly made on Temper'd Steel, and on Lead, by the Operation of Heat, without the Intervention of a Liquor. But the other particular I am to observe to you is of more Importance to our present Subject, and it is, That though Nature and Art may in some cases so change the Asperity of the Superficial parts of a Body, as to change its Colour by either of the ways I have propos'd Single or Unassisted, yet for the most part 'tis by two or three, or perhaps by more of the fore-mention'd ways Associated together, that the Effect is produc'd, and if you consider how Variouslly those several ways and some others Ally'd unto them, which I have left unmention'd, may be Compounded and Apply'd, you will not much wonder that such fruitfull, whether Principles (or Manners of Diversification) / should be fitted to Change or Generate no small store of Differing Colours.

27. Hitherto, *Pyrophilus*, we have in discoursing of the Asperity of Bodies consider'd the little Protuberances of other Superficial particles which make up that Roughness, as if we took it for granted, that they must be perfectly Opacous and Impenetrable by the Beams of Light, and so, must contribute to the Variety of Colours as they terminate more or less Light, and reflect it to the Eye mix'd with more or less of thus or thus mingl'd Shades. But to deal Ingenuously with you, *Pyrophilus*, before I proceed any further, I must not conceal from you, that I have often thought it worth a Serious Enquiry, whether or no Particles of Matter, each of them singly Insensible, and therefore Small enough to be capable of being such Minute Particles, as the *Atomists* both of old and of late have (not absurdly) called *Corpuscula Coloris*,<sup>†\*</sup> may not yet consist each of them of divers yet Minuter Particles, betwixt which we may conceive little Commissures where they Adhere to one another, and, however, may not be Porous enough to be, at least in some degree, Pervious to

the unimaginably subtile Corpuscles that make up the Beams of / Light, and consequently to be in such a degree Diaphanous. For, *Pyrophilus*, that the proposed Enquiry may be of moment to him that searches after the Nature of Colour, you'll easily grant, if you consider, that whereas Perfectly Opacous bodies can but reflect the incident Beams of Light, those that are Diaphanous are qualified to refract them too, and that

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Refraction has such a stroak in the Production of Colours, as you cannot but have taken notice of, and perhaps admir'd in the Colours generated by the Trajection of Light through Drops of Water that exhibit a Rain-bow, through Prismatical glasses, and through divers other Transparent bodies. But 'tis like, *Pyrophilus*, you'll more easily allow that about this matter 'tis rather Important to have a Certainty, than that 'tis Rational to entertain a Doubt; wherefore I must mention to you some of the Reasons that make me think it may need a further Enquiry, for I find that in a Darkned Room, where the Light is permitted to enter but at One hole the little wandering Particles of Dust, that are commonly called Motes, and, unless in the Sun-beams, are not taken notice of by the unassisted Sight, I have, I say, often observ'd, that these roving Corpuscles being look'd on by an Eye plac'd on one side of the / Beams that enter'd the Little hole, and by the Darkness having its Pupill much Enlarg'd, I could discern that these Motes as soon as they came within the compass of the Luminous, whether Cylinder or Inverted Cone, if I may so call it, that was made up by the Unclouded Beams of the Sun, did in certain positions appear adorn'd with very vivid Colours, like those of the Rain-bow, or rather like those of very Minute, but Sparkling fragments of Diamonds; and as soon as the Continuance of their Motion had brought them to an Inconvenient position in reference to the Light and the Eye, they were only visible without Darting any lively Colours as before, which seems to argue that these little Motes, or minute Fragments, of several sorts of bodies reputed Opacous, and only crumbled as to their Exteriour and Looser parts into Dust, did not barely Reflect the Beams that fell upon them, but remit them to the Eye Refracted too. We may also observe, that several Bodies, (as well some of a Vegetable, as others of an Animal nature) which are wont to pass for Opacous, appear in great part Transparent, when they are reduc'd into Thin parts, and held against a powerful Light. This I have not only taken notice of in pieces of Ivory reduc'd but into Thick Leaves, as also in divers considerable / Thick shells of Fishes, and in shaving of Wood, but I have also found that a piece of Deal, far thicker than one would easily imagine, being purposely interpos'd betwixt my Eye plac'd in a Room, and the clear Daylight, was not only somewhat Transparent, but (perhaps by reason of its Gummous nature) appear'd quite through of a lovely Red. And in the Darkned Room above mention'd, Bodies held against the hole at which the Light enter'd, appear'd far less Opacous than they would elsewhere have done, insomuch that I could easily and plainly see through the whole Thickness of my Hand, the Motions of a Body plac'd (at a very near distance indeed, but yet) beyond it. And even in Minerals, the Opacity is not always so great as many think, if the Body be made Thin, for White Marble though of a pretty Thickness, being within a Due distance plac'd betwixt the Eye and a Convenient Light, will suffer the Motions of ones Finger to be well discern'd through it, and so will pieces, Thick enough, of many common Flints. But above all, that Instance is remarkable, that is afforded us by *Muscovie* glass, (which some call *Selenites*, others *Lapis Specularis*) for though plates of this

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Mineral, though but of a moderate Thickness, do often appear Opacous, yet if / one of these be Dextrously split into the thinnest Leaves 'tis made up of, it will yield such a number of them, as scarce any thing but Experience could have perswaded me, and these Leaves will afford the most Transparent sort of consistent Bodies, that, for ought I have observ'd, are yet known; and a single Leaf or Plate will be so far from being Opacous, that 'twill scarce be so much as Visible. And multitudes of Bodies there are, whose Fragments seem Opacous to the naked Eye, which yet, when I have included them in good *Microscopes*, appear'd Transparent; but, *Pyrophilus*, on the other side I am not yet sure that there are no Bodies, whose Minute Particles even in such a *Microscope* as that



of mine, which I was lately mentioning, will not appear Diaphanous. For having consider'd *Mercury* Precipitated *per se*, the little Granules that made up the powder, look'd like little fragments of Coral beheld by the naked Eye at a Distance (for very Near at hand Coral will sometimes, especially if it be Good, shew some Transparency.) Filings likewise of Steel and Copper, though in an excellent *Microscope*, and a fair Day, they show'd like pretty Big Fragments of those Metalls, and had considerable Brightness on some of their Surfaces, yet I was not satisfi'd, that I perceiv'd / any Reflection from the Inner parts of any of the Filings. Nay, having look'd in my best *Microscope* upon the Red *Calx* of Lead, (commonly call'd *Minium*) neither I, nor any I shew'd it to, could discern it to be other than Opacous, though the Day were Clear, and the Object strongly Enlightned. And the deeply Red Colour of *Vitriol* appear'd in the same *Microscope* (notwithstanding the great Comminution effected by the Fire) but like Grossly beaten Brick. So that, *Pyrophilus*, I shall willingly resign you the care of making some further Enquiries into the Subject we have now been considering; for I confess, as I told you before, that I think that the Matter may need a further Scrutiny, nor would I be forward to Determine how far or in what cases the Transparency or Semi-diaphaniety of the Superficial Corpuscles of Bigger Bodies, may have an Interest in the Production of their Colours, especially because that even in divers White bodies, as Beaten Glass, Snow and Froth, where it seems manifest that the Superficial parts are singly Diaphanous, (being either Water, or Air, or Glass) we see not that such Variety of Colours are produc'd as usually are by the Refraction of Light, even in those Bodies, when by their Bigness, Shape, &c. they are conveniently / qualify'd to exhibit such Various and Lively Colours as those of the Rain-bow, and of Prismatical Glasses.

28. By what has been hitherto discours'd, *Pyrophilus*, we may be assisted to judge of that famous Controversie which was of Old disputed betwixt the *Epicureans* and other *Atomists* on the one side, and most other *Philosophers* on the other side. The former Denying Bodies to be Colour'd in the Dark, and the Latter making Colour to be an Inherent quality, as well as Figure, Hardness; Weight, or the like.†\* For though this Controversie be Reviv'd, and hotly

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Agitated among the *Moderns*, yet I doubt whether it be not in great part a Nominal dispute, and therefore let us, according to the Doctrine formerly deliver'd, Distinguish the Acceptions of the word Colour, and say, that if it be taken in the Stricter Sense, the *Epicureans* seem to be in the Right, for if Colour be indeed, though not according to them, but Light Modify'd, how can we conceive that it can Subsist in the Dark, that is, where it must be suppos'd there is no Light; but on the other side, if Colour be consider'd as a certain Constant Disposition of the Superficial parts of the Object to Trouble the Light they Reflect after such and such a Determinate manner, / this Constant, and, if I may so speak, Modifying disposition persevering in the Object, whether it be Shin'd upon or no, there seems no just reason to deny, but that in this Sense, Bodies retain their Colour as well in the Night as Day; or, to Speak a little otherwise, it may be said, that Bodies are Potentially Colour'd in the Dark, and Actually in the Light. But of this Matter discoursing more fully elsewhere, as 'tis a difficulty that concerns Qualities in general, I shall forbear to insist on it here.

#### Chap. IV.

1. Of greater Moment in the Investigation of the Nature of Colours is the Controversie, Whether those of the Rain-bow, and those that are often seen in Clouds, before the Rising, or after the Setting of the Sun; and in a word, Whether those other Colours, that are wont to be call'd Emphatical, ought or ought not to be accounted True Colours. I need not tell you that the Negative is the Common Opinion, especially in the Schools, as may appear by that Vulgar distinction of Colours, whereby these under Consideration are term'd Apparent, by way of Opposition / to those that in the other Member of the Distinction are call'd True or Genuine. This question I say seems to

me of Importance, upon this Account, that it being commonly Granted, (or however, easie enough to be Prov'd) that Emphatical Colours are Light it self Modify'd by Refractions chiefly, with a concurrence sometimes of Reflections, and perhaps some other Accidents depending on these two; if these Emphatical Colours be resolv'd to be Genuine, it will seem consequent, that Colours, or at least divers of them, are but Diversify'd Light, and not such Real and Inherent qualities as they are commonly thought to be.

2. Now since we are wont to esteem the Echoes and other Sounds of Bodies, to be True Sounds, all their Odours to be True Odours, and (to be short) since we Judge other Sensible qualities to be True ones, because they are the proper Objects of some or other of our Senses, I see not why Emphatical Colours, being the proper and peculiar Objects of the Organ of Sight, and capable to Affect it as Truly and as Powerfully as other Colours, should be reputed but Imaginary ones.

And if we have (which perchance you'll allow) formerly evinc'd Colour, (when / the word is taken in its more Proper sense) to be but Modify'd Light,

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there will be small Reason to deny these to be true Colours, which more manifestly than others disclose themselves to be produc'd by Diversifications of the Light.

3. There is indeed taken notice of a Difference betwixt these Apparent colours, and those that are wont to be esteem'd Genuine, as to the Duration, which has induc'd some Learned Men to call the former rather Evanid than Fantastical. But as the Ingenious *Gassendus* does somewhere Judiciously observe,†\* if this way of Arguing were Good, the Greeness of a Leaf ought to pass for Apparent, because, soon Fading into a Yellow, it scarce lasts at all, in comparison of the Greeness of an Emerald. I shall add, that if the Sun-beams be in a convenient manner trajected through a Glass-prism, and thrown upon some well-shaded Object within a Room, the Rain-bow thereby Painted on the Surface of the Body that Terminates the Beams, may oftentimes last longer than some Colours I have produc'd in certain Bodies, which would justly, and without scruple be accounted Genuine Colours, and yet suddenly Degenerate, and lose their Nature.

4. A greater Disparity betwixt Emphatical / Colours, and others, may perhaps be taken from this, that Genuine Colours seem to be produc'd in Opacous Bodies by Reflection, but Apparent ones in Diaphanous Bodies, and principally by Refraction, I say Principally rather than Solely, because in some cases Reflection also may concurr, but still this seems not to conclude these Latter Colours not to be True ones. Nor must what has been newly said of the Differences of True and Apparent Colours, be interpreted in too Unlimited a Sense, and therefore it may perhaps somewhat Assist you, both to Reflect upon the two fore-going Objections, and to judge of some other Passages which you'll meet with in this Tract, if I take this Occasion to observe to you, that if Water be Agitated into Froth, it exhibits you know a White colour, which soon after it Loses upon the Resolution of the Bubbles into Air and Water, now in this case either the Whiteness of the Froth is a True Colour or not, if it be, then True Colours, supposing the Water pure and free from Mixtures of any thing Tenacious, may be as Short-liv'd as those of the Rain-bow; also the Matter, wherein the Whiteness did Reside, may in a few moments perfectly Lose all foot-steps or remains of it. And / besides, even Diaphanous Bodies may be capable of exhibiting True Colours by Reflection, for that Whiteness is so produc'd, we shall anon make it probable. But if on the other side it be said, that the Whiteness of Froth is an Emphatical Colour, then it must no longer be said, that Fantastical Colours require a certain Position of the Luminary and the Eye, and must be Vary'd or Destroy'd by the Change thereof, since Froth appears White, whether the Sun be Rising or Setting, or in the Meridian, or any where between it and the Horizon, and from what (Neighbouring) place soever the Beholders Eye looks upon it. And since by making a Liquor Tenacious enough, yet without

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Destroying its Transparency, or Staining it with any Colour, you may give the Little Films, whereof the Bubbles consist, such a Texture, as may make the Froth last very many Hours, if not some Days, or even Weeks, it will render it somewhat Improper to assign Duration for the Distinguishing Character to Discriminate Genuine from Fantastical Colours. For such Froth may much outlast the undoubtedly true Colours of some of Nature's Productions, as in that Gaudy Plant not undeservedly call'd the Mervail of *Peru*, the Flowers do often Fade, the / same Day they are Blown; And I have often seen a *Virginian* Flower, which usually Withers within the compass of a Day; and I am credibly Inform'd, that not far from hence a curious Herborist has a Plant, whose Flowers perish in about an Hour. But if the Whiteness of Water turn'd into Froth must therefore be reputed Emphatical, because it appears not that the Nature of the Body is Alter'd, but only that the Disposition of its Parts in reference to the Incident Light is Chang'd, why may not the Whiteness be accounted Emphatical too, which I shall shew anon to be Producibile, barely by such another change in Black Horn? and yet this so easily acquit'd Whiteness seems to be as truly its Colour as the Blackness was before; and at least is more Permanent than the Greenness of Leaves, the Redness of Roses, and, in short, than the Genuine Colours of the most part of Nature's Productions. It may indeed be further Objected, that according as the Sun or other Luminous Body changes place, these Emphatical Colours alter or vanish. But not to repeat what I have just now said, I shall add, that if a piece of Cloath in a Drapers Shop (in such the Light being seldome Primary) be variously Folded, it will appear of differing / Colours, as the Parts happen to be more Illuminated or more Shaded, and if you stretch it Flat, it will commonly exhibit some one Uniform Colour, and yet these are not wont to be reputed Emphatical, so that the Difference seems to be chiefly this, that in the Case of the Rain-bow, and the like, the Position of the Luminary Varies the Colour, and in the Cloath I have been mentioning, the Position of the Object does it. Nor am I forward to allow that in all Cases the Apparition of Emphatical Colours requires a Determinate position of the Eye, for if Men will have the Whiteness of Froth Emphatical, you know what we have already Inferr'd from thence. Besides, the Sun-beams trajected through a Triangular Glass, after the manner lately mention'd, will, upon the Body that Terminates them, Paint a Rain-bow, that may be seen whether the Eye be plac'd on the Right Hand of it or the Left, or Above or Beneath it, or Before or Behind it; and though there may appear some Little Variation in the Colours of the Rain-bow, beheld from Differing parts of the Room, yet such a Diversity may be also observ'd by an Attentive Eye in Real Colours, look'd upon under the like Circumstances. Nor will it follow, / that because there remains no Foot-steps of the Colour upon the Object, when the Prism is Remov'd, that therefore the Colour was not Real, since the Light was truly Modify'd by the Refraction and Reflection it Suffer'd in its Trajection through the Prism; and the Object in our case serv'd for a Specular Body, to Reflect that Colour to the Eye. And that you may not be Startled, *Pyrophilus*,

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that I should Venture to say, that a Rough and Colour'd Object may serve for a *Speculum* to Reflect the Artificial Rain-bow I have been mentioning, consider what usually happens in Darkned Rooms, where a Wall, or other Body conveniently Situated within, may so Reflect the Colours of Bodies, without the Room, that they may very clearly be Discern'd and Distinguish'd, and yet 'tis taken for granted, that the Colours seen in a Darkned Room, though they leave no Traces of themselves upon the Wall or Body that Receives them, are the True Colours of the External Objects, together with which the Colours of the Images are Mov'd or do Rest. And the Errour is not in the Eye, whose Office is only to perceive the Appearances of things, and which does Truly so, but in the Judging or Estimative faculty, which Mistakingly / concludes that Colour to belong to the Wall, which does indeed belong to the Object, because the Wall is that from whence the Beams of Light that carry the Visible *Species*, do come in Straight Lines directly to the Eye, as for the same Reason we are wont at a certain Distance from Concave Sphærical Glasses, to perswade our selves that we see the Image come forth to Meet us, and Hang in the Air betwixt the Glass and Us, because the Reflected Beams that Compose the Image cross in that place, where the Image seems to be, and thence, and not from the Glass, do in Direct Lines take their Course to the Eye, and upon the like Cause it is, that divers Deceptions in Sounds and other Sensible Objects do depend, as we elsewhere declare.

5. I know not, whether I need add, that I have purposely Try'd, (as you'll find some Pages hence, and will perhaps think somewhat strange) that Colours that are call'd Emphatical, because not Inherent in the Bodies in which they Appear, may be Compounded with one another, as those that are confessedly Genuine may. But when all this is said, *Pyrophilus*, I must Advertise you, that it is but problematically Spoken, and that though I think the Opinion / I have endeavour'd to fortifie Probable, yet a great part of our Discourse concerning Colours may be True, whether that Opinion be so or not.

## Chap. V.

1. There are you know, *Pyrophilus*, besides those Obsolete Opinions about Colours which have been long since Rejected, very Various Theories that have each of them, even at this day, Eminent Men for its Abettors;†\* for the Peripatetick Schools, though they dispute amongst themselves divers particulars concerning Colours, yet in this they seem Unanimously enough to Agree, that Colours are Inherent and Real Qualities, which the Light doth but Disclose, and not concurr to Produce. Besides there are *Moderns*, who with a slight Variation adopt the Opinion of *Plato*, and as he would have Colour to be nothing but a Kind of Flame consisting of Minute Corpuscles as it were Darted by the Object against the Eye, to whose Pores their Littleness and Figure made them congruous, so these would have Colour to be an

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Internal Light of the more Lucid parts of the Object, Darkned and consequently Alter'd by the Various Mixtures of the less Luminous / parts.†\* There are also others, who in imitation of some of the Antient *Atomists*, make Colour not to be Lucid steam, but yet a Corporeal *Effluvium* issuing out of the Colour'd Body, but the Knowingst of these have of late Reform'd their Hypothesis, by acknowledging and adding that some External Light is necessary to Excite, and as *they* speak, Sollicit these Corpuscles of Colour as *they* call them, and Bring them to the Eye. Another and more principal Opinion of the *Modern* Philosophers, to which this last nam'd may by a Favourable explication be reconcil'd, is that which derives Colours from the Mixture of Light and Darkness, or rather Light and Shadows. And as for the *Chymists* 'tis known, that the generality of them ascribes the Origine of Colours to the Sulphureous Principle in Bodies, though I find, as I elsewhere largely shew, that some of the Chiefest of them derive Colours rather from Salt than Sulphur, and others, from the third Hypostatical Principle, *Mercury*. And as for the *Cartesians* I need not tell you, that they, supposing the Sensation of Light to be produc'd by the Impulse made upon the Organs of Sight, by certain extremely Minute and Solid Globules, to which the Pores of the Air and other Diaphanous / bodies are pervious, endeavour to derive the Varieties of Colours from the Various Proportion of the Direct Progress or Motion of these Globules to their Circumvolution or Motion about their own Centre, by which Varying Proportion they are by this Hypothesis suppos'd qualify'd to strike the Optick Nerve after several Distinct manners, so to produce the perception of Differing Colours.†\*

2. Besides these six principal Hypotheses, *Pyrophilus*, there may be some others, which though Less known, may perhaps as well as these deserve to be taken into consideration by you; but that I should copiously debate any of them at present, I presume you will not expect, if you consider the Scope of these Papers, and the Brevity I have design'd in them, and therefore I shall at this time only take notice to you in the general of two or three things that do more peculiarly concern the Treatise you have now in your hands.

3. And first, though the Embracers of the several Hypotheses I have been naming to you, by undertaking each Sect of them to explicate Colours indefinitely, by the particular Hypotheses they maintain, seem to hold it forth as the only Needfull Theory about that Subject, yet for my part I doubt / whether any one of all these Hypotheses have a right to be admitted Exclusively to all

others, for I think it Probable, that Whiteness and Blackness may be explicated by Reflection alone without Refraction, as you'll find endeavour'd in the Discourse you'll meet with e're long Of the Origine of Whiteness and Blackness,<sup>†\*</sup> and on the other side, since I have not found that by any Mixture of white and True Black, (for there is a Blewish Black which many

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mistake for a Genuine) there can be a Blew, a Yellow, or a Red, to name no other Colours, produced, and since we do find that these colours may be produc'd in the Glass-prism and other Transparent bodies, by the help of Refractions, it seems that Refraction is to be taken in into the Explication of some Colours, to whose Generation they seem to concurr, either by making a further or other Commixture of Shades with the Refracted Light, or by some other way not now to be discours'd. And as it seems not improbable, that in case the Pores of the Air, and other Diaphanous bodies be every where almost fill'd with such *Globuli* as the *Cartesians* suppose, the Various kind of Motion of these *Globuli*, may in many cases have no small stroak in Varying our Perception of Colour, so / without the Supposition of these *Globuli*, which 'tis not so easie to evince, I think we may probably enough conceive in general, that the Eye may be Variously affected, not only by the Entire Beams of Light that fall upon it as they are such, but by the Order, and by the Degree of Swiftmess, and in a word by the Manner according to which the Particles that compose each Particular Beam arrive at the Sensory, so that whatever be the Figure of the Little Corpuscles, of which the Beams of Light consist, not only the Celerity or Slowness of their Revolution or Rotation in reference to their Progressive Motion, but their more Absolute Celerity, their Direct or Undulating Motion, and other Accidents, which may attend their Appulse to the Eye, may fit them to make Differing Impressions on it.

4. Secondly, For these and the like Considerations, *Pyrophilus*, I must desire that you would look upon this little Treatise, not as a Discourse written Principally to maintain any of the fore-mention'd Theories, Exclusively to all others, or substitute a New one of my Own, but as the beginning of a History of Colours, upon which, when you and your Ingenious friends shall have Enrich'd it, a Solid Theory may be / safely built. But yet because this History is not meant barely for a Register of the things recorded in it, but for an *Apparatus* to a sound and comprehensitive Hypothesis, I thought fit, so to temper the whole Discourse, as to make it as conducive, as conveniently I can to that End, and therefore I have not scrupled to let you see that I was willing, as to save you the labour of Cultivating some Theories that I thought would never enable you to reach the Ends you aim at, so to contract your Enquiries into a Narrow compass, for both which purposes I thought it requisite to do these two things, the *One*, to set down some Experiments which by the help of the Reflections and Insinuations that attend them, may assist you to discover the Infirmiss and Insufficieny both of the common Peripatetick Doctrine, and of the now more applauded Theory of the *Chymists* about Colour, because these two Doctrines having Possess'd themselves, the one of the most part of the Schools, and the other of the Esteem of the Generality of Physicians and other Learned Men, whose Professions and Ways of Study do not exact that they should Scrupulously examine the very First and Simplest Principles of Nature, I fear'd it would be to / little purpose, without doing something to discover the Insufficieny of these

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Hypotheses, that I should, (which was the *Other* thing I thought requisite for me to do) set down among my other Experiments those in the greatest Number, that may let you see, that, till I shall be Better Inform'd, I encline to take Colour to be a Modification of Light, and would invite you chiefly to Cultivate that Hypothesis, and Improve it to the making out of the Generation of Particular Colours, as I have Endeavour'd to apply it to the Explication of Whiteness and Blackness.

5. Thirdly. But, *Pyrophilus*, though this be at present the Hypothesis I preferr, yet I propose it but in a General Sense, teaching only that the Beams of Light, Modify'd by the Bodies whence they are

sent (Reflected or Refracted) to the Eye, produce there that Kind of Sensation, Men commonly call Colour; But whether I think this Modification of the Light to be perform'd by Mixing it with Shades, or by Varying the Proportion of the Progress and Rotation of the *Cartesian Globuli Cælestes*, or by some other way which I am not now to mention, I pretend not here to Declare.<sup>†\*</sup> Much less do I pretend to Determine, or scarce so much as to Hope to / know all that were requisite to be Known, to give You, or even my Self, a perfect account of the Theory of Vision and Colours, for in Order to such an undertaking I would first Know what Light is, and if it be a Body (as a Body or the Motion of a Body it seems to be) what Kind of Corpuscles for Size and Shape it consists of, with what Swiftmess they move Forwards, and Whirl about their own Centres. Then I would Know the Nature of Refraction, which I take to be one of the Abstrusest things (not to explicate Plausibly, but to explicate Satisfactorily) that I have met with in Physicks; I would further Know what Kind and what Degree of Commixture of Darkness or Shades is made by Refractions or Reflections, or both, in the Superficial particles of those Bodies, that being Shin'd upon, constantly exhibit the one, for Instance, a Blew, the other a Yellow, the third a Red Colour; I would further Know why this Contemperation of Light and Shade, that is made, for Example, by the Skin of a Ripe Cherry, should exhibit a Red, and not a Green, and the Leaf of the same Tree should exhibit a Green rather than a Red; and indeed, Lastly, why since the Light that is Modify'd into these Colours consists but of Corpuscles / moved against the *Retina* or Pith of the Optick Nerve, it should there not barely give a Stroak, but produce a Colour, whereas a Needle wounding likewise the Eye, would not produce Colour but Pain. These, and perhaps other things I should think requisite to be Known, before I should judge my Self to have fully Comprehended the True and Whole Nature of Colours; and therefore, though by making the Experiments and Reflections deliver'd in this Paper, I have endeavour'd somewhat to Lessen my Ignorance in this Matter, and think it far more Desireable to discover a Little, than to discover Nothing, yet I pretend but to make it Probable by the Experiments I mention, that some Colours may be Plausibly enough Explicated in the General by the Doctrine here propos'd; For whensoever I would Descend to

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the Minute and Accurate Explication of Particulars, I find my Self very Sensible of the great Obscurity of things, without excepting those which we never see but when they are Enlightned, and confess with *Scaliger*, *Latet natura hæc*, (says he, Speaking of that of Colour) & *sicut aliarum rerum species in profundissima caligine inscitiae humanæ*.<sup>†@</sup> /

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## *THE EXPERIMENTAL HISTORY OF COLOURS.*

### **Part II.**

#### ***Of the Nature of Whiteness and Blackness.***

### **Chap. I.**

1. Though after what I have acknowledged, *Pyrophilus*, of the Abstruse Nature of Colours in *particular*, you will easily believe, that I pretend not to give you a Satisfactory account of Whiteness and Blackness; Yet now wholly to frustrate your Expectation of my offering something by way of Specimen towards the Explication of some Colours in particular, / I shall make choice of These as the most Simple Ones, (and by reason of their mutual Opposition the Least hardly explicable) about which to present you my Thoughts, upon condition you will take them at most to be my Conjectures, not my Opinions.

2. When I apply'd my Self to consider, how the cause of Whiteness might be explan'd by Intelligible and Mechanical Principles, I remembred not to have met with any thing among the Antient



Corpuscularian Philosophers, touching the Quality we call Whiteness, save that *Democritus* is by *Aristotle* said to have ascrib'd the Whiteness of Bodies to their Smoothness, and on the contrary their Blackness to their Asperity.<sup>†@</sup> But though about the Latter of those Qualities his Opinion be allowable, as we shall see anon, yet that he needs a Favourable Interpretation in what is Deliver'd concerning the First, (at least if his Doctrine be not Mis-represented in this point, as it has been in many others) we shall quickly have Occasion to manifest. But amongst the *Moderns*, the most Learned *Gassendus* in his Ingenious Epistle publish'd in the Year 1642. *De apparente / Magnitudine solis humilis & sublimis*, reviving the *Atomical* Philosophy, has, though but Incidentally, deliver'd something towards the Explication of Whiteness upon Mechanical Principles: And because no Man that I know of, has done so before him, I shall, to

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be sure to do him Right, give you his Sense in his own Words:<sup>†@</sup> *Cogites velim* (says he) *lucem quidem in Diaphano nullius coloris videri, sed in Opaco tamen terminante Candicare, ac tantò magis, quantò densior seu collectior fuerit. Deinde aquam non esse quidem coloris ex se candidi & radium tamen ex eâ reflexum versus oculum candicare. Rursus cum plana aquæ Superficies non nisi ex una parte eam reflexionem faciat: si contigerit tamen illam in aliquot bullas intumescere, bullam unamquamque reflectionem facere, & candoris speciem creare certa Superficie parte. Ad hæc Spumam ex aqua pura non alia ratione videri candescere & albescerere quam quod sit congeries confertissima minutissimarum bullarum, quarum unaquæque suum radium reflectit, unde continens candor alborve apparet. Denique Nivem nihil aliud videri quam speciem purissimæ spumæ ex bullulis quam minutissimis & confertissimis cohærentis. Sed ridiculum me exhibeam, si tales meas nugas uberius proponem. /*

3. But though in this passage, that very Ingenious Person has Anticipated part of what I should say; Yet I presume you will for all that expect, that I should give you a fuller Account of that Notion of Whiteness, which I have the least Exceptions to, and of the Particulars whence I deduce it, which to do, I must mention to you the following Experiments and Observations.

Whiteness then consider'd as a Quality in the Object, seems chiefly to depend upon this, That the Superficies of the Body that is call'd White; is Asperated by almost innumerable Small Surfaces, which being of an almost Specular Nature, are also so Plac'd, that some Looking this way, and some that way, they yet Reflect the Rays of Light that fall on them, not towards one another, but outwards towards the Spectators Eye. In this Rude and General account of Whiteness, it seems that besides those Qualities, which are common to Bodies of other Colours, as for instance the Minuteness and Number of the Superficial parts, the two chief things attributed to Bodies as White are made to be, First, that its Little Protuberances and Superficial parts be of somewhat a Specular Nature, that they may as little Looking-glasses each of them Reflect the Beams it / receives, (or the little Picture of the Sun made on it) without otherwise considerably Altering them; whereas in most other Colours, they are wont to be much Chang'd, by being also Refracted, or by being Return'd to the Eye, mixt with Shades or otherwise. And next, that its Superficial parts be so Situated, that they Retain not the Incident Rays of Light by Reflecting them Inwards, but Send them almost all Back, so that the Outermost Corpuscles of a White Body, having their Various Little

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Surfaces of a Specular Nature, a Man can from no place Behold the Body, but that there will be among those Innumerable *Superficieculæ*, that Look some one way, and some another, enough of them Obverted to his Eye, to afford like a broken Looking-glass, a confused *Idæa*, or Representation of Light, and make such an Impression on the Organ, as that for which Men are wont to call a Body White. But this Notion will perhaps be best Explan'd by the same Experiments and Observations, on which it is Built, And therefore I shall now advance to *Them*.

4. And in the first place I consider, that the Sun and other Powerfully Lucid Bodies, are not only wont to Offend, which we call to Dazle our Eyes, but that if any / Colour be to be Ascrib'd to them as they are Lucid, it seems it should be Whiteness: For the Sun at Noon-day, and in Clear weather, and when his Face is less Troubled, and as it were Stained by the Steams of Sublunary Bodies, and when his Beams have much less of the Atmosphere to Traject in their Passage to our Eyes, appears of a Colour more approaching to White, than when nearer the Horizon, the Interposition of certain Sorts of Fumes and Vapours make him oftentimes appear either Red, or at least more Yellow. And when the Sun Shines upon that Natural Looking-glass, a Smooth water, that part of it, which appears to this or that particular Beholder, the most Shin'd on, does to his Eye seem far Whiter than the rest. And here I shall add, that I have sometimes had the Opportunity to observe a thing, that may make to my present purpose, namely, that when the Sun was Veil'd over as it were, with a Thin White Cloud, and yet was too Bright to be Look'd upon Directly without Dazling, by casting my Eyes upon a Smooth water, as we sometimes do to observe Eclipses without prejudice to our Eyes, the Sun then not far from the Meridian, appear'd to me not Red, but so White, that 'twas / not without some Wonder, that I made the Observation. Besides, though we in *English* are wont to say, a thing is Red hot, as an Expression of its being Superlatively *Ignitum*, (if I may so Speak for want of a proper *English* word)<sup>†\*</sup> yet in the Forges of Smiths, and the Furnaces of other Artificers, by that which they call a White heat, they mean a further Degree of *Ignition*, than by that which both they and we call a Red heat.

5. Secondly, I consider, that common Experience informs us, that as much Light Over-powers the Eye, so when the Ground is covered with Snow, (a Body extremely White) those that have Weak Eyes are wont to complain of too much Light: And even those that have not, are generally Sensible of an Extraordinary measure of Light in the Air; and if they are fain to Look very long upon the Snow, find their Sight Offended by it. On which occasion we may call to mind what *Xenophon* relates,<sup>†\*</sup> that his *Cyrus* marching his Army for divers-days through Mountains covered with Snow, the Dazling splendor of its Whiteness prejudic'd the Sight of very many of his

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Souldiers, and Blinded some of them; and other Stories of that Nature may be met with in Writers of good / Note. And the like has been affirm'd to me by credible Persons of my own Acquaintance, and especially by one who though Skill'd in Physick and not Antient confess'd to me when I purposely ask'd him, that not only during his stay in *Muscovy*, he found his Eyes much Impair'd, by being reduc'd frequently to Travel in the Snow, but that the Weakness of his Eyes did not Leave him when he left that Country, but has follow'd him into these Parts, and yet continues to Trouble him.<sup>†\*</sup> And to this doth agree what I as well as others have observ'd, namely, that when I Travell'd by Night, when the Ground was all cover'd with Snow, though the Night otherwise would not have been Lightsome, yet I could very well see to Choose my way. But much more Remarkable to my present purpose is that, which I have met with in *Olaus Magnus*, concerning the way of Travelling in Winter in the *Northern* Regions, where the Days of that Season are so very Short; for after other things not needfull to be here Transcribed:<sup>†@</sup> *Iter*, says he, *Diurnum duo scilicet montana milliaria (quæ 12 Italica sunt) conficiunt. Nocte verò sub splendissima luna, duplatum iter consumunt aut triplatum. Neque id incommodè fit, / cum nivium reverberatione lunaris splendor sublimes & declives campos illustret, ac etiam montium præcipitia ac noxias feras à longè prospiciant evitandas*. Which Testimony I the less Scruple to allege, because that it agrees very well with what has been Affirm'd to me by a Physician of *Mosco*, whom the Notion I have been Treating of concerning Whiteness invited me to ask whether he could not See much farther when he Travell'd by Night in *Russia* than he could do in *England*, or elsewhere, when there was no Snow upon the Ground; For this Ingenious Person inform'd me, that he could See Things at a farr greater Distance, and with more Clearness, when he Travell'd by Night on the *Russian* Snow, though without the Assistance of Moonshine, than we in these Parts would easily be perswaded. Though it seems not unlikely to me, that the Intenseness of the Cold may contribute something to the considerableness



of the Effect, by much Clearing the Air of Darkish Steams, which in these more Temperate Climates are wont to Thicken it in Snowy weather: For having purposely inquir'd of this Doctor, and consulted that Ingenious Navigator Captain *James's* Voyage hereafter to be further mention'd,<sup>†\*</sup> I find

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both their Relations / agree in this, that in Dark Frosty Nights they could Discover more Stars, and See the rest Clearer than we in *England* are wont to do.

6. I know indeed that divers Learned Men think, that Snow so strongly Affects our Eyes, not by a Borrow'd, but a Native Light; But I venture to give it as a Proof, that White Bodies reflect more Light than Others, because having once purposely plac'd a parcel of Snow in a Room carefully Darkned, that no Celestial Light might come to fall upon it; neither I, nor an Ingenious Person, (Skill'd in Opticks) whom I desir'd for a Witness, could find, that it had any other Light than what it receiv'd. And however, 'tis usual among those that Travel in Dark Nights, that the Guides wear something of White to be Discern'd by, there being scarce any Night so Dark, but that in the Free Air there remains some Light, though Broken and Debilitated perhaps by a thousand Reflections from the Opacous Corpuscles that Swim in the Air, and send it to one another before it comes to arrive at the Eye.

7. Thirdly, And the better to shew that White Bodies reflect store of Light, in comparison of those that are otherwise Colour'd, I did in the Darkn'd Room, / formerly mention'd, hold not far from the Hole, at which the Light was admitted, a Sheet only of White Paper, from whence casting the Sun-beams upon a White Wall, whereunto it was Obverted, it manifestly appear'd both to Me, and to the Person I took for a Witness of the Experiment, that it Reflected a far greater Light, than any of the other Colours formerly mention'd, the Light so thrown upon the Wall notably Enlightning it, and by it a good part of the Room. And yet further to shew you, that White Bodies Reflect the Beams From them, and not Towards themselves, Let me add, that Ordinary Burning-glasses, such as are wont to be employ'd to light Tobacco, will not in a great while Burn, or so much as Discolour a Sheet of White Paper. Insomuch that even when I was a Boy, and Lov'd to make Tryals with Burning-glasses, I could not but wonder at this Odd *Phænomenon*, which set me very Early upon Guessing at the Nature of Whiteness, especially because I took notice, that the Image of the Sun upon a White Paper was not so well Defin'd (the Light seeming too Diffus'd) as upon Black, and because I try'd, that Blacking over the Paper with Ink, not only the Ink would be quickly Dry'd up, but the / Paper that I could not Burn before, would be quickly set on Fire. I have also try'd, that by exposing my Hand with a Thin Black Glove over it to the Warm Sun, it was thereby very quickly and considerably more Heated, than if I took off the Glove, and held my Hand Naked, or put on it another Glove of Thin but White Leather. And having thus shewn you, *Pyrophilus*, that White Bodies reflect the most Light of any, let us now proceed, to consider what is further to be taken notice of in them, in order to our present Enquiry.

8. And Fourthly, whereas among the Dispositions we attributed to White Bodies, we also intimated this, That such Bodies are apt, like *Speculums*, though but Imperfect ones, to Reflect the Light that falls on them Untroubled or Unstain'd, we shall besides other particulars to be met with in these Papers, offer you this in favour of the Conjecture; That in the Darkned

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Room several times mention'd in this Treatise, we try'd that the Sun-beams being cast from a Coloured Body upon a neighbouring White Wall, the Determinate Colour of the Body was from the Wall reflected to the Eye; whereas we could in divers cases manifestly Alter the Colour arriving at the Eye, by Substituting / at a convenient Distance, a (conveniently) Colour'd (and Glossy) Body instead of the White Wall. As by throwing the Beams from a Yellow Body upon a Blew, there

would be Exhibited a kind of Green, as in the Experiments about Colours is more fully Declar'd.

9. I know not whether I should on this Occasion take notice, that when, as when looking upon the Calm and Smooth Surface of a River betwixt my Eye and the Sun, it appear'd to be a natural *Speculum*, wherein that part which Reflected to my Eye the Entire and defin'd Image of the Sun, and the Beams less remote from those which exhibited That Image, appear'd indeed of a great and Whitish Brightness, but the rest Comparatively Dark enough: if afterwards the Superficies chanc'd to be a little, but not much troubled, by a gentle Breath of Wind, and thereby reduc'd into a Multitude of Small and Smooth *Speculums*, the Surface of the River would suitably to the Doctrine lately deliver'd, at a Distance appear very much of Kin to White, though it would lose that Brightness or Whiteness upon the Return of the Surface to Calmness and an Uniform Level. And I have sometimes for Tryals sake brought in by a Lenticular Glass, the Image of a River, Shin'd upon / by the Sun, into an Upper Room Darkn'd, and Distant about a Quarter of a Mile from the River, by which means the Numerous Declining Surfaces of the Water appear'd so Contracted, that upon the Body that receiv'd the Images, the whole River appear'd a very White Object at two or three paces distance.<sup>†\*</sup> But if we drew Near it, this Whiteness appear'd to proceed from an Innumerable company of Lucid Reflections, from the several Gently wav'd Superficies of the Water, which look'd Near at hand like a Multitude of very Little, but Shining Scales of Fish, of which many did every moment Disappear, and as many were by the Sun, Wind and River generated anew. But though this Observation seem'd Sufficiently to discover, how the Appearing Whiteness in that case was Produc'd, yet in some other cases Water may have the Same, though not so Vivid a Colour upon other Accounts; for oftentimes it happens that the Smooth Surface of the Water does appear Bright or Whitish, by reason of the Reflection not immediatly of the Images of the Sun, but of the Brightness of the Sky; and in such cases a Convenient Wind may where it passes along make the Surface look Black, by causing many such Furrows and Cavities, as may make the Inflected Superficies / of the Water reflect the Brightness of the Sky rather Inward than Outward. And again, if the Wind increase into a Storm, the Water may appear White, especially near the Shore and the Ship, namely because the Rude Agitation Breaks it into Fome or Froth. So much do Whiteness and Blackness depend upon the Disposition of the Superficial parts of a Body to Reflect the Beams

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of Light Inward or Outward. But that as White Bodies reflect the most Light of any, so there Superficial Particles are, in the Sense newly Deliver'd, of a Specular Nature, I shall now further endeavour to shew both by the making of Specular bodies White, and the making of a White body Specular.

10. In the Fifth place then, I will inform You, that (not to repeat what *Gassendus* observes concerning Water)<sup>†\*</sup> I have for Curiosity sake Distill'd Quick-silver in a Cucurbit, fitted with a Capacious Glass-head, and observ'd that when the Operation was perform'd by the Degrees of Fire requisite for my purpose, there would stick to the Inside of the Alembick a multitude of Little round drops of *Mercury*. And as you know that *Mercury* is a Specular Body, so each of these Little drops was a small round Looking-glass, / and a Multitude of them lying Thick and Near one another, they did both in my Judgment, and that of those I Invited to see it, make the Glass they were fastened to, appear manifestly a White Body. And yet as I said, this Whiteness depended upon the Minuteness and Nearness of the Little Mercurial *Globuli*, the Convexity of whose Surfaces fitted them to represent in a Narrow compass a Multitude of Little Lucid Images to differingly situated Beholders. And here let me observe a thing that seems much to countenance the Notion I have been recommending: namely, that whereas divers parts of the Sky, and especially the Milky-way, do to the naked Eye appear White, (as the name it self imports) yet the Galaxie look'd upon through the Telescope, does not shew White, but appears to be made up of a Vast multitude of Little Starrs; so that a Multitude of Lucid Bodies, if they be so Small that they cannot Singly or apart be discern'd by the Eye, and if they be sufficiently Thick set by one another, may by their confus'd beams appear to the Eye One White Body. And why it is not possible, that the like may be done, when a

Multitude of Bright and Little Corpuscles being crowded together, are made to send together Vivid beams to the Eye, / though they Shine but as the Planets by a Borrow'd Light?

11. But to return to our Experiments. We may take notice, That the White of an Egg, though in part Transparent, yet by its power of Reflecting some Incident Rays of Light, is in some measure a Natural *Speculum*, being long agitated with a Whisk or Spoon, loses its Transparency, and becomes very White, by being turn'd into Froth, that is into an Aggregate of Numerous small Bubbles, whose Convex Superficies fits them to Reflect the Light every way Outwards. And 'tis worth Noting, that when Water, for instance, is Agitated into Froth, if the Bubbles be Great and Few, the Whiteness will be but Faint, because the number of *Specula* within a Narrow compass is but Small, and they are not Thick set enough to Reflect so Many Little Images or Beams of the Lucid Body, as are requisite to produce a Vigorous Sensation of Whiteness: And partly least it should be said, that the Whiteness of such Globulous Particles proceeds from the Air Included in the Froth; (which to make good, it should be prov'd that the Air it self is White) and partly to illustrate the better the Notion we have propos'd of Whiteness, I shall add, that I

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purposely made this Experiment, I took a quantity [of] / Fair water, & put to it in a clear Glass phial, a convenient quantity of Oyl or Spirit of Turpentine, because that Liquor will not Incorporate with Water, and yet is almost as Clear and Colourless as it; these being Gently Shaken together, the Agitation breaks the Oyl (which as I said, is Indispos'd to Mix like Wine or Milk *per minima*<sup>†\*</sup> with the Water) into a Multitude of Little Globes, which each of them Reflecting Outwards a Lucid Image, make the Imperfect Mixture of the two Liquors appear Whitish; but if by Vehemently Shaking the Glass for a competent time you make a further Comminution of the Oyl into far more Numerous and Smaller *Globuli*, and thereby confound it also better with the Water, the Mixture will appear of a Much greater Whiteness, and almost like Milk; whereas if the Glass be a while let alone, the Colour will by degrees Impair, as the Oily globes grow Fewer and Bigger, and at length will quite Vanish, leaving both the Liquors Distinct and Diaphanous as before. And such a Tryal hath not ill succeeded, when instead of the Colourless Oyl of Turpentine I took a Yellow Mixture made of a good Proportion of Crude Turpentine dissolv'd in that Liquor; and (if I mis-remember not) it also succeeded better than one would / expect, when I employ'd an Oyl brought by Filings of Copper infused in it, to a deep Green. And this (by the way) may be the Reason, why often times when the Oyls of some Spices and of Anniseeds &c. are Distilled in a Limbec with Water, the Water (as I have several times observ'd) comes over Whitish, and will perhaps continue so for a good while, because if the Fire be made too Strong, the subtile Chymical Oyl is thereby much Agitated and Broken, and Blended with the Water in such Numerous and Minute Globules, as cannot easily in a short time Emerge to the Top of the Water, and whilst they Remain in it, make it, for the Reason newly intimated, look Whitish; and perhaps upon the same Ground a cause may be rendred, why Hot water is observ'd to be usually more Opacous and Whitish, than the same Water Cold, the Agitation turning the more Spirituous or otherwise Conveniently Dispos'd Particles of the Water into Vapours, thereby Producing in the Body of the Liquor a Multitude of Small Bubbles, which interrupt the Free passage, that the Beams of Light would else have Every way, and from the Innermost parts of the Water Reflect many of them Outwards. These and the like Examples, *Pyrophilus*, / have induc'd me to Suspect, that the Superficial Particles of White bodies, may for the Most part be as well Convex as Smooth; I content my self to say *Suspect* and *for the most part*, because it seems not Easie to prove, that when Diaphanous bodies, as we shall see by and by, are reduc'd into White Powders, each Corpuscle must needs be of a Convex Superficies, since perhaps it may Suffice that Specular Surfaces look severally ways. For (as we have seen) when a Diaphanous Body comes to be reduc'd to very Minute parts, it thereby requires a Multitude of Little Surfaces within a Narrow compass. And though each of these should not be of a Figure Convenient to Reflect a

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Round Image of the Sun, yet even from such an Inconveniently Figur'd body, there may be Reflected some (either Streight or Crooked) Physical Line of Light, which Line I call Physical, because it has some Breadth in it, and in which Line in many cases some Refraction of the Light falling upon the Body it depends on, may contribute to the Brightness, as if a Slender Wire, or Solid Cylinder of Glass be expos'd to the Light, you shall see in some part of it a vivid Line of Light, and if we were able to draw out and lay together a Multitude of these Little / Wires or Thrids of Glass, so Slender, that the Eye could not discern a Distance betwixt the Luminous Lines, there is little doubt (as far as I can guess by a Tryal purposely made with very Slender, but far less Slender Thrids of Glass, whose Aggregate was Look'd upon one way White) but the whole Physical Superficies compos'd of them, would to the Eye appear White, and if so, it will not be always necessary that the Figure of those Corpuscles, that make a Body appear White, should be *Globulous*. And as for Snow it self, though the Learned *Gassendus* (as we have seen above)<sup>†\*</sup> makes it to seem nothing else but a pure Frozen Froth, consisting of exceedingly Minute and Thick set Bubbles; yet I see no necessity of Admitting that, since not only by the Variously and Curiously Figur'd Snow, that I have divers times had the Opportunity with Pleasure to observe, but also by the Common Snow, it rather doth appear both to the Naked Eye, and in a *Microscope*, often, if not most commonly, to consist principally of Little Slender Icicles of several Shapes, which afford such Numerous Lines of Light, as we have been newly Speaking of.

12. Sixthly, If you take a Diaphanous Body, as for instance a Piece of Glass, and / reduce it to Powder, the same Body, which when it was Entire, freely Transmitted the Beams of Light, acquiring by Contusion a multitude of Minute Surfaces, each of which is as it were a Little, but Imperfect *Speculum*, is qualify'd to Reflect in a Confus'd manner, so many either Beams, or Little and Singly Unobservable Images of the Lucid Body, that from a Diaphanous it Degenerates into a White Body. And I remember, I have for Trials sake taken Lumps of Rock Crystal, and Heating them Red hot in a Crucible, I found according to my Expectation, that being Quench'd in Fair water, even those that remain'd in seemingly entire Lumps, exchang'd their Translucency for Whiteness, the Ignition and Extinction having as it were Crack'd each Lump into a multitude of Minute Bodies, and thereby given it a great multitude of new Surfaces. And ev'n with Diaphanous Bodies, that are Colour'd, there may be this way a Greater Degree of Whiteness produc'd, than one would lightly think; as I remember, I have by Contusion obtain'd Whitish Powders of *Granates*, Glass of *Antimony*, and *Emeralds* finely Beaten, and you may more easily make the Experiment, by taking Good Venereal *Vitriol* of a Deep Blew, / and comparing with some of the Entire Crystals purposely reserv'd, some of the Subtile Powder of the same Salt, which will Comparatively exhibit a very considerable degree of Whitishness.

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13. Seventhly, And as by a Change of Position in the Parts, a Body that is not White, may be made White, so by a Slight change of the Texture of its Surface, a White Body may be Depriv'd of its Whiteness. For if, (as I have try'd in Gold-smiths Shops) you take a piece of Silver that has been freshly Boyl'd, as the Artificers call it, (which is done by, first Brushing, and then Decocting it with Salt and Tartar, and perhaps some other Ingredients) you shall find it to be of a Lovely White. But if you take a piece of Smooth Steel, and therewith Burnish a part of it, which may be presently done; you shall find that Part will Lose its Whiteness, and turn a *Speculum*, looking almost every where Dark, as other Looking-glasses do, which may not a little confirm our Doctrine. For by this we may guess, what it is chiefly that made the Body White before, by considering that all that was done to deprive it of that Whiteness, was only to Depress the Little Protuberances that were before on the Surface of the Silver / into one Continu'd Superficies, and thereby effect this, that now the Image of the Lucid Body, and consequently a Kind of Whiteness shall appear to your Eye, but in some place of the greater Silver Looking-glass (whence the Beams reflected at an Angle Equal to that wherewith they fall on it, may reach your Eye) whilst the Asperity remain'd Undestroy'd, the Light falling on innumerable Little *Specula* Obverted some one way, and some another, did from all Sensibly Distinguishable parts of the Superficies reflect confus'd Beams or Representations of Light

to the Beholders Eye, from whence soever he chance to Look upon it. And among the Experiments annex'd to this Discourse, you will find One, wherein by the Change of Texture in Bodies, Whiteness is in a Trice both Generated and Destroy'd.<sup>†\*</sup>

## Chap. II.

1. What we have Discours'd of Whiteness, may somewhat Assist us to form a Notion of Blackness, those two Qualities being Contrary enough to Illustrate each other. Yet among the Antient *Philosophers* I find less Assistance / to form a Notion of Blackness than of Whiteness, only *Democritus* in the passage above Recited out of *Aristotle* has given a General Hint of the Cause of this Colour, by referring the Blackness of Bodies to their Asperity.<sup>†\*</sup> But this I call but a General Hint, because those Bodies that are Green, and Purple, and Blew, seem to be so as well as Black ones, upon the Account of their Superficial Asperity. But among the *Moderns*, the formerly mention'd *Gassendus*, perhaps invited by this Hint of *Democritus*, had Incidentally in another Epistle given us, though a very Short, yet a somewhat Clearer account of the Nature of Blackness in these words: *Existimare par est corpora suapte Naturâ nigra constare ex particulis, quarum Superficieculæ scabræ sint, nec facilè*

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*lucem extrorsum reflectant.*<sup>†\*</sup> I wish this Ingenious Man had enlarg'd himself upon this Subject; For indeed it seems, that as that which makes a Body White, is chiefly such a Disposition of its Parts, that it Reflects (I mean without much Interruption) more of the Light that falls on it, than Bodies of any other Colour do, so that which makes a Body Black is principally a Peculiar kind of Texture, chiefly of its Superficial Particles, whereby it does as it were Dead the Light / that falls on it, so that very little is Reflected Outwards to the Eye.

2. And this Texture may be Explicated two, and perhaps more than two several ways, whereof the first is by Supposing in the Superficies of the Black Body a Particular kind of Asperity, whereby the Superficial Particles reflect but Few of the Incident Beams Outwards, and the rest Inwards towards the Body it self. As if for Instance, we should conceive the Surface of a Black Body to be Asperated by an almost Numberless throng of Little Cylinders, Pyramids, Cones, and other such Corpuscles, which by their being Thick Set and *Erected*, reflect the Beams of Light from one to another Inwards, and send them too and fro so often, that at length they are Lost, before they can come to Rebound out again to the Eye. And this is the first of the two mention'd ways of Explicating Blackness. The other way is by Supposing the Texture of Black Bodies to be such, that either by their Yielding to the Beams of Light, or upon some other Account, they do as it were Dead the Beams of Light, and keep them from being Reflected in any Plenty, or with any Considerable Vigour of Motion, Outwards. According to this Notion it may be said, that / the Corpuscles that make up the Beams of Light, whether they be Solary *Effluviums*, or Minute Particles of some Ætherial Substance, Thrusting on one another from the Lucid Body, do, falling on Black Bodies, meet with such a Texture, that such Bodies receive Into themselves, and Retain almost all the Motion communicated to them by the Corpuscles that make up the Beams of Light, and consequently Reflect but Few of them, or those but Languidly, towards the Eye, it happening here almost in like manner as to a Ball, which thrown against a Stone or Floor, would Rebound a great way Upwards, but Rebounds very Little or not at all, when it is thrown against Water, or Mud, or a Loose Net, because the Parts yield, and receive into themselves the Motion, on whose Account the Ball should be Reflected Outwards. But this Last way of Explicating Blackness, I shall content my Self to have Propos'd, without either Adopting it, or absolutely Rejecting it. For the Hardness of Touch-stones, Black Marble, and other Bodies, that being Black are Solid, seem to make it somewhat Improbable, that such Bodies should be of so Yielding a Texture, unless we should say, that some Bodies may be more Dispos'd to Yield to the Impulses of / the Corpuscles of Light by reason of a Peculiar Texture, than other Bodies, that in other Tryals appear to be Softer than they. But though the Former of these two Explications of Blackness be that, by which we shall



as we said, we shall not Absolutely Reject this Latter, partly because they both Agree in this, that Black Bodies Reflect but Little of the Light that falls on them, and partly because it is not Impossible, that in some Cases both the Disposition of the Superficial particles, as to Figure and Position, and the Yielding of the Body, or some of its Parts, may Joyntly, though not in an Equal measure concurr to the rendring of a Body Black. The Considerations that induc'd me to propose this Notion of Blackness, as I Explain'd it, are principally these:

3. First, That as I lately said, Whiteness and Blackness being generally reputed to be Contrary Qualities, Whiteness depending as I said upon the Disposition of the Parts of a Body to Reflect much Light, it seems likely, that Blackness may depend upon a Contrary Disposition of the Black Bodies Surface; But upon this I shall not Insist.

4. Next then we see, that if a Body of / One and the same Colour be plac'd, part in the Sun-beams, and part in the Shade, that part which is not Shin'd on will appear more of Kin to Blackness than the other, from which more Light Rebounds to the Eye; And Dark Colours seem the Blacker, the less Light they are Look'd upon in, and we think all Things Black in the Dark, when they send no Beams to make Impressions on our Organs of Sight, so that Shadows and Darkness are near of Kin, and Shaddow we know is but a Privation of Light, and accordingly Blackness seems to proceed from the Paucity of Beams Reflected from the Black Body to the Eye, I say the Paucity of Beams, because those Bodies that we call Black, as Marble, Jeat, &c. are Short of being perfectly so, else we should not See them at all. But though the Beams that fall on the Sides of those Erected Particles that we have been mentioning, do Few of them return Outwards, yet those that fall upon the Points of those Cylinders, Cones, or Pyramids, may thence Rebound to the Eye, though they make there but a Faint Impression, because they Arrive not there, but Mingl'd with a great Proportion of Little Shades. This may be Confirm'd by my having procur'd a Large piece of Black / Marble well Polish'd, and brought to the Form of a Large Sphærical and Concave *Speculum*; For on the Inside this Marble being well Polish'd, was a kind of Dark Looking-glass, wherein I could plainly see a Little Image of the Sun, when that Shin'd upon it. But this Image was very far from Offending and Dazling my Eyes, as it would have done from another *Speculum*; Nor, though the *Speculum* were Large, could I in a Long time, or in a Hot Sun set a piece of Wood on Fire, though a far less *Speculum* of the same Form, and of a more Reflecting Matter, would have made it Flame in a Trice.

5. And on this Occasion we may as well in Reference to something formerly deliver'd concerning Whiteness, as in Reference to what has been newly said, Subjoyn what we further observ'd touching the Differing Reflections of Light from White and Black Marble, namely, that having taking a pretty Large Mortar of White Marble, New and Polish'd in the Inside, and Expos'd it to the Sun, we found that it Reflected a great deal of Glaring Light, but so Dispers'd, that we could not make the Reflected Beams concurr in any such Conspicuous *Focus*, as that newly taken notice of in the

Black Marble, though / perhaps there may enough of them be made to meet near the Bottom, to make some Kind of *Focus*, especially since by holding in the Night-time a Candle at a convenient Distance, we were able to procure a Concourse of some, though not many of the Reflected Beams, at about two Inches distant from the Bottom of the Mortar: But we found the Heat even of the Sun-beams so Dispersedly Reflected to be very Lanquid, even in Comparison of the Black Marbles *Focus*. And the Little Picture of the Sun, that appear'd upon the White Marble as a *Speculum*, was but very Faint and exceeding ill Defin'd. Secondly, That taking two pieces of Plain and Polish'd Surfaces, and casting on them Successively the Beams of the same Candle, in such manner, as that the Neighbouring Superficies being Shaded by an Opacous and Perforated Body, the Incident

Beams were permitted to pass but through a Round Hole of about Half an Inch Diameter, the Circle of Light that appear'd on the White Marble was in Comparison very Bright, but very ill Defin'd; whereas that on the Black Marble was far less Luminous, but much more precisely Defin'd.

6. Thirdly, When you Look upon a piece of Linnen that has Small Holes in it, / those Holes appear very Black, and Men are often deceiv'd in taking Holes for Spots of Ink; And Painters to represent Holes, make use of Black, the Reason of which seems to be, that the Beams that fall on those Holes, fall into them so Deep, that none of them is Reflected back to the Eye. And in narrow Wells part of the Mouth seems Black, because the Incident Beams are Reflected Downwards from one side to another, till they can no more Rebound to the Eye.

We may consider too, that if Differing parts of the same piece of Black Velvet be stroak'd Opposite ways, the piece of Velvet will appear of two Distinct kinds of Blackness, the one far Darker than the other, of which Disparity the Reason seems to be, that in the Less obscure part of the Velvet, the Little Silken Piles whereof 'tis made up, being Inclin'd, there is a Greater part of each of them Obverted to the Eye, whereas in the other part the Piles of Silk being more Erected, there are far Fewer Beams Reflected Outwards from the Lateral parts of each Pile, so that most of those that Rebound to the Eye, come from the Tops of the Piles, which make but a Small part of the whole Superficies, that may be cover'd by the piece of Velvet. / Which Explication I propose, not that I think the Blackness of the Velvet proceeds from the Cause assign'd, since each Single Pile of Silk is Black by reason of its Texture, in what Position soever you Look upon it; But that the Greater Blackness of one of these Tuffts seems to proceed from the Greater Paucity of Beams Reflected from it, and that from the Fewness of those Parts of a Surface that Reflect Beams, and the Multitude of those Shaded Parts that Reflect none. And I remember, that I have oftentimes observ'd, that the Position of Particular Bodies far greater than Piles of Silk in reference to the Eye, may notwithstanding their having each of them a Colour of its own, make one part of their Aggregate appear far Darker than the other; For I have near Great Towns often taken notice, that a Cart-load of Carrots pack'd up,

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appear'd of a much Darker Colour when Look'd upon, where the Points of the Carrots were Obverted to the Eye, than where the Sides of them were so.

7. Fourthly, In a Darkned Room, I purposely observ'd, that if the Sun-beams, which came in at the Hole were receiv'd upon White or any other Colour, and directed to a Convenient place of the Room, / they would Manifestly, though not all Equally, Encrease the Light of that Part; whereas if we Substituted, either a piece of Black Cloth or Black Velvet, it would so Dead the Incident Beams, that the place (newly mention'd) whereto I Obverted the Black Body, would be Less Enlightned than it was before, when it received its Light but from the Weak and Oblique Reflections of the Floor and Walls of a pretty Large Room, through which the Beams that came in at the Hole were Confusedly and Brokenly Dispers'd.

8. Fifthly, And to shew that the Beams that fall on Black Bodies, as they do not Rebound Outwards to the Eye, so they are Reflected towards the Body it self as the Nature of those Erected Particles, to which we have imputed Blackness, requires, we will add an Experiment that will also confirm our Doctrine touching Whiteness; Namely, that we took a Broad and Large Tile, and having Whitened over one half of the Superficies of it, and Black'd the other, we expos'd it to the Summer Sun; And having let it lye there a convenient time (for the Difference is more Apparent, if it have not lain there too long) we found, as we expected, that whilst the Whited part of the Tile remained Cool enough, the / Black'd part of the same Tile was grown not only Sensible, but very Hot, (sometimes to a strong Degree.) And to satisfie some of our Friends the more, we have sometimes left upon the Surface of the Tile, besides the White and Black parts thereof, a part that Retain'd the native Red of the Tile it self, and Exposing them to the Sun, we observ'd this Last mention'd to have Contracted a

Heat in comparison of the White, but a Heat Inferiour to that of the Black, of which the Reason seems to be, that the Superficial Particles of Black Bodies, being, as we said, more Erected, than those of White or Red ones, the Corpuscles of Light falling on their sides, being for the most part Reflected Inwards from one Particle to another, and thereby engag'd as it were and kept from Rebouncing Upwards, they communicate their brisk Motion, wherewith they were impell'd against the Black Body, (upon whose account had they fallen upon a White Body, they would have been Reflected Outwards) to the Small parts of the Black Body, and thereby Produce in those Small parts such an Agitation, as (when we feel it) we are wont to call Heat. I have been lately inform'd, that an Observation near of Kin to Ours, has been made by some Learned Men in *France* and / *Italy*, by long Exposing to a very Hot Sun, two pieces of Marble, the one White, the other Black;†\* But though the Observation be worthy of them, and may confirm the same Truth with Our Experiment, yet besides that our Tryal needs not the Summer, nor any Great Heat to succeed, It

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seems to have this Advantage above the other, that whereas Bodies more Solid, and of a Closer Texture, though they use to be more Slowly Heated, are wont to receive a Greater Degree of Heat from the Sun or Fire, than (*Cæteris paribus*) Bodies of a Slighter Texture; I have found by the Information of Stone-cutters, and by other ways of Enquiry, that Black Marble is much Solider and Harder than White, so that possibly the Difference betwixt the Degrees of Heat they receive from the Sun-beams will by many be ascrib'd to the Difference of their Texture, rather than to that of their Colour, though I think our Experiment will make it Probable enough that the greater part of that Difference may well be ascrib'd to that Disposition of Parts, which makes the one Reflect the Sun-beams Inward, and the other Outwards. And with this Doctrine accords very well, that Rooms hung with Black, are not only Darker than else they would be, but are / wont to be Warmer too; Insomuch that I have known a great Lady, whose Constitution was somewhat Tender, complain that she was wont to catch Cold, when she went out into the Air, after having made any long Visits to Persons, whose Rooms were hung with Black. And this is not the only Lady I have heard complain of the Warmth of such Rooms, which though perhaps it may be partly imputed to the *Effluvia* of those Materials wherewith the hangings were Dy'd, yet probably the Warmth of such Rooms depends chiefly upon the same Cause that the Darkness does; As (not to repeat what I formerly Noted touching my Gloves,) to satisfie some Curious Persons of that Sex, I have convinc'd them, by Tryall, that of two Pieces of Silken Stuff given me by themselves, and expos'd in their Presence, to the same Window, Shin'd on by that Sun, the White was *considerably* Heated, when the Black was not so much as *Sensibly* so.

9. Sixthly, I remember, that Acquainting one Day a *Virtuoso* of Unsuspected Credit, that had Visited Hot Countries, with part of what I have here Deliver'd concerning Blackness, he Related to me by way of Confirmation of it, a very notable / Experiment, which he had both Seen others make, and Made himself in a Warm Climate, namely, that having carefully Black'd over Eggs, and Expos'd them to the Hot Sun, they were thereby in no very Long time well Roasted, to which Effect I conceive the Heat of the Climate must have Concurr'd with the Disposition of the Black Surface to Reflect the Sun-beams Inward, for I remember, that having made that among other Tryals in *England*, though in Summer-time, the Eggs I expos'd, acquir'd indeed a considerable Degree of Heat, but yet not so Intense a One, as prov'd sufficient to Roast them.

10. Seventhly, and Lastly, Our Conjectures at the Nature of Blackness may be somewhat Confirm'd by the (formerly mention'd) Observation of the Blind *Dutch-man*, that Discerns Colours with his Fingers;†\* for he says, that he Feels a greater Roughness upon the Surfaces of Black Bodies, than upon those of Red, or Yellow, or Green. And I remember, that the Diligent *Bartholinus* says, that a Blind Earl of *Mansfield* could Distinguish White from

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Black only by the Touch,<sup>†@</sup> which would sufficiently Argue a great Disparity in the Asperities, or other / Superficial Textures of Bodies of those two Colours, if the Learn'd Relator had Affirm'd the Matter upon his own Knowledge.

11. These, *Pyrophilus*, are the chief things that Occurr to me at present, about the Nature of Whiteness and Blackness, which if they have Rendred it so much as Probable, that in *Most*, or at least *Many* Cases, the Causes of these Qualities may be such as I have Adventur'd to Deliver, it is as much as I Pretend to; for till I have Opportunity to Examine the Matter by some further Tryals, I am not sure, but that in some White and Black Bodies, there may Concurr to the Colour some peculiar Texture or Disposition of the Body, whereby the Motion of the Small Corpuscles that make up the Incident Beams of Light, may be Differingly Modify'd, before they reach the Eye, especially in this, that White Bodies do not only Copiously Reflect those Incident Corpuscles Outwards, but Reflect them Briskly, and do not otherwise Alter them in the manner of their Motion. Nor shall I now stay to Enquire, whether some of those other ways, (as a Disposition to Alter the Velocity, the Rotation, or the Order and Manner of Appulse to the Eye of the Reflected Corpuscles / that Compos'd the Incident Beams of Light) which we mention'd when we consider'd the Production of Colours in General, may not in some Cases be Applicable to those of White and Black Bodies: For I am yet so much a *Seeker* in this Matter, and so little Wedded to the Opinions I have propos'd, that what I am to add shall be but the Beginning of a Collection of Experiments and Observations towards the History of Whiteness and Blackness, without at present interposing my Explications of them, that so, I may assist your Enquires without much Fore-stalling or Biassing your Judgment./

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## Experiment[s] In Consort, Touching Whiteness & Blackness.<sup>†\*</sup>

### *Experiment I.*

Having promis'd in the 114, and 115. Pages of the foregoing Discourse of Whiteness and Blackness, to shew, that those two Colours may by a change of Texture in bodies, each of them apart Diaphanous and Colourless, be at pleasure and in a trice as well Generated as Destroy'd, We shall begin with Experiments that may acquit us of that promise.

Take then what Quantity you please of Fair Water, and having Heated it, put into it as much good Common Sublimate, as it is able to Dissolve, and (to be sure of having / it well glutted:) continue putting in the Sublimate, till some of it lye Untouch'd in the bottom of the Liquor, Filter this Solution through Cap-paper, to have it cleer and limpid, and into a spoonfull or two thereof, (put into a clean glass vessel,) shake about four or five drops (according as you took more or less of this Solution) of good limpid Spirits of Urine, and immediately the whole mixture will appear White like Milk, to which mixture if you presently add a convenient proportion of Rectifi'd *Aqua Fortis* (for the number of drops is hard to determine, because of the Differing Strength of the liquor, but easily found by tryal) the Whiteness will presently disappear, and the whole mixture become Transparent, which you may, if you please, again reduce to a good degree of Whiteness (though inferiour to the first) onely by a more copious affusion of fresh Spirit of Urine. *N.B.* First, That it is not so necessary to employ either *Aqua Fortis* or Spirit of Urine about this Experiment, but that we have made it with other liquors instead of these; of which perhaps more elsewhere. Secondly, That this Experiment, though not made with the same *Menstruums*, nor producing the same Colour, is yet much of Kin to that other to be / mentioned in this Tract among our other Experiments of Colours, about turning a Solution of Præcipitate into an Orange-colour, and the Chymical Reason being much alike in both, the annexing it to one of them may suffice FOR both.

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Make a strong Infusion of broken Galls in Fair Water, and having Filtred it into a clean Vial, add more of the same liquor to it, till you have made it somewhat Transparent, and sufficiently diluted the Colour, for the credit of the Experiment, lest otherwise the Darkness of the liquor might make it be objected, that 'twas already almost Ink; Into this Infusion shake a convenient quantity of a Cleer, but very strong Solution of Vitriol, and you shall immediately see the mixture turn Black almost like Ink, and such a way of producing Blackness is vulgar enough; but if presently after you doe upon this mixture drop a small quantity of good oyl of Vitriol, and, by shaking the Vial disperse it nimbly through the two other liquors, you shall (if you perform your part well, and have employ'd oyl of Vitriol Cleer and Strong enough) see the Darkness of the liquor presently begin / to be discuss'd, and grow pretty Cleer and Transparent, losing its Inky Blackness, which you may again restore to it by the affusion of a small quantity of a very strong Solution of Salt of Tartar. And though neither of these Atramentous liquors will seem other than very Pale Ink, if you write with a clean Pen dipt in them, yet that is common to them with some sorts of Ink that prove very good when Dry, as I have also found, that when I made these carefully, what I wrote with either of them, especially with the Former, would when throughly Dry grow Black enough not to appear bad Ink. This Experiment of taking away and restoring Blackness from and to the liquors, we have likewise tryed in Common Ink; but there it succeeds not so well, and but very slowly, by reason that the Gum wont to be employed in the making it, does by its Tenacity oppose the operations of the above mention'd Saline liquors. But to consider Gum no more, what some kind of Præcipation may have to do in the producing and destroying of Inks without it, I have elsewhere given you some occasion and assistance to enquire; But I must not now stay to do so my self, only I shall take notice to you, that though it be taken for granted that bodies will not be Præcipated by Alcalizat Salts, / that have not first been dissolved in some Acid *Menstruums*, yet I have found upon tryals, which my conjectures lead me to make on purpose, That divers Vegetables *barely infus'd*, or *but slightly decocted in common water*, would, upon the affusion of a Strong and Cleer *Lixivium* of Potashes, and much more of some other Præcipitating liquors that I sometimes employ, afford good store of a Crudled matter, such as I have had in the Præcipitations of Vegetable substances, by the intervention of Acid things, and that this matter was easily separable from the rest of the liquor, being left behind by it in the Filtre; and in making the first Ink mention'd in this Experiment, I found that I could by Filtration separate pretty store of a very Black pulverable substance, that remain'd in the

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Filtre, and when the Ink was made Cleer again by the Oyl of Vitriol, the affusion of dissolv'd *Sal Tartari* seem'd but to Præcipitate, and thereby to Unite and render Conspicuous the particles of the Black mixture that had before been dispers'd into very Minute and singly Invisible particles by the Incisive and resolving power of the highly Corrosive Oyl of Vitriol.<sup>†\*</sup> /

And to manifest, *Pyrophilus*, that Galls are not so requisite as many suppose to the making Atramentous Liquors, we have sometimes made the following Experiment, We took dried Rose leaves and Decocted them for a while in Fair Water, into two or three spoonfulls of this Decoction we shook a few drops of a strong and well filtrated Solution of Vitriol (which perhaps had it been Green would have done as well) and immediately the / mixture did turn Black, and when into this mixture presently after it was made, we shook a just Proportion of *Aqua Fortis*, we turn'd it from a Black Ink to a deep Red one, which by the affusion of a little Spirit of Urine may be reduc'd immediately to an Opacous and Blackish Colour. And in regard, *Pyrophilus*, that in the former Experiments, both the Infusion of Galls, and the Decoction of Roses, and the Solution of Copperis employ'd about them, are endow'd each of them with its own Colour, there may be a more noble Experiment of the sudden production of Blackness made by the way mention'd in the Second Section of the Second Part of our Essays, for though upon the Confusion of the two Liquors there



mention'd, there do immediately emerge a very Black mixture, yet both the Infusion of *Orpiment* and the Solution of *Minium* were before their being joyn'd together, Limpid and Colourless. /

### *Experiment III.*

If pieces of White Harts-horn be with a competent degree of fire distill'd in a Glass-retort, they will, after the avolation of the Flegm, Spirit, Volatile Salt, and the looser and lighter parts of the Oleagenous substance, remain behind of a Cole-black colour. And even Ivory it self being skilfully Burnt (how I am wont to do it, I have elsewhere set down)-<sup>†\*</sup> affords Painters one of the best and deepest Blacks they have, and yet in the Instance of distill'd Harts-horn, the operation being made in Glass-vessels carefully clos'd, it appears there is no Extraneous Black substance that Insinuates it self into White Harts-horn, and thereby makes it turn Black; but that the Whiteness is destroy'd, and the Blackness generated, only by a Change of Texture, made in the burnt Body, by the Recess of some parts and the Transposition of others. And though I remember not that in many Distillations of Harts-horn I ever found the *Cap. Mort.* to pass from Black to a true Whiteness, whilst it continu'd in Clos'd vessels, yet having taken out the Cole-black fragments, and Calcin'd them in

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Open vessels, I could in few hours quite destroy that Blackness, & without / sensibly changing their Bulk or Figure, reduce them to great Whiteness. So much do these two Colours depend upon the Disposition of the little parts, that the Bodies wherein they are to be met with do consist of. And we find, that if Whitewine Tartar, or even the white Crystals of such Tartar be burnt without being truly Calcin'd, the *Cap. Mortuum* (as the Chymists call the more Fixt part) will be Black. But if you further continue the Calcination till you have perfectly Incinerated the Tartar, & kept it long enough in a Strong fire, the remaining *Calx* will be White. And so we see that not only other Vegetable substances, but even White woods, as the Hazel, will yield a Black Charcoal, and afterwards Whitish ashes; And so Animal substances naturally White, as Bones and Eggshells, will grow Black upon the being Burnt, and White again when they are perfectly Calcin'd.

### *Experiment IV.*

But yet I much Question whether that Rule delivered by divers, as well Philosophers as Chymists, *adusta nigra, sed perusta alba*,<sup>†\*</sup> will hold as Universally as is presum'd, since I have several Examples to allege against / it: For I have found that by burning Alabaster, so as both to make it appear to boyl almost like Milk, and to reduce it to a very fine Powder, it would not at all grow Black, but retain its Pure and Native Whiteness, and though by keeping it longer than is usual in the fire, I produc'd but a faint Yellow, even in that part of the Powder that lay nearest the top of the Crucible, yet having purposely enquired of an Experienc'd Stone-cutter, who is Curious enough in trying Conclusions in his own Trade, he told me he had found that if Alabaster or Plaister of Paris be very long kept in a Strong fire, the whole heap of burnt Powder would exchange its Whiteness for a much deeper Colour than the Yellow I observ'd. Lead being Calcin'd with a Strong fire turns (after having perhaps run thorough divers other Colours) into *Minium*, whose Colour we know is a deep Red; and if you urge this *Minium*, as I have purposely done with a Strong fire, you may much easier find a Glassie and Brittle Body darker than *Minium*, than any white *Calx* or Glass. 'Tis known among Chymists, that the white *Calx* of Antimony, by the further and more vehement operation of the fire may be melted into Glass, which we have obtain'd of a Red Colour, which is / far deeper than that of the *Calx* of Burnt Antimony; and though common Glass of Antimony being usually Adulterated with *Borax*, have its Colour thereby diluted, oftentimes to a very pale Yellow; yet not onely ours made more sincerely, was, as we said, of a Colour less remote from Black, than was the *Calx*; but we observ'd, that by Melting it once or twice more, and so exposing it to the further operation of the Fire, we had, as we expected, the Colour heightned. To which we shall add but this one Instance, (which is worth the taking notice of in Reference

to Colours:) That, if you take Blew, but Unsophisticated, Vitriol, and burn it very Slowly, and with a Gentle degree of Heat, you may observe, that when 'tis Burnt but a Little, and yet so far as that you may rub it to Powder betwixt your fingers, it will be of a White or Whitish Colour; But if you Prosecute the Calcination, this Body which by a light Adustion was made White, will pass through other Colours, as Gray, Yellowish, and Red; and if you further burn it with a Long and Vehement fire, by that time it comes to be *Perustum*,<sup>†\*</sup> it will be of a dark Purple, nearer to Black, not only than the first *Calx*, but than the Vitriol before it at all felt the fire. I might add that *Crocus / Martis* (*per se* as they call it) made by the Lasting violence of the Reverberated flames is not so near a Kin to White, as the Iron or Steel that afforded it was before its Calcinations; but that I suppose, these Instances may suffice to satisfy you, that Minerals are to be excepted out of the fore-mentioned Rule, which perhaps, though it seldome fail in substances belonging to the Vegetable or Animal Kingdome, may yet be Question'd even in some of these, if that be true, which the Judicious Traveller *Bellonius* affirms,<sup>†\*</sup> that Charcoales made out of the Wood of *Oxycæder* are White; And I could not find, that though in Retorts Harts-horn and other White Bodies will be Denigrated by Heat, yet Camphire would not at all lose its Whiteness, though I have purposely kept it in such a heat, as made it melt and boyl.<sup>†\*</sup> /

### ***Experiment V.***

And now I speak of Camphire, it puts me in mind of adding this Experiment, That, though as I said in Clos'd Glasses, I could not Denigrate it by Heat, but it would Sublime to the sides and top of the Glass, / as it was before, yet not only it will, being set on fire in the Free Air, send forth a Copious smoak, but having purposely upon some of it that was Flaming, clapt a Large Glass, almost in the forme of a Hive, (but more Slender only) with a Hole at the top, (which I caus'd to be made to trye Experiments of Fire and Flame in) it continued so long burning that it Lin'd all the Inside of the Glass with a Soot as Black as Ink, and so Copious, that the Closeness of the Vessel consider'd, almost all that part of the White Camphire that did take Fire, seem'd to have been chang'd into that deep Black Substance.

### ***Experiment VI.***

And this also brings into my mind another Experiment that I made about the production of Blackness, whereof, for Reasons too long to be here deduced,

I expected and found a good Success, an it was this: I took Rectifi'd Oyl of Vitriol (that I might have the Liquor Clean as well as Strong) and by degrees mixt with it a convenient proportion of the Essential Oyl, as Chymists call it, of Wormwood, drawn over with store of Water in a Limbec, and warily Distilling the mixture in a Retort, there remain'd / a scarce credible quantity of dry Matter, Black as a Coal. And because the Oyl of Wormwood, though a Chymical Oyl drawn by a *Virtuoso*, seem'd to have somewhat in it of the Colour of the Plant, I Substituted in its Room, the Pure and Subtile Essential Oyl of Winter-Savory, and mixing little by little this Liquor, with (if I misremember not) an Equal weight of the formerly mention'd Rectifi'd Oyl of Vitriol, and Distilling them as before in a Retort, besides what there pass'd over into the Receiver, even these two clear Liquors left me a Considerable Proportion, (though not so great as the two former) of a Substance Black as Pitch, which I yet Keep by me as a Rarity.

### ***Experiment VII.***

A way of Whiting Wax Cheaply and in Great Quantity may be a thing of good Oeconomical Use, and we have elsewhere set down the Practice of Trades-men that Blanch it; But here Treating of Whiteness only in Order to the Philosophy of Colours, I shall not Examine which of the Slow wayes may be best Employ'd, to free Wax from the Yellow Melleous parts, but shall rather set down a Quick / way of making it White, though but in very Small Quantities. Take then a little Yellow Wax, scraped or thinly sliced, and putting it into a Bolts-head or some other Convenient Glass, pour to it a pretty deal of Spirit of Wine, and placing the Vessel in Warm Sand, Encrease the Heat by degrees, till the Spirit of Wine begin to Simper or to Boyl a little; and continuing that degree of Fire, if you have put Liquor enough, you will quickly have the Wax dissolv'd, then taking it off the fire, you may either suffer it to Cool as hastily as with Safety to the Glass you can, or Pour it whilst 'tis yet Hot into a Filtre of Paper, and either in the Glass where it Cools, or in the Filtre, you will soon find the Wax and *Menstruum* together reduc'd into a White Substance, almost like Butter, which by letting the Spirit Exhale will shrink into a much Lesser Bulk, but still retaining its Whiteness. And that which is pretty in the working of this Magistry of Wax, is, that the Yellowness vanishes, neither appearing in the Spirit of Wine that passes Limpid through the Filtre, nor in the Butter of Wax, if I may so call it, that, as I said, is White. /

### *Experiment VIII.*

There is an Experiment, *Pyrophilus*, which though I do not so exactly remember, and though it be somewhat Nice to make, yet I am willing to Acquaint You with, because the thing Produc'd, though it be but a Curiosity, is wont not a little to please the Beholders, and it is a way of turning by the

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help of a Dry Substance, an almost Golden-Colour'd Concrete, into a White one, the several Tryals are not at present so fresh in my Memory to enable me to tell you Certainly, whether an Equal onely or a Double weight of Common Sublimate must be taken in reference to the Tinglass, but if I mistake not, there was in the Experiment that succeeded best, Two parts of the Former taken to One of the Latter.<sup>†\*</sup> These Ingredients being finely Powdred and Exactly mix'd, we Sublim'd together by degrees of fire (the due Gradation of which is in this Experiment a thing of main Importance) there ascended a matter of a very peculiar Texture, for it was for the most part made up of very Thin, Smooth, Soft and Slippery Plates, almost like the finest sort of the Scales of Fishes, but of so Lovely a White Inclining to / Pearl-Colour, and so Curious and Shining a Gloss, that they appear'd in some respect little Inferiour to Orient Pearls, and in other Regards, they seem'd to Surpass them, and were Applauded for a sort of the Prettiest Trifles that we had ever prepar'd to Amuse the Eye. I will not undertake that though you'l hardly miss changing the Colour of your shining Tin-glass, yet you will the first or perhaps the second time hit Right upon the way of making the Glistring Sublimate I have been mentioning.

### *Experiment IX.*

When we Dissolve in *Aqua Fortis* a mixture of Gold and Silver melted into one Lump, it usually happens that the Powder of Gold that falls to the bottom, as not being Dissoluble by that *Menstruum*, will not have its own Yellow, but appear of a Black Colour, though neither the Gold, nor the Silver, nor the *Aqua Fortis* did before manifest any Blackness. And divers Alchymists, when they make Solutions of Minerals they would Examine, are very Glad, if they see a Black Powder Præcipitated to the Bottom, taking it for a Hopefull Sign, that those Particles are of a Golden Nature, / which appear in a Colour so ordinary to Gold parted from other Metalls by *Aqua Fortis*, that it is a trouble to the Refiner to Reduce the Præcipitated *Calx* to its Native Colour. For though, (as we have try'd,) that may be Quickly enough done by Fire, which will make this Gold look very Gloriously (as indeed 'tis at least one of the Best wayes that is Practis'd for the Refining of Gold,) yet it requires both Watchfulness and Skill, to give it such a Degree of Fire as will serve to



Restore it to its Lustre, without giving it such a One, as may bring it to Fusion, to which the Minuteness of the *Corpuscles* it consists of makes the Powder very apt. And this brings into my Mind, that having taken a Flat and Bright piece of Gold, that was Refin'd by a Curious and Skilfull Person on purpose to Trye to what height of Purity Gold could be brought by Art, I found that this very piece, as Glorious as it look'd, being rubb'd a little upon a piece of fine clean Linnen, did sully it with a kind of

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Black; and the like I have observ'd in Refin'd Silver, which I therefore mention, because I formerly suspected that the Impurity of the Metall might have been the only Cause of what I have divers times observ'd in wearing Silver-hilted Swords, Namely, that / where they rubb'd upon my Clothes, if they were of a Light-Colour'd Cloath, the Affriction would quickly Black them; and Congruously hereunto I have found Pens Blackt almost all over, when I had a while carri'd them about me in a Silver Ink-case. To which I shall only add, that whereas in these several Instances of Denigration, the Metalls are worn off, or otherwise Reduc'd into very Minute Parts, that Circumstance may prove not Unworthy your Notice.

### *Experiment X.*

That a Solution of Silver does Dye Hair of a Black Colour, is a Known Experiment, which some persons more Curious than Dextrous, have so Unluckily made upon themselves as to make their Friends very Merry. And I remember that the other day, I made my self some Sport by an Improvement of this Observation, for having dissolv'd some Pure Silver in *Aqua Fortis*, and Evaporated the *Menstruum ad siccitatem*,<sup>†\*</sup> as they speak, I caus'd a Quantity of fair Water to be pour'd upon the *Calx* two or three several times, and to be at each Evaporated, till the *Calx* was very Drye, and all the Greenish Blewness that is wont to appear in Common Crystals of Silver, / was quite carry'd away. Then I made those I meant to Deceive, Moisten some part of their Skin with their own Spittle, and slightly Rub the moistned parts with a little of this Prepar'd Silver; Whereupon they Admir'd to see, that a Snow-white Body laid upon the White Skin should presently produce a deep Blackness, as if the stains had been made with Ink, especially considering that this Blackness could not, like that produc'd by ordinary Ink, be readily Wash'd off, but requir'd many Hours, and part of it some dayes to its Obliteration. And with the same White *Calx* and a little Fair Water we likewise Stain'd the White Hafts of Knives, with a lasting Black in those parts where the *Calx* was Plentifully enough laid on, for where it was laid on but very Thinly, the Stain was not quite of so Deep a Colour.

### *Experiment XI.*

The Cause of the Blackness of those many Nations, which by one common Name we are wont to call *Negroes*, has been long since Disputed of by Learned Men, who possibly had not done amiss, if they had also taken into Consideration, why some whole races of other Animals besides Men, as / Foxes and Hares, are Distinguish'd by a Blackness not familiar to the Generality of Animals of the same Species; The General Opinion (to be mention'd a little lower) has been rejected even by some of the Antient

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Geographers, and among the Moderns *Ortelius* and divers other Learned Men have Question'd it.<sup>†\*</sup> But this is no place to mention what thoughts I have had to and fro about these Matters: Only as I shall freely Acknowledge, that to me the Enquiry seems more Abstruse than it does to many others, and that because consulting with Authors, and with Books of Voyages, and with Travellers, to satisfie my self in matters of Fact, I have met with some things among them, which seem not to

agree very well with the Notions of the most Classick Authors concerning these things; for it being my Present Work to deliver rather matters Historical than Theorys, I shall Annex some few of my Collections, instead of a Solemn Disputation. It is commonly presum'd that the Heat of the Climates wherein they live, is the reason, why so many Inhabitants of the Scorching Regions of *Africa* are Black; and there is this familiar Observation to Countenance this Conjecture, That we plainly see that Mowers, Reapers, and other Countrey-people, / who spend the most part of the Hot Summer dayes expos'd to the Sun, have the skin of their Hands and Faces, which are the parts immediately Expos'd to the Sun and Air, made of a Darker Colour than before, and consequently tending to Blackness; And Contrarywise we observe that the *Danes* and some other people that Inhabit Cold Climates, and even the *English* who feel not so Rigorous a Cold, have usually Whiter faces than the *Spaniards*, *Portugalls* and other European Inhabitants of Hotter Climates. But this Argument I take to be far more Specious than Convincing; for though the Heat of the Sun may Darken the colour of the Skin, by that Operation, which we in *English* call Sun-burning; yet Experience doth not Evince, that I remember, That that Heat alone can produce a Discolouring that shall amount to a true Blackness, like that of *Negroes*, and we shall see by and by that even the Children of some *Negroes* not yet 10. dayes Old (perhaps not so much by three quarters of that time) will notwithstanding their Infancy be of the same Hue with their Parents. Besides, there is this strong Argument to be alleg'd against the Vulgar Opinion, that in divers places in *Asia* under the same Parallel, or even of the same / Degree of Latitude with the *African* Regions Inhabited by Blacks, the People are at most but Tawny;<sup>†@</sup> And in *Africa* it self divers Nations in the Empire of *Ethiopia* are not *Negroes*, though Situated in the Torrid Zone, and as neer the Æquinoc'tial, as other Nations that are so (as the Black Inhabitants of *Zeylan* and *Malabar* are not in our Globes plac'd so near the Line as *Amara* the Famousest place in *Ethiopia*.) Moreover, (that which is of no small Moment in our present Disquisition) I find not by the best Navigators and

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Travellers to the *West-Indies*, whose Books or themselves I have consulted on this Subject, that excepting perhaps one place or two of small extent, there are any Blacks Originally Natives of any part of *America* (for the Blacks now there have been by the *Europeans* long Transplanted thither) though the New World contain in it so great a Variety of Climates, and particularly reach quite Cross the Torrid Zone from one Tropick to another.

And though it be true that the *Danes* be a Whiter People than the *Spaniards*, yet that may proceed rather from other causes (not here to be enquired into) than from the Coldness of the Climate, since not onely the / *Swedes* and other Inhabitants of those Cold Countreys, are not usually so White as the *Danes*, nor Whiter than other Nations in proportion to their Vicinity to the Pole. [And since the Writing of the former part of this Essay, having an opportunity on a Solemn occasion to take Notice of the Numerous Train of some Extraordinary Embassadors sent from the *Russian* Emperour to a great Monarch, observ'd, that (though it were then Winter) the Colour of their Hair and Skin was far less Whitish than the *Danes* who Inhabit a milder Region is wont to be, but rather for the most part of a Darkish Brown; And the Physician to the Embassadour with whom those *Russes* came, being ask'd by me whether in *Muscovy* it self the Generality of the People were more inclin'd to have Dark-colour'd Hair than Flaxen, he answer'd Affirmatively; but seem'd to suspect that the True and Antient *Russians*, a Sept of whom he told me he had met with in one of the Provinces of that vast Empire, were rather White like the *Danes*, than any thing near so Brown as the present *Muscovites* whom he guesses to be descended of the *Tartars*, and to have inherited their Colour from them.<sup>†\*</sup>] But to Prosecute our former Discourse, I shall add for further Proof of the Conjecture I was / countenancing that good Authors inform us that there are *Negroes* in *Africa* not far from the *Cape of good Hope*, and consequently beyond the Southern Tropick, and without the Torrid Zone, much about the same Northern Latitude (or very little more) wherein there are divers *American* Nations that are not *Negroes*, and wherein the Inhabitants of *Candia*, some parts of *Sicily*, and even of *Spain* are not so much as Tawny-Mores. But (which is a fresh and strong Argument against the common Opinion,) I find by our recent Relations of *Greenland*, (our Accounts whereof



we owe to the Curiosity of that Royal *Virtuoso* the present King of *Denmark*,) that the Inhabitants are Olive-colour'd, or rather of a Darker Hiew. But if the Case were the same with Men, and those other kinds of Animals I formerly nam'd, I should offer something as a considerable proof,

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That, Cold may do much towards the making Men White or Black, and however I shall set down the Observation as I have met with it, as worthy to come into the History of Whiteness and Blackness, and it is, that in some parts of *Russia* and of *Livonia* it is affirm'd by *Olaus Magnus* and others,<sup>†\*</sup> that Hares and Foxes (some add Partridges) which before were Black, or Red, or / Gray, do in the depth of Winter become White by reason of the great Cold; (for that it should be, as some conceive, by Looking upon the Snow, seems improbable upon divers accounts).

And I remember that having purposely enquir'd of a *Virtuoso* who lately Travell'd through *Livonia* to *Mosco*, concerning the Truth of this Tradition, he both told me, he believ'd it, and added, that he saw divers of those lately nam'd Animals either in *Russia* or *Livonia* (for I do not very well remember whether of the two) which, though White when he saw them in Winter, they assur'd him had been Black, or of other Colours before the Winter began, and would be so again when it was over. But for further satisfaction, I also consulted one that had for some years been an Eminent Physician in *Russia*,<sup>†\*</sup> who though he rejected some other Traditions that are generally enough believ'd concerning that Countrey, told me nevertheless, that he saw no cause to doubt of this Tradition of *Olaus Magnus* as to Foxes and Hares, not onely because 'tis the common and uncontroul'd Assertion of the Natives, but also because he himself in the Winter could never that he remember'd see Foxes and Hares of any other Colour than White; And I my self having seen a small White / Fox brought out of *Russia* into *England* towards the latter end of Winter, foretold those that shew'd him me, that he would change Colour in Summer, and accordingly coming to look upon him again in *July*, I found that the Back and Sides, together with the upper part of the Head and Tayl were already grown of a Dark Colour, the lower part of the Head and Belly containing as yet a Whiteness. Let me add, that were it not for some scruple I have, I should think more than what *Olaus* relates, confirm'd by the Judicious *Olearius*,<sup>†\*</sup> who was twice employ'd into those parts as a Publick Minister, who in his Account of *Moscovy* has this Passage: *The Hares there are Gray; but in some Provinces they grow white in the Winter.* And within some few Lines after: *It is not very Difficult to find the Cause of this Change, which certainly proceeds only from the Outward Cold, since I know that even in Summer, Hares will change Colour, if they be kept a competent time in a Cellar;* I say, were it not for some Scruple, because I take notice, that in the same Page the Author Affirms, that the like change of Colour that happens to Hares in some Provinces of *Muscovy*, happens to them also in *Livonia*, and yet immediately subjoyns, that in *Curland* the Hares vary not their Colour in Winter, / though these two last named Countries be contiguous, (that is) sever'd only by the river of *Dugna*; For it is scarce conceivable how Cold alone should have, in Countries so near, so strangely differing an operation, though no less strange a thing is confess'd by many, that

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ascribe the Complexion of *Negroes* to the Heat of the Sun, when they would have the River of *Cenega* so to bound the *Moors*, that though on the North-side they are but Tawny, on the other side they are Black.<sup>†\*</sup>

There is another Opinion concerning the Complexion of *Negroes*, that is not only embrac'd by many of the more Vulgar Writers, but likewise by that ingenious Traveller Mr. *Sandys*,<sup>†\*</sup> and by a late most learned Critick, besides other men of Note, and these would have the Blackness of *Negroes* an effect of *Noah's Curse* ratify'd by God's, upon *Cham*; But though I think that even a Naturalist may without disparagement believe all the Miracles attested by the Holy Scriptures, yet in this case to flye to a Supernatural Cause, will, I fear, look like Shifting off the Difficulty, instead of Resolving it; for we enquire not the First and Universal, but the Proper, Immediate, and Physical Cause of the Jetty Colour of *Negroes*; And not only we do not find expressed in the /

Scripture, that the Curse meant by *Noah* to *Cham*,<sup>†</sup> was the Blackness of his Posterity, but we do find plainly enough there that the Curse was quite another thing, namely, that he should be a Servant of servants, that is by an Ebraism, a very Abject Servant to his Brethren, which accordingly did in part come to pass, when the *Israelites* of the posterity of *Sem*, subdued the *Canaanites*, that descended from *Cham*, and kept them in great Subjection. Nor is it evident that Blackness is a Curse, for Navigators tell us of Black Nations, who think so much otherwise of their own condition, that they paint the Devil White. Nor is Blackness inconsistent with Beauty, which even to our European Eyes consists not so much in Colour, as an Advantageous Stature, a Comely Symmetry of the parts of the Body, and Good Features in the Face. So that I see not why Blackness should be thought such a Curse to the *Negroes*, unless perhaps it be, that being wont to go Naked in those Hot Climates, the colour of their Skin does probably, according to the Doctrine above deliver'd, make the Sun-beams more Scorching to them, than they would prove to a people of a White Complexion.

Greater probability there is, That the Principal Cause (for I would not exclude / all concurrent ones) of the Blackness of *Negroes* is some **Peculiar and Seminal** Impression, for not onely we see that *Blackmore* boyes brought over into these Colder Climates lose not their Colour; But good Authors inform us, That the Off-spring of *Negroes* Transplanted out of *Africa*, above a hundred years ago, retain still the Complexion of their Progenitors, though possibly in Tract of time it will decay; As on the other side, the White people removing into very Hot Climates, have their Skins by the Heat of the Sun scorch'd into Dark Colours; yet neither they, nor their Children have been observ'd, even in the Countreys of *Negroes*, to descend to a Colour amounting to that of the Natives; whereas I remember I have Read in *Pisos* excellent account of *Brasile*,<sup>†@</sup> that betwixt the *Americans* and *Negroes* are generated a distinct sort of Men, which they call *Cabocles*, and betwixt *Portugalls* and *Æthiopian* women, He tells us, he has sometimes seen Twins, whereof one had a White skin, the other a Black; not to mention here some other instances, he gives, that the productions of the mixtures of differing people, that is (indeed,) the **effects of Seminal** Impressions which they / consequently argue to have been their Causes; and we shall not much scruple at this, if we consider, that even Organical parts may receive great Differences from such peculiar Impressions, upon what account soever they came to be settled in the first Individual persons, from whom they are Propagated to Posterity, as we see in the Blobber-Lips and Flat-Noses of most Nations of *Negroes*. And if we may Credit what Learned men deliver concerning the Little Feet of the *Chinesses*, the *Macrocephali* taken notice of by *Hippocrates*,<sup>†\*</sup>

will not be the only Instance we might apply to our present purpose.

And on this occasion it will not perchance be Impertinent to add something of what I have observ'd in other Animals, as that there is a sort of Hens that want Rumps; And that (not to mention that in several places there is a sort of Crows or Daws that are not Cole-black as ours, but partly of a Whitish Colour) in spite of *Porphyries* examples of Inseparable Accidents,<sup>†\*</sup> I have seen a perfectly White Raven, as to Bill as well as Feathers, which I attentively consider'd, for fear of being impos'd upon. And this recalls into my Memory, what a very Ingenious Physician has divers times related to me of a young Lady, to whom being call'd, he found that though / she much complain'd of want of Health, yet there appear'd so little cause either in her Body, or her Condition to Guess that She did any more than fancy her self Sick, that scrupling to give her Physick, he perswaded her Friends rather to divert her Mind by little Journeys of Pleasure, in one of which going to Visit St. *Winifrids* Well, this Lady, who was a *Catholick*, and devout in her Religion, and a pretty while in the Water to perform some Devotions, and had occasion to fix her Eyes very attentively upon the Red pipples-stones, which in a scatter'd order made up a good part of those that appear'd through the water, and a while after growing Bigg, she was deliver'd of a Child, whose

White Skin was Copiously speckl'd with spots of the Colour and Bigness of those Stones, and though now this Child have already liv'd several years, yet she still retains them. I have but two things to add concerning the Blackness of *Negroes*, the one is, that the Seat of that Colour seems to be but the thin *Epidermes*, or outward Skin, for I knew a young *Negroe*, who having been lightly Sick of the Small Pox or Measles, (for it was doubted which of the two was his Disease) I found by enquiry of a person that was concern'd for him, that in those places where the little Tumors / had broke their passage through the Skin, when they were gone, they left Whitish specks behind them; And the lately commended *Piso* assures us, that having the opportunity in *Brasil* to Dissect many *Negroes*, he cleerly found that their Blackness went no deeper than the very outward Skin, which *Cuticula* or *Epidermis* being remov'd, the undermost Skin or *Cutis* appear'd just as White as that of *Europæan* Bodyes. And the like has been affirmed to me by a Physician of our own, whom, hearing he had Dissected a *Negroe* here in *England*, I consulted about this particular.<sup>†\*</sup> The other thing to be here taken notice of concerning *Negroes* is, That having enquir'd of an Intelligent acquaintance of mine (who keeps in the *Indies* about 300. of them as well Women as Men to work in his Plantations,) whether their Children come Black into the world; he answer'd, That they did not, but were brought forth of almost the like Reddish Colour with our *European* Children; and having further enquir'd, how long it was before these Infants appear'd Black, he reply'd, that 'twas not wont to be many daies. And agreeable to this account I find that, given us in a freshly publish'd

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French Book written by a *Jesuit*, that had good opportunity / of Knowing the Truth of what he Delivers, for being one of the Missionaries of his Order into the Southern *America* upon the Laudable Design of Converting Infidels to Christianity, he Baptiz'd several Infants, which when newly Born, were much of the same Colour with *European* Babes, but within about a Week began to appear of the Hue of their Parents.<sup>†\*</sup> But more Pregnant is the Testimony of our Countrey-man *Andrew Battel*, who being sent Prisoner by the

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*Portugalls* to *Angola*, liv'd there, and in the adjoining Regions, partly as a Prisoner, partly as a Pilot, and partly as a Souldier, near 18. years, and he mentioning the *African* Kingdom of *Longo*, peopl'd with Blacks, has this passage: *The Children in this Countrey are Born White, and change their Colour in two dayes to a Perfect Black.*<sup>†@</sup> As for Example, *The Portugalls which dwell in the Kingdome of Longo have sometimes Children by the Negroe-women, and many times the Fathers are deceived, thinking, when the Child is Born, that it is theirs, and within two dayes it proves the Son or Daughter of a Negroe, which the Portugalls greatly grieve at; And the same person has elsewhere a Relation, which, if he have made no use at all of the / liberty of a Traveller, is very well worth our Notice, since this, together with that we have formerly mention'd of Seminal Impressions, shews a possibility, that a Race of Negroes might be begun, though none of the Sons of Adam for many Precedent Generations were of that Complexion.*

For I see not why it should not be at least as possible, that White Parents may sometimes have Black Children, as that *African Negroes* should sometimes have lastingly White ones, especially since concurrent causes may easily more befriend the Productions of the Former kind, than under the scorching Heat of *Africa* those of the Latter. And I remember on the occasion of what he delivers, that of the White Raven formerly mention'd, the Possessor affirm'd to me, that in the Nest out of which he was taken White, they found with him but one other Young one, and that he was of as Jetty a Black as any common Raven. But let us hear our Author himself:<sup>†@</sup> *Here are* (sayes he, speaking of the formerly mention'd Regions) *Born in this Countrey White Children, which is very rare among them, for their Parents are Negroes; And when any of them are Born, they are presented to the King, and are call'd Dondos; These are as White as any / White Men. These are the Kings Witches, and are brought up in Witchcraft, and alwayes wait on the King: There is no man that dare meddle with these Dondos, if they go to the Market they may take what they list, for all Men stand in awe of them. The King of Longo hath four of them.* And yet this Countrey in our



Globes is plac'd almost in the midst of the Torrid Zone (four or five Degrees Southward of the Line.) And our Author elsewhere tells us of the Inhabitants, that they are so fond of their Blackness, that they will not suffer any that is not of that Colour (as the *Portugalls* that come to Trade thither) to be so much as Buri'd in their Land, of which he

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annexes a particular example,<sup>†@</sup> that may be seen in his Voyage preserv'd by our Industrious Countreyman Mr. *Purchas*.<sup>†\*</sup> But it is high time for me to dismiss Observations, and go on with Experiments.

### *Experiment XII.*

The way, *Pyrophilus*, of producing Whiteness by Chymical Præcipitations is very well worth our observing, for thereby Bodyes of very Differing Colours as well as Natures, though dissolv'd in Several Liquors, / are all brought into *Calces* or Powders that are White. Thus we find that not only Crabs-eyes, that are of themselves White, and Pearls that are almost so, but *Coral* and *Minium* that are Red, being dissolv'd in Spirit of Vinegar, may be uniformly Præcipitated by Oyl of *Tartar* into White Powders. Thus Silver and Tin separately dissolv'd in *Aqua Fortis*, will the one Præcipitate it self, and the other be Præcipitated by common Salt-water into a White *Calx*, and so will Crude Lead and Quicksilver first dissolv'd likewise in *Aqua Fortis*. The like *Calx* will be afforded as I have try'd by a Solution of that shining Mineral Tinglass dissolv'd in *Aqua Fortis*, and Præcipitated out of it; and divers of these *Calces* may be made at least as Fair and White, if not better Colour'd, if instead of Oyl of *Tartar* they were Præcipitated with Oyl of *Vitriol*, or with another Liquor I could Name. Nay, that Black Mineral *Antimony* it self, being reduc'd by and with the Salts that concur to the Composition of common Sublimate, into that Cleer though Unctuous Liquor that Chymists commonly call Rectifi'd Butter of *Antimony*, will by the bare affusion of store of Fair Water be struck down into that Snow-white Powder, which when the adhering Saltness is

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well wash'd / off, Chymists are pleas'd to call *Mercurius Vitæ*, though the like Powder may be made of *Antimony*, without the addition of any *Mercury* at all. And this Lactescence if I may so call it, does also commonly ensue when Spirit of Wine, being Impregnated with those parts of Gums or other Vegetable Concretions, that are supos'd to abound with Sulphureous Corpuscles, fair Water is suddenly pour'd upon the Tincture or Solution. And I remember that very lately I did, for Tryal sake, on a Tincture of *Benjamin* drawn with Spirit of Wine, and brought to be as Red as Blood, pour some fair Water, which presently mingling with the Liquor, immediately turn'd the whole Mixture White. But if such Seeming Milks be suffer'd to stand unstirr'd for a convenient while, they are wont to let fall to the bottome a Resinous Substance, which the Spirit of Wine Diluted and Weakned by the Water pour'd into it, was unable to support any longer. And something of Kin to this change of Colour in Vegetables is that, which Chymists are wont to observe upon the pouring of Acid Spirits upon the Red Solution of *Sulphur*, dissolv'd in an Infusion of Pot-ashes, or in some other sharp *Lixivium*, the Præcipitated *Sulphur* before it subsides, immediately turning the Red Liquor / into a White one. And other Examples might be added of this way of producing Whiteness in Bodyes by Præcipitating them out of the Liquors wherein they have been Dissolv'd; but I think it may be more usefull to admonish you, *Pyrophilus*, that this observation admits of Restrictions, and is not so Universal, as by this time perhaps you have begun to think it; For though most Præcipitated Bodyes are White, yet I know some that are not; For Gold Dissolv'd in *Aqua Regis*, whether you Præcipitate it with Oyl of *Tartar*, or with Spirit of *Sal Armoniack*, will not afford a White but a Yellow *Calx*. *Mercury* also though reduc'd into Sublimate, and Præcipitated with Liquors abounding with Volatile Salts, as the Spirits drawn from Urine, Harts-horn, and other Animal substances, yet will afford, as we Noted in our first Experiment about Whiteness and Blackness, a White Præcipitate, yet with some Solutions hereafter to be mentioned,<sup>†\*</sup> it will let fall

an Orange-Tawny Powder. And so will Crude *Antimony*, if, being dissolv'd in a strong Lye, you pour (as farr as I remember) any Acid Liquor upon the Solution newly Filtrated, whilst it is yet Warm. And if upon the Filtrated Solution of *Vitriol*, you pour a Solution of / one of these fix'd Salts, there will subside a Copious substance, very farr from having any Whiteness, which the Chymists are pleas'd to call, how properly I have elsewhere examin'd, the *Sulphur of Vitriol*. So that most part of Dissolv'd Bodyes being by Præcipitation brought to White Powders, and yet some affording Præcipitates of other Colours, the reason of both the Phænomena may deserve to be enquir'd into.

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### *Experiment XIII.*

Some Learned Modern Writers are of Opinion, that the Account upon which Whiteness and Blackness ought to be call'd, as they commonly are, the two Extreme Colours, is, †@ That Blackness (by which I presume is meant the Bodyes endow'd with it) receives no other Colours; but Whiteness very easily receives them all; whence some of them compare Whiteness to the *Aristotelian Materia prima*, that being capable of any sort of Forms, as they suppose White Bodyes to be of every kind of Colour. †\* But not to Dispute about Names or Expressions, the thing it self that is affirm'd as Matter of Fact, seems to be True enough in most Cases, not in all, or so, / as to hold Universally. For though it be a common observation among Dyers, That Clothes, which have once been throughly imbu'd with Black, cannot so well afterwards be Dy'd into Lighter Colours, the præexistent Dark Colour infecting the Ingredients, that carry the Lighter Colour to be introduc'd, and making it degenerate into some more Sad one; Yet the Experiments lately mention'd may shew us, that were the change of Colour in Black Bodies is attempted, not by mingling Bodyes of Lighter Colours with them, but by Addition of such things as are proper to alter the Texture of those Corpuscles that contain the Black Colour, 'tis no such difficult matter, as the lately mention'd Learned Men imagine, to alter the Colour of Black Bodyes. For we saw that Inks of several Kinds might in a trice be depriv'd of all their Blackness; and those made with Log-wood and Red-Roses might also be chang'd, the one into a Red, the other into a Reddish Liquor; and with Oyl of *Vitriol* I have sometimes turn'd Black pieces of Silk into a kind of Yellow, and though the Taffaty were thereby made Rotten, yet the spoyling of that does no way prejudice the Experiment, the change of Black Silk into Yellow, being never the less True, because / the Yellow Silk is the less good. And as for Whiteness, I think the general affirmation of its being so easily Destroy'd or Transmuted by any other Colour, ought not to be receiv'd without some Cautions and Restrictions. For whereas, according to what I formerly Noted, Lead is by Calcination turned into that Red Powder we call *Minium*; And Tin by Calcination reduc'd to a White *Calx*, the common Putty that is sold and us'd so much in Shops, instead of being, as it is pretended and ought to be, only the *Calx* of Tin, is, by the Artificers that make it, to save the charge of Tin, made, (as some of themselves have confess'd, and as I long suspected by the Cheap rate it may be bought for) but of half Tin and half Lead, if not far more Lead than Tin, and yet the Putty in spite of so much Lead is a very White Powder, without disclosing any mixture of *Minium*. And so if you take two parts of Copper, which is a High-colour'd Metall, to but one of Tin, you may by Fusion bring them into one Mass, wherein the Whiteness of the Tin

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is much more Conspicuous and Predominant than the Reddishness of the Copper. And on this occasion it may not be Impertinent to mention an Experiment, which I relate upon the Credit of a very Honest man, / whom I purposely enquir'd of about it, being my self not very fond of making Tryals with *Arsenick*, the Experiment is this, That if you Colliquate *Arsenick* and Copper in a due proportion, the *Arsenick* will Blanch the Copper both within and without, which is an Experiment well enough Known; but when I enquir'd, whether or no this White mixture being skilfully kept a while upon the Cupel would not let go its *Arsenick*, which made Whiteness its prædominant Colour,



and return to the Reddishness of Copper, I was assur'd of the Affirmative; so that among Mineral Bodyes, some of those that are White, may be far more capable, than those I am reasoning with seem to have known, of Eclipsing others, and of making their Colour Prædominant in Mixtures. In further Confirmation of which may be added, that I remember that I also took a lump of Silver and Gold melted together, wherein by the Æstimate of a very Experienc'd Refiner, there might be about a fourth or third part of Gold, and yet the Yellow Colour of the Gold was so hid by the White of the Silver, that the whole Mass appear'd to be but Silver, and when it was rubb'd upon the Touchstone, an ordinary beholder could scarce have distinguish'd it from the Touch of common / Silver; though if I put a little *Aqua Fortis* upon any part of the White Surface it had given the Touchstone, the Silver in the moistned part being immediately taken up and conceal'd by the Liquor, the Golden Particles would presently disclose that native Yellow, and look rather as if Gold, than if the above mention'd mixture, had been rubb'd upon the Stone.

#### *Experiment XIV.*

I took a piece of Black-horn, (polish'd as being part of a Comb) this with a piece of broken glass I scrap'd into many thin and curdled flakes, some shorter and some longer, and having laid a pretty Quantity of these scrapings together, I found, as I look'd for, that the heap they compos'd was White, and though, if I laid it upon a clean piece of White Paper, its Colour seem'd somewhat Eclips'd by the greater Whiteness of the Body it was compar'd with, looking somewhat like Linnen that had been sulli'd by a little wearing, yet if I laid it upon a very Black Body, as upon a Beaver Hatt, it then appear'd to be of a good White, which Experiment, that you may in a trice make when you please, seems very much to Disfavour both their Doctrine / that would have Colours to flow from the substantial Forms of Bodyes, and that of the Chymists also, who ascribe them to one or other of their three Hypostatical Principles; for though in our Case there was so great a Change made, that the same Body without being substantially either Increas'd or Lessened, passes immediately from one extreme Colour to another (and that too from Black to White) yet this so great and sudden change is effected by a slight Mechanical Transposition of parts, there being no Salt or *Sulphur* or *Mercury*

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that can be pretended to be Added or Taken away, nor yet any substantial Form that can reasonably be suppos'd to be Generated and Destroy'd, the Effect proceeding only from a Local Motion of the parts which so vary'd their Position as to multiply their distinct Surfaces, and to Qualifie them to Reflect far more Light to the Eye, than they could before they were scrap'd off from the entire piece of Black horn.

#### *Experiment XV.*

And now, *Pyrophilus*, it will not be improper for us to take some notice of an Opinion touching the cause of Blackness, which I judg'd it not so seasonable to Question, till I / had set down some of the Experiments, that might justifie my dissent from it. You know that of late divers Learned Men, having adopted the three Hypostatical Principles, besides other Notions of the Chymists, are very inclinable to reduce all Qualities of Bodies to one or other of those three Principles, and Particularly assign for the cause of Blackness the Sootie steam of *adust* or *torrifi'd Sulphur*. But I hope that what we have deliver'd above to countenance the Opinion we have propos'd about the Cause of Blackness, will so easily supply you with several Particulars that may be made use of against this Opinion, that I shall now represent to You but two things concerning it.

And First it seems that the favourers of the Chymicall Theories might have pitcht upon some more proper term, to express the Efficient of Blackness than *Sulphur adust*; for we know that *common Sulphur*, not only when Melted, but even when Sublim'd, does not grow Black by suffering the

Action of the fire, but continues and ascends Yellow, and rather more than less White, than it was before its being expos'd to the fire. And if it be set on fire, as when we make that acid Liquor, that Chymists call *Oleum Sulphuris per campanam*, it affords / very little Soot, and indeed the flame yeelds so little, that it will scarce in any degree Black a sheet of White Paper, held a pretty while over the flame and smoak of it, which is observed rather to Whiten than Infect linnen, and which does plainly make Red Roses grow very Pale, but not at all Black, as far as the Smoak is permitted to reach the leaves. And I can shew you of a sort of fixt Sulphur made by an Industrious Laborant of your acquaintance, who assur'd me that he was wont to keep it for divers weeks together night and day in a naked and Violent fire, almost like that of the Glass-house, and when, to satisfie my Curiosity, I made him take out a lump of it, though it were glowing hot (and yet not melted,) it did not, when I had suffered it to cool, appear Black, the true Colour of it being a true Red. I know it may be said, that *Chymists* in the Opinion above recited mean the *Principle of Sulphur*, and not *common Sulphur* which receives its name, not from its being *all* perfectly of a Sulphureous Nature, but for that *plenty* and *Predominancy* of the Sulphureous Principle in it. But allowing this, 'tis easie to reply, that still according to this very Reason, torrifi'd Sulphur should afford more Blackness, than most other concretes, / wherein that Principle is confess'd to be far less copious. Also when I have expos'd Camphire to the fire in

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Close Vessels, as Inflamable, and consequently (according to the Chymists) as Sulphureous a Body as it is, I could not by such a degree of Heat, as brought it to Fusion, and made it Boil in the glass, impress any thing of Blackness, or of any other Colour, than its own pure White, upon this Vegetable concrete. But what shall we say to Spirit of Wine, which being made by a Chymical Analysis of the Liquor that affords it, and being totally Inflamable, seems to have a full right to the title they give it of *Sulphur Vegetabile*, & yet this fluid Sulphur not only contracts not any degree of Blackness by being often so heated, as to be made to Boil, but when it burns away with an Actual flame, I have not found that it would discolour a piece of White Paper held over it, with any discernable Soot. Tin also, that wants not, according to the Chymists, a *Sulphur Joviale*, when throughly burned by the fire into a *Calx*, is not Black, but eminently White. And I lately noted to you out of *Bellonius*, that the Charcoals of Oxy-cedar are not of the former of these two Colours, but of the latter.<sup>†\*</sup> And the Smoak of our Tin by coals here in *England*, has been / usually observ'd, rather to Blanch linnen than to Black it. To all which, other particulars of the like nature might be added, but I rather choose to put you in mind of the third Experiment, about making Black Liquors, or Ink, of Bodies that were non of them Black before. For how can it be said, that when those Liquors are put together actually Cold, and continue so after their mixture, there intervenes any new *Adustion of Sulphur* to produce the emergent Blackness? (and the same question will be appliable to the Blackness produc'd upon the blade of a Knife, that has cut Lemmons and some kind of Sowr apples, if the juyce (though both Actually and Potentially Cold) be not quickly wip'd off. And when by the instilling either of a few drops of Oyl of Vitriol as in the second Experiment, or of a little of the Liquor mention'd in the Passage pointed at in the fourth Experiment, (where I teach at once to Destroy one black Ink, and make another) the Blackness produc'd by those Experiments is presently destroy'd; if the Colour proceeded only from the Plenty of Sulphurous parts, torrify'd in the Black Bodies, I demand, what becomes of them, when the Colour so suddenly disappears? For it cannot Reasonably be said, that all those that / suffic'd to make so great a quantity of Black Matter, should resort to so very small a proportion of the Clarifying Liquor, (if I may so call it) as to be deluted by it, with out at all Denigrating it. And if it be said that the Instill'd Liquor dispers'd those Black Corpuscles, I demand, how that Dispersion comes to destroy their Blackness, but by making such a Local Motion of their parts, as destroys their former Texture? which may be a Matter of such moment in cases like ours, that I remember that I have in few houres, without addition, from Soot it self, attain'd pretty store of Crystalline Salt, and good store of Transparent Liquor, and (which I have on another occasion noted as remarkable) this so Black substance had its Colour so alter'd, by the change of Texture it receiv'd from the fire, wherewith it was distiill'd, that it did for a great

while afford such plenty of very white Exhalations, that the Receiver, though large, seem'd to be almost fill'd with Milk.

Secondly, But were it granted, as it is in some cases not Improbable, that divers Bodies may receive a Blackness from a Sootie Exhalation, occasion'd by the Adustion of their Sulphur, which (for the Reasons lately mention'd I should rather call their Oyly parts;) yet still this account / is applicable but to some Particular Bodies, and will afford us no General Theory of Blackness. For if, for example, White Harts-horn, being, in Vessels well luted to each other, expos'd to the fire, be said to turn Black by the Infection of its own Smoak, I think I may justly demand, what it is that makes the Smoak or Soot it self Black, since no such Colour, but its contrary, appear'd before in the Harts-horn? And with the same Reason, when we are told, that torrify'd Sulphur makes bodies Black, I desire to be told also, why Torefaction makes Sulphur it self Black? nor will there be any Satisfactory Reason assign'd of these Quæries, without taking in those Fertile as well as Intelligible Mechanical Principles of the Position and Texture of the Minute parts of the body in reference to the Light and the Eye; and these applicable Principles may serve the turn in many cases, where the Adustion of Sulphur cannot be pretended; as in the appearing Blackness of an Open window, lookt upon at a somewhat remote distance from the house, as also in the Blackness Men think they see in the Holes that happen to be in White linnen, or Paper of the like Colour; and in the Increasing Blackness immediatly Produc'd barely by so rubbing Velvet, / whose Piles were Inclined before, as to reduce them to a more Erected posture, in which and in many other cases formerly alleg'd, there appears nothing requisite to the Production of *the* Blackness, but the hindering of the incident Beams of Light from rebounding plentifully enough to the Eye. To be short, those I reason with, do concerning Blackness, what the Chymists are wont also to do concerning other Qualities, namely to content themselves to tell us, in what Ingredient of a Mixt Body, the Quality enquir'd after, does reside, instead of explicating the Nature of it, which (to borrow a comparison from their own Laboratories) is much as if in an enquiry after the cause of Salivation, they should think it enough to tell us, that the several Kinds of Præcipitates of Gold and *Mercury*, as likewise of Quick-silver and Silver (for I know that make and use of such Præcipitates also) do Salivate upon the account of the *Mercury*, which though Disguis'd abounds in them, whereas the Difficulty is as much to know upon what account *Mercury* it self, rather than other Bodies, has that power of working by Salivation. Which I say not, as though it were not *something* (and too often the most we can arrive at) to discover in which of the / Ingredients of a Compounded Body, the Quality, whose Nature is sought, resides, but because, though this Discovery it self may pass for *something*, and is oftentimes more than what is taught us about the same subjects in the Schools, yet we ought not to think it *enough*, when more Clear and Particular accounts are to be had./

THE  
Experimental History  
OF COLOURS  
Begun

**The Third Part.**  
**Containing Promiscuous Experiments About Colours.**

*Experiment I.*

Because that, according to the Conjectures I have above propos'd, one of the most General Causes of the Diversity of Colours in Opacous Bodyes, is, that some reflect the Light mingl'd with more, others with less of Shade (either / as to Quantity, or as to Interruption) I hold it not unfit to mention in the first place, the Experiments that I thought upon to examine this Conjecture. And though

coming to transcribe them out of some Physiological *Adversaria* I had written in loose Papers, I cannot find one of the chief Records I had of my Tryals of this Nature, yet the Papers that scap'd miscarrying, will, I presume, suffice to manifest the main thing for which I now allege them; I find then among my *Adversaria*, the following Narrative.

*October* the 11. About ten in the Morning in Sun-shiny Weather, (but not without fleeting Clouds) we took several sorts of Paper Stain'd, some of one Colour, and some of another; and in a Darken'd Room whose Window look'd Southward, we cast the Beams that came in at a hole about three Inches and a half in Diameter,<sup>†\*</sup> upon a White wall that was plac'd on one side, about five foot distance from them.

The White gave much the Brightest Reflection.

The Green, Red and Blew being Compar'd together, the Red gave much the strongest Reflection, and manifestly enough also threw its *Colour* upon the Wall; The Green and Blew were scarce Discernable / by their Colours, and seem'd to reflect an almost Equal Light.

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The Yellow compar'd with the two last nam'd, Reflected somewhat more Light.

The Red and Purple being Compar'd together, the former manifestly Reflected a good deal more Light.

The Blew and Purple compar'd together, the former seem'd to Reflect a little more Light, though the Purple Colour were more manifestly seen.

A Sheet of very well sleek'd Marbl'd paper being Apply'd as the others, did not cast any of its Distinct Colours upon the Wall; nor throw its Light upon it with an Equal Diffusion; but threw the Beams Unstain'd and Bright to this and that part of the Wall, as if it's Polish had given it the Nature of a specular Body. But comparing it with a sheet of White Paper, we found the Reflection of the latter to be much Stronger, it diffusing almost as much Light to a *good Extent* as the Marble Paper did to *one part* of the Wall.

The Green and Purple left us somewhat in suspence which Reflected the most Light; only the Purple seem'd to have some little Advantage over the Green, which was Dark in its kind.

Thus much I find in our above mention'd / *Collections*, among which there are also some Notes concerning the Production of *Compounded Colours*, by *Reflection* from Bodyes differinglly Colour'd. And these Notes we intended should supply us with what we should mention as our second Experiment: but having lost the Paper that contain'd the Particulars, and remembring onely in General, that if the Objects which Reflected the Light were not Strongly Colour'd and somewhat Glossy, the Reflected Beams would not manifestly make a Compounded Colour upon the Wall, and even then but very Faintly, we shall now say no more of that Matter, only reserving our selves to mention hereafter the Composition of a Green, which we still retain in Memory.

## ***Experiment II.***

We may add, *Pyrophilus*, on this Occasion, that though a Darken'd Room be Generally thought requisite to make the Colour of a Body appear by Reflection from another Body, that is not one of those that are commonly agreed upon to be Specular (as Polish'd Metall, Quick silver, Glass, Water, &c.), Yet I have often observ'd that when I wore Doublets Lin'd with some silken / Stuff that was very Glossy and Vividly Colour'd, especially Red, I could in an Inlightned Room plainly enough Discern the Colour, upon the Pure White Linnen that came out at my Sleeve and reach'd to my

Cufs; as if that Fine White Body were more Specular, than Colour'd and Unpolish'd Bodyes are thought Capable of being.

### ***Experiment III.***

Whilst we were making the newly mention'd Experiments, we thought fit to try also what Composition of Colours might be made by Altering the Light in its Passage to the Eye by the Interposition not of Perfectly Diaphanous

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Bodies, (that having been already try'd by others as well as by us, as we shall soon have occasion to take notice) but of Semi-opacous Bodyes, and those such as look'd upon in an ordinary Light, and not held betwixt it and the Eye, are not wont to be Discriminated from the rest of Opacous Bodyes; of this Tryal, our mention'd *Adversaria* present us the following Account.

Holding these Sheets, sometimes one sometimes the other of them, before the Hold betwixt the Sun and the Eye, with / the Colour'd sides obverted to the Sun; we found them *single* to be somewhat Transparent, and appear of the same Colour as before, onely a little alter'd by the great Light they were plac'd in; but laying *two* of them one over another and applying them so to the Hole, the Colours were compounded as follows.

The Blew and Yellow scarce exhibited any thing but a Darker Yellow, which we ascrib'd to the Coarseness of the Blew Papers, and its Darkness in its Kind. For applying the Blew parts of the Marbl'd Paper with the Yellow Paper after the same manner, they exhibited a good Green.

The Yellow and Red look'd upon together gave us but a Dark Red, somewhat (and but a little,) inclining to an Orange Colour.

The Purple and Red look'd on together appear'd more Scarlet.

The Purple and Yellow made an Orange.

The Green and Red made a Dark Orange Tawny.

The Green and Purple made the Purple appear more Dirty.

The Blew and Purple made the Purple more Lovely, and far more Deep.

The Red parts of the Marbl'd Paper look'd upon with the Yellow appear'd of a / Red far more like Scarlet than without it.

But the Fineness or Coarseness of the Papers, their being carefully or slightly Colour'd, and divers other Circumstances, may so vary the Events of such Experiments as these, that if, *Pyrophilus*, you would Build much on them, you must carefully Repeat them.

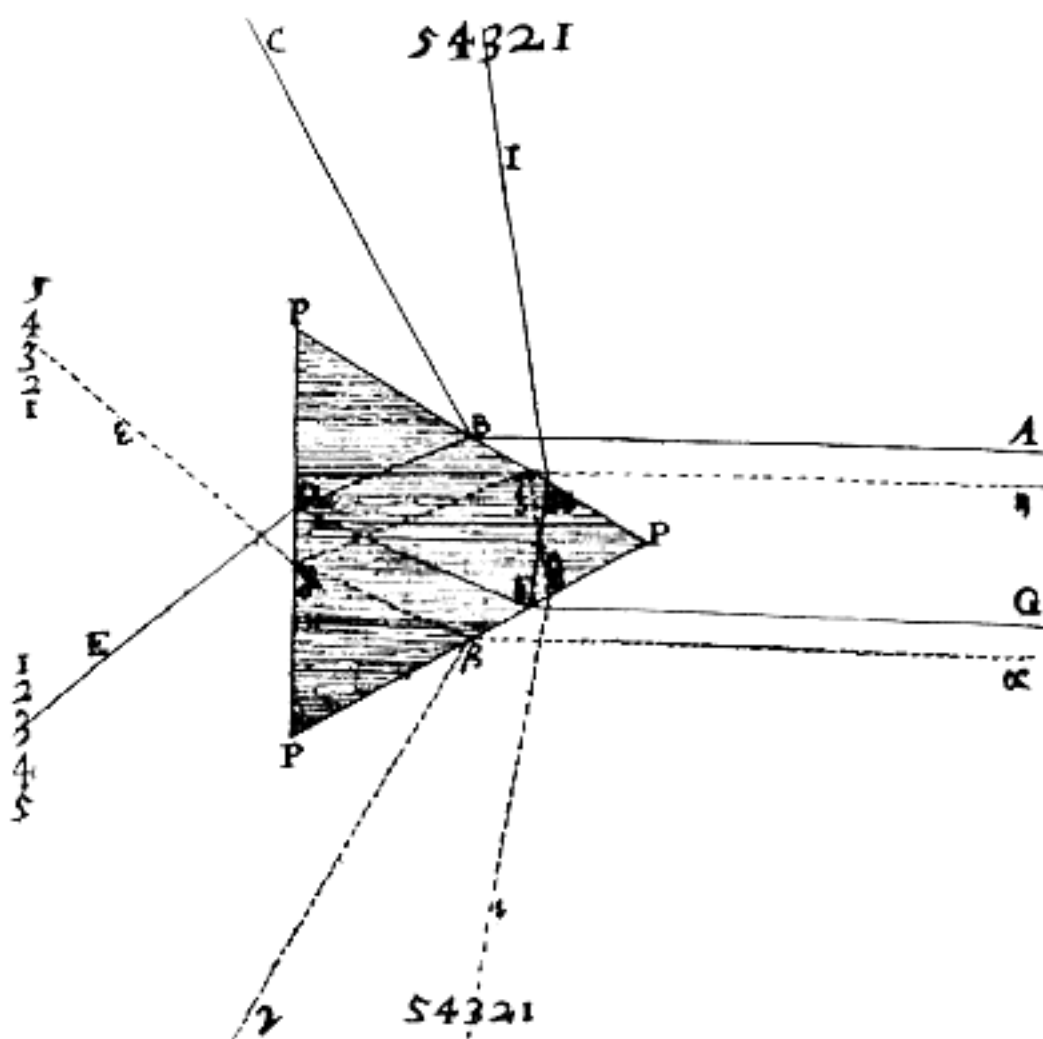
### ***Experiment IV.***

The Triangular Prismatical Glass being the Instrument upon whose Effects we may the most Commodiously speculate the Nature of Emphatical Colours, (and perhaps that of Others too;) we thought it might be usefull to observe the several Reflections and Refractions which the Incident Beams of Light suffer in Rebouncing from it, and Passing through it. And this we thought might be Best done, not (as is usual,) in an ordinary Inlightn'd Room, where (by reason of the Difficulty of doing otherwise) ev'n the Curious have left Particulars Unheeded, which may in a convenient place



be easily taken notice of; but in a Darken'd Room, where by placing the Glass in a convenient Posture, the Various Reflections and Refractions may be Distinctly observ'd; and where it may appear *what* Beams are Unting'd, / and *which* they are, that upon the Bodyes that terminate them, do Paint either the Primary or Secondary Iris. In pursuance of this we did in the above mention'd Darken'd Room, make observation of no less than four Reflections,

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*The Explication of the Scheme.*

*P P P.* An æquilaterotriangular Crystalline prism, one of whose edges *P*. is placed directly towards the Sun.

*A B & α β* Two rays from the Sun falling on the Prism at *B β*/ and thence partly reflected towards *C & γ*. and partly refracted towards *D & δ*.

*B C & β γ*. Those reflected Rays.

*B D & β δ*. Those refracted Rays which are partly refracted towards *E & ε*. and there paint an Iris 1 2 3 4 5. denoting the five consecutions of colours Red, Yellow, Green, Blew, and Purple; and are partly reflected towards *F & ζ*.

*D F & δ ζ*. Those Reflected Rays which are partly refracted towards *G & η*. colourless. and partly reflected, towards *H & θ*.

*F H & ζ θ*. Those reflected Rays which are refracted towards *I & ι*. and there paint an other fainter iris, the colours of which are contrary to the former 5 4 3 2 1. signifying Purple, Blew, Green, Yellow, Red, so that the Prism in this posture exhibits four Rainbows.

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and three Refractions that were afforded us by the same Prism, and thought that notwithstanding what was taught us by the Rules of Catoptricks and Dioptricks, it would not be amiss to find also, by hiding sometimes one part of the Prism, and sometimes another, and observing where the Light or Colour Vanish'd thereupon, by which Reflection and by which Refraction each of the several places whereon the Light rebounding from, or passing through, the Prism appear'd either Sincere or Tincted, was produc'd. But because it would be Tedious and not so Intelligible to deliver this in Words, I have thought fit to Referr You to the Annexed Scheme where the Newly mention'd particulars may be at one View taken Notice of.<sup>†\*</sup>

### *Experiment V.*

I know not whether you will think it Inconsiderable to annex to this Experiment, That we observ'd in a Room not / Darken'd, that the Prismatical Iris (if I may so call it) might be Reflected without losing any of its several *Colours* (for we now consider not their *Order*) not onely from a plain Looking-glass and from the calm Surface of Fair Water, but also from a Concave Looking-glass; and that Refraction did as little Destroy those Colours as Reflection. For by the help of a large (double Convex) Burning-glass through which we Refracted the Suns Beams, we found that one part of the Iris might be made to appear either beyond, or on this side of the other Parts of the same Iris; but yet the same Vivid Colours would appear in the Displac'd part (if I may so term it) as in the other. To which I shall add, that having, by hiding the side of the Prism, obverted to the Sun with an Opacous Body, wherein only one small hole was left for the Light to pass through, reduc'd the Prismatical Iris (cast upon White Paper) into a very narrow compass, and look'd upon it through a Microscope; the Colours appear'd the same as to kind that they did to the naked Eye. /

### *Experiment VI.*

It may afford matter of Speculation to the Inquisitive, such as you, *Pyrophilus*, that as the Colours of outward Objects brought into a Darken'd Room, do so much depend for their Visibility upon the Dimness of the Light they are there beheld by; that the ordinary Light of the day being freely let in upon them, they immediately disappear: so our Tryals have inform'd us, that as to the Prismatical Iris painted on the Floor by the Beams of the Sun Trajected through a Triangular-glass; though the Colours of it appear very Vivid ev'n at Noon-day, and in Sun shiny Weather, yet by a more Powerfull Light they may be made to disappear. For having sometimes, (in prosecution of some Conjectures of mine not now to be Insisted on,) taken a large Metalline Concave *Speculum*, and with it cast the converging Beams of the Sun upon a

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Prismatical Iris which I had caus'd to be projected upon the Floor, I found that the over-powerfull Light made the Colours of the Iris disappear. And if I so Reflected the Light as that it cross'd but the middle of the Iris, in that part only the Colours vanish'd or were made Invisible; those / parts of the Iris that were on the right and left hand of the Reflected Light (which seem'd to divide them, and cut the Iris asunder) continuing to exhibit the same Colours as before. But upon this we must not now stay to Speculate.

### *Experiment VII.*

I have sometimes thought it worth while to take notice, whether or no the Colours of Opacous Bodies might not appear to the Eye somewhat Diversify'd, not only by the Disposition of the Superficial parts of the Bodyes themselves and by the Position of the Eye in Reference to the Object and the Light, (for these things are Notorious enough;) but according also to the Nature of the Lucid Body that shines upon them. And I remember that in Prosecution of this Curiosity, I observ'd a manifest Difference in some Kinds of Colour'd Bodyes look'd on by Day-light, and afterwards by

the light of the Moon; either directly falling on them or Reflected upon them from a Concave Looking-glass. But not finding at present in my Collections about Colours any thing set down of this Kind, I shall, till I have opportunity to repeat them, content my self to add what I find Register'd concerning / Colours look'd on by Candle-light, in regard that not only the Experiment is more easie to be repeated, but the Objects being the same sorts of Colour'd Paper lastly mention'd, the Collation of the two Experiments may help to make the Conjectures they will suggest somewhat the less uncertain.

Within a few dayes of the time above mention'd, divers Sheets of Colour'd Paper that had been look'd upon before in the Sun-shine were look'd upon at night by the light of a pretty big Candle, (snuff'd) and the Changes that were observ'd were these.

The Yellow seem'd much fainter than in the Day, and inclinable to a pale Straw Colour.

The Red seem'd little Chang'd; but seem'd to Reflect Light more strongly than any other Colour (for White was none of them.)

A fair Deep Green look'd upon by it self, seem'd to be a Dark Blew: But being look'd upon together with a Dark Blew, appear'd Greenish; and beheld together with a Yellow appear'd more Blew than at first.

The Blew look'd more like a Deep Purple or Murray than it had done in the Day-light. /

The Purple seem'd very little alter'd.

The Red look'd upon with the Yellow made the Yellow look almost like Brown Cap-paper.

*N[ote]*. The Caution Subjoyned to the third Experiment, is also Applicable to this.

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### *Experiment VIII.*

But here I must not omit to subjoyn, that to satisfie our Selves, whether or no the Light of a Candle were not made unsincere, and as it were Ting'd with a Yellow Colour by the Admixture of the Corpuscles it assumes from its Fuel; we did not content our selves with what appears to the Naked Eye, but taking a pretty thick Rod or Cylinder (for thin Peeces would not serve the turn) of deep Blew Glass, and looking upon the Candles flame at a Convenient distance through it, we perceiv'd as we expected, the Flame to look Green; which as we often note, is the Colour wont to emerge from the Composition of Opacous Bodies, which were apart one of them Blew, and the other Yellow. And this perchance may be the main Reason of that which some observe, that a sheet of very White Paper being look'd upon by Candle light, 'tis not easie at first to discern it from / a light Yellow or Lemon Colour; White Bodyes (as we have elsewhere observ'd) having more than those that are otherwise Colour'd, of a Specular Nature; in regard that though they exhibit not, (unless they be Polish'd,) the shape of the Luminary that shines on them, yet they Reflect its Light more Sincere and Untroubl'd, by either Shades or Refractions, than Bodyes of other Colours (as Blew, or Green, or Yellow or the like.)

### *Experiment IX.*

We took a Leaf of such Foliated Gold as Apothecaries are wont to Gild their Pills with; and with the Edge of a Knife, (lightly moysten'd by drawing it over the surface of the Tongue, and afterwards) laid upon the edge of the Gold Leaf; we so fasten'd it to the Knife, that being held against the light, it continu'd extended like a little Flagg. This Leaf being held very near the Eye, and obverted to the

Light, appear'd so full of Pores, that it seem'd to have such a kind of Transparency as that of a Sive, or a piece of Cyprus, or a Love-Hood; but the Light that pass'd by these Pores was in its Passages so Temper'd with Shadow, and Modify'd, that the Eye discern'd / no more a Golden colour, but a Greenish Blew. And for other's satisfaction, we did in the Night look upon a Candle through such a Leaf of Gold; and by trying the Effect of several Proportions of Distance betwixt the Leaf, the Eye and the Light, we quickly hit upon such a Position for the Leaf of Gold, as that the flame, look'd on through it, appear'd of a Greenish Blew, as we have seen in the Day time. The like Experiment try'd with a Leaf of Silver succeeded not well.

### *Experiment X.*

We have sometimes found in the Shops of our Druggists, a certain Wood, which is there call'd *Lignum Nephriticum*, because the Inhabitants of the Country where it grows, are wont to use the Infusion of it made in fair Water against the Stone of the Kidneys, and indeed an Eminent Physician of our Acquaintance, who has very Particularly enquir'd into that Disease, assures me, that he has found such an Infusion one of the most effectual Remedyes,

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which he has ever tried against that formidable Disease. The ancientest Account I have met with of this Simple, is given us by the Experienc'd *Monardes* in these Words. / *Nobis*, says he, *Nova Hispania mittit quoddam ligni genus crassum & enode, cujus usus jam diu receptus fuit in his Regionibus ad Renum vitia & Urinæ difficultates ac arenulas pellendas. Fit autem hac ratione, Lignum assulatim & minutim concisum in limpidissima aqua fontana maceratur, inque ea relinquitur, donec aqua à bibentibus absumpta sit, dimidia hora post injectum lignum aqua cœruleum colorem contrahit, qui sensim intenditur pro temporis diuturnitate, tamet si lignum candidum sit.*<sup>†@</sup>

This Wood, *Pyrophilus*, may afford us an Experiment, which besides the singularity of it, may give no small assistance to an attentive Considerer towards the detection of the Nature of Colours. The Experiment as we made it is this. Take *Lignum Nephriticum*, and with a Knife cut it into thin Slices, put about a handfull of these Slices into two three or four pound of the purest Spring-water, let them infuse there a night, but if you be in hast, a much shorter time may suffice; *decant* this Impregnated Water into a clear Glass Vial, and if you hold it directly between the Light and your Eye you shall see it wholly Tincted (excepting the very top of the Liquor, wherein you will sometimes discern a Sky-colour'd Circle) with / an almost Golden Colour, unless your Infusion have been made too Strong of the Wood, for in that case it will against the Light appear somewhat Dark and Reddish, and requires to be diluted by the addition of a convenient quantity of fair Water. But if you hold this Vial from the Light, so that your Eye be plac'd betwixt the Window and the Vial, the Liquor will appear of a deep and lovely Cœruleous Colour, of which also the drops, if any be lying on the out-side of the Glass, will seem to be very perfectly; And thus far we have try'd the Experiment, and found it to succeed even by the Light of Candles of the larger size. If you so hold the Vial over against your Eyes, that it may have a Window on one side of it, and a Dark part of the Room both before it and on the other side, you shall see the Liquor partly of a Blewish and partly of a Golden Colour. If turning your back to the Window, you powr out some of the Liquor towards the Light and towards your Eyes, it will seem at the comming out of the Glass to be perfectly Cœruleous, but when it is fallen down a little way, the drops may seem Particolour'd, according as the Beams of Light do more or less fully Penetrate and Illustrate them. If you take a Bason about / half full of Water, and having plac'd it so in the Sun-beams Shining into a Room, that one part of the Water may be freely Illustrated by the Beams of Light, and the other part of it Darkned by the shadow

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of the Brim of the Bason, if then I say you drop of our Tincture, made somewhat strong, both into the Shaded and Illuminated parts of the Water, you may by looking upon it from several places, and

by a little Agitation of the water, observe divers pleasing Phœnomena which were tedious to particularize. If you powr a little of this Tincture upon a sheet of White Paper, so as the Liquor may remain of some depth upon it, you may perceive the Neighbouring drops to be partly of one Colour, and partly of the other, according to the position of your Eye in reference to the Light when it looks upon them, but if you powr off all the Liquor, the paper will seem Dy'd of an almost Yellow Colour. And if a sheet of Paper with some of this Liquor in it be plac'd in a window where the Sunbeams may shine freely on it, then if you turn your back to the Sun and take a Pen or some such slender Body, and hold it over-thwart betwixt the Sun and the Liquor, you may perceive that the Shadow projected by the Pen upon the Liquor, will not all of it be a vulgar / and Dark, but in part a curiously Colour'd shadow, that edge of it, which is next the Body that makes it, being almost of a lively Golden Colour, and the remoter verge of a Cœruleous one.

These and other Phœnomena, which I have observ'd in this delightfull Experiment, divers of my friends have look'd upon not without some wonder, and I remember an excellent Oculist finding by accident in a friends Chamber a fine Vial full of this Liquor, which I had given that friend, and having never heard any thing of the Experiment, nor having any Body near him that could tell him what this strange Liquor might be, was a great while apprehensive, as he presently after told me, that some strange new distemper was invading his Eyes. And I confess that the unusualness of the Phœnomena made me very sollicitous to find out the Cause of this Experiment, and though I am far from pretending to have found it, yet my enquiries have, I suppose, enabled me to give such hints, as may lead your greater sagacity to the discovery of the Cause of this wonder. And first finding that this Tincture, if it were too copious in the water, Kept the Colours from being so lively, and their Change from being so discernable, and / finding also that the Impregnating Virtue of this Wood did by its being frequently Infus'd in New Water by degrees Decay, I Conjectur'd that the Tincture afforded by the Wood must proceed from some Subtler parts of it drawn forth by the Water, which swimming too and fro in it did so Modifie the Light, as to exhibit such and such Colours; and because these Subtile parts were so easily Soluble even in Cold water, I concluded that they must abound with Salts, and perhaps contain much of the Essential Salt, as the *Chymists* call it, of the Wood. And to try whether these Subtile parts were Volatile enough to be Distill'd, without the Dissolution of their Texture, I carefully Distill'd some of the Tincted Liquor in very low Vessels, and the gentle heat of a Lamp Furnace; but found all that came over to be as Limpid and Colourless as Rock-water, and the Liquor remaining in the Vessel to be so deeply Cœruleous, that it requir'd to be oppos'd to a very strong Light to appear of any other Colour. I took likewise a Vial with Spirit of Wine, and a little Salt of Harts-horn, and found

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that there was a certain proportion to be met with betwixt the Liquor and the Salt, which made the Mixture fit to exhibit some little Variety / of Colours not Observable in ordinary Liquors, as it was variously directed in reference to the Light and the Eye, but this Change of Colour was very far short from that which we had admir'd in our Tincture. But however, I suspected that the Tinging Particles did abound with such Salts, whose Texture, and the Colour springing from it, would probably be alter'd by peircing Acid Salts, which would in likelihood either make some Dissipation of their Parts, or Associate themselves to the like Bodies, and either way alter the colour exhibited by them; whereupon Pouring into a small Vial full of Impregnated Water, a very little Spirit of Vinegar, I found that according to my Expectation, the Cœruleous Colour immediately vanish'd, but was deceiv'd in the Expectation I had, that the Golden Colour would do so too; for, which way soever I turned the Vial, either to or from the Light, I found the Liquor to appear always of a Yellowish Colour and no other: Upon this I imagin'd that the Acid Salts of the Vinegar having been able to deprive the Liquor of its Cœruleous Colour, a Sulphureous Salt being of a contrary Nature, would be able to Mortifie the Saline Particles of Vinegar, and Destroy their / Effects; And accordingly having plac'd my Self betwixt the Window, and the Vial, and into the same Liquor dropt a few drops of Oyl of Tartar *per Deliquium*, (as *Chymists* call it) I observ'd with pleasure, that immediately upon the Diffusion of this Liquor, the Impregnated Water was restor'd to its former



Cœruleous Colour; And this Liquor of *Tartar* being very Ponderous, and falling at first to the Bottom of the Vial, it was easie to observe that for a little while the Lower part of the Liquor appear'd deeply Cœruleous, whilst all the Upper part retain'd its former Yellowness, which it immediately lost as soon as either Agitation or Time had made a competent Diffusion of the Liquor of *Tartar* through the Body of the former Tincture; and this restor'd Liquor did, as it was Look'd upon against or from the Light, exhibit the same *Phænomena* as the Tincted Water did, before either of the Adventitious Liquors was pour'd into it.

Having made, *Pyrophilus*, divers Tryals upon this Nephritick Wood, we found mention made of it by the Industrious Jesuit *Kircherus*, who having received a Cup Turned of it from the *Mexican* Procurator of his Society, has probably receiv'd also from him the Information he gives us concerning / that *Exotick* Plant, and therefore partly for that Reason, and partly because what he Writes concerning it, does not perfectly agree with what we have deliver'd, we shall not Scruple to acquaint you in his own Words, with as much of what he writes concerning our Wood, as is requisite to our present purpose. †@ *Hoc loco* (says he) *neutiquam omittendum duximus quoddam ligni candidi*

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*Mexicani genus, quod Indigenæ Coalle & Tlapazatli vocant, quod etsi experientia hucusque non nisi Cæruleo aquam colore tingere docuerit, nos tamen continua experientia invenimus id aquam in omne Colorum genus transformare, quod merito cuipiam Paradoxum videri posset; Ligni frutex grandis, ut aiunt, non rarè in molem arboris excrescit, truncus illius est crassus, enodis, instar piri arboris, folia ciceris foliis, aut rutæ haud absimilia, flores exigui, oblongi, lutei & spicatim digesti; est frigida & humida planta, licet parum recedat à medio temperamento. Hujus itaque descriptæ arboris lignum in poculum efformatum, aquam eidem infusam primo in aquam intense Cæruleam, colore floris Buglossæ, tingit, & quo diutius in eo steterit, tanto intensiorem colorem acquirit. Hanc igitur aquam si Vitrea Sphæræ infuderis, lucique exposueris, ne ullum quidem Cærulei coloris / vestigium apparebit, sed instar aquæ puræ putæ fontanæ limpidam claramque aspicientibus se præbebit. Porro si hanc phialam vitream versus locum magis umbrosum direxeris, totus humor gratissimum virorem referet; si adhuc umbrosioribus locis, subrubrum, & sic pro rerum objectarum conditione, mirum dictu, colorem mutabit; in tenebris verò vel in vase opaco posita, Cæruleum colorem suum resumat.*

In this passage we may take notice of the following Particulars. And first, he calls it a White *Mexican* Wood, whereas (not to mention that *Mornardes* informs us that it is brought out of *Nova Hispania*<sup>†\*</sup>) the Wood that we have met with in several places, and employ'd as *Lignum Nephriticum*, was not White, but for the most part of a much Darker Colour, not unlike that of the Sadder Colour'd Wood of Juniper. 'Tis true, that *Monardes* himself also says, that the Wood is White; and it is affirm'd, that the Wood which is of a Sadder Colour is Adulterated by being Imbu'd with the Tincture of a Vegetable, in whose Decoction it is steep'd. But having purposely enquir'd of the Eminentest of our *English* Druggists, he peremptorily deny'd it. And indeed, having consider'd some of the fairest Round pieces of this / Wood that I could meet with in these Parts, I had Opportunity to take notice that in one or two of them it was the External part of the Wood that was White, and the more Inward part that was of the other Colour, the contrary of which would probably have appear'd, if the Wood had been Adulterated after the afore-mention'd manner. And I have at present by me a piece of such Wood, which for about an Inch next the Bark is White, and then as it were abruptly passes to the above-mention'd Colour, and yet this Wood by the Tincture, it afforded us in Water, appears to have its Colour'd part Genuine enough; for as for the White part, it appears upon tryal of both at once, much less enrich'd with the tingent Property.

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Next, whereas our Author tells us, that the Infusion of this Wood expos'd in a Vial to the Light, looks like Spring-water, in which he afterwards adds, that there is no Tincture to be seen in it, our

Observation and his agree not, for the Liquor, which opposed to the Darker part of a Room exhibits a Sky-colour, did constantly, when held against the Light, appear Yellowish or Reddish, according as its Tincture was more Dilute or Deep; and / then, whereas it has been already said, that the Cœruleous Colour was by Acid Salts abolished, this Yellowish one surviv'd without any considerable Alteration, so that unless our Author's Words be taken in a very Limited Sense, we must conclude, that either his Memory mis-inform'd him, or that his White *Nephritick* Wood, and the Sadder Colour'd one which we employ'd, were not altogether of the same Nature: What he mentions of the Cup made of *Lignum Nephriticum*, we have not had Opportunity to try, not having been able to procure pieces of that Wood great enough, and otherwise fit to be turned into Cups; but as for what he says in the Title of his Experiment, that this Wood tinges the Water with all sorts of Colours, that is much more than any of those pieces of Nephritick Wood that we have hitherto employ'd, was able to make good; The change of Colours discernable in a Vial full of Water, Impregnated by any of them, as it is directed towards a place more Lightsome or Obscure, being far from affording a Variety answerable to so promising a Title. And as for what he tells us, that in the Dark the Infusion of our Wood will resume a Cœruleous Colour, I wish he had Inform'd us how he Try'd it./

But this brings into my mind, that having sometimes for Curiosity sake, brought a round Vial with a long Neck fill'd with the Tincture of *Lignum Nephriticum* into the Darken'd Room already often mention'd, and holding it sometimes in, sometimes near the Sun-beams that enter'd at the hole, and sometimes partly in them, and partly out of them, the Glass being held in several postures, and look'd upon from several Neighbouring parts of the room, disclos'd a much greater Variety of Colours than in ordinary inlightn'd Rooms it is wont to do; exhibiting, besides the usual Colours, a Red in some parts, and a Green in others, besides Intermediate Colours produc'd by the differing Degrees, and odd mixtures of Light and Shade.

By all this You may see, *Pyrophilus*, the reasonableness of what we elsewhere had occasion to mention, when we have divers times told you, that it is usefull to have New Experiments try'd over again, though they were, at first, made by Knowing and Candid Men, such Reiterations of Experiments commonly exhibiting some New Phœnomena, detecting some Mistake or hinting some Truth, in reference to them, that was not formerly taken notice of. And some of our friends have been pleas'd to / think, that we have made no unusefull addition to this Experiment, by shewing a way, how in a moment our Liquor may be depriv'd of its Blewness, and restor'd to it again by the affusion of a very few drops of Liquors, which have neither of them any Colour at all of their own. And that which deserves some particular wonder, is, that the Cœruleous Tincture of our Wood is subject by the

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former Method to be Destroy'd or Restor'd, the Yellowish or Reddish Tincture continuing what it was. And that you may see, that Salts are of a considerable use in the striking of Colours, let me add to the many Experiments which may be afforded us to this purpose by the Dyers Trade, this Observation; That as far as we have hitherto try'd, those Liquors in general that are strong of Acid Salts have the Power of Destroying the Blewness of the Infusion of our Wood, and those Liquors indiscriminately that abound with Sulphureous Salts, (under which I comprehend the Urinous and Volatile Salts of Animal Substances, and the Alcalisate or fixed Salts that are made by Incineration) have the vertue of Restoring it. /

### *A Corollary of the Tenth Experiment.*

That this Experiment, *Pyrophilus*, may be as well Usefull as Delightfull to You, I must mind You, *Pyrophilus*, that in the newly mention'd Observation, I have hinted to You a New and Easie way of Discovering in many Liquors (for I dare not say in all) whether it be an Acid or Sulphureous Salt, that is Predominant; and that such a Discovery is oftentimes of great Difficulty, and may frequently

be of great Use, he that is not a Stranger to the various Properties and Effects of Salts, and of how great moment it is to be able to distinguish their Tribes, may readily conceive. But to proceed to the way of trying other Liquors by an Infusion of our Wood, take it briefly thus. Suppose I have a mind to try whether I conjecture aright, when I imagine that Allom, though it be plainly a Mixt Body, does abound rather with Acid than Sulphureous Salt. To satisfie my self herein, I turn my back to the Light, and holding a small Vial full of the Tincture of *Lignum Nephriticum*, which look'd upon in that Position, appears Cœruleous, I drop into it a little of a strong Solution of Allom made in Fair Water, and finding upon the / Affusion and shaking of this New liquor, that the Blewness formerly conspicuous in our Tincture does presently vanish, I am thereby incited to suppose, that the Salt Prædominant in Allom belongs to the Family of Sour Salts; but if on the other side I have a mind to examine whether or no I rightly conceive that Spirit of Urine, or of Harts-horn is rather of a Saline Sulphureous (if I may so speak) than of an Acid Nature, I drop a little of the Saline Spirit of either into the Nephritick Tincture, and finding that the Cœruleous Colour is rather thereby Deepned than Destroy'd, I collect that the Salts, which constitute these Spirits, are rather Sulphureous than Acid. And to satisfie my self yet farther in this particular, I take a small Vial of fresh Tincture, and placing both it and my self in reference to the Light as formerly, I drop into the Infusion just as much Distill'd Vinegar, or other Acid liquor as will serve to Deprive it of its Blewness (which a few drops, if the Sour Liquor be strong, and the Vial small will suffice to do) then without changing my Posture, I drop and shake into the same Vial a small proportion of Spirit of Harts-horn or Urine, and finding that upon this affusion, the Tincture immediately recovers its Cœruleous Colour, I am thereby confirm'd / in my former

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Opinion, of the Sulphureous Nature of these Salts. And so, whereas it is much doubted by some Modern Chymists to what sort of Salt, that which is Prædominant in Quick-lime belongs, we have been perswaded to referr it rather to Lixivate than Acid Salts, by having observ'd, that though an Evaporated Infusion of it will scarce yield such a Salt, as Ashes and other Alcalizate Bodyes are wont to do, yet if we deprive our Nephritick Tincture of its Blewness by just so much Distill'd Vinegar as is requisite to make that Colour Vanish, the *Lixivium* of Quick-lime will immediately upon its Affusion recall the Banished Colour; but not so Powerfully as either of the Sulphureous Liquors formerly mention'd. And therefore I allow my self to guess at the *Strength* of the Liquors examin'd by this Experiment, by the *Quantity* of them which is sufficient to Destroy or Restore the Cœruleous Colour of our Tincture. But whether concerning Liquors, wherein neither Acid nor Alcalisate Salts are eminently Prædominant, our Tincture will enable us to conjecture any thing more than that such Salts are not Prædominant in them, I take not upon me to determine here, but leave to further Tryal; For I find not that Spirit of / Wine, Spirit of Tartar freed from Acidity, or Chymical Oyl of Turpentine, (although Liquors which must be conceiv'd very Saline, if Chymists have, which is here no place to Dispute, rightly ascrib'd tastes to the Saline Principle of Bodyes,) have any Remarkable Power either to deprive our Tincture of its Cœruleous Colour, or restore it, when upon the Affusion of Spirit of Vinegar it has disappear'd.

### *Experiment XI.*

And here I must not omit, *Pyrophilus*, to inform You, that we can shew You even in a Mineral Body something that may seem very near of Kin to the Changeable Quality of the Tincture of *Lignum Nephriticum*, for we have several flat pieces of Glass, of the thickness of ordinary Panes for Windows, one of which being interposed betwixt the Eye and a cleer Light, appears of a Golden Colour, not much unlike that of the moderate Tincture of our Wood, but being so look'd upon as that the Beams of light are not so much Trajected thorough it as Reflected from it to the Eye, that Yellow seems to degenerate into a pale Blew, somewhat like that of a Turquoise. And that which may also appear strange, is this, / that if in a certain posture you hold one of these Plates Perpendicular to the Horizon, so that the Sun-beams shine upon half of it, the other half being Shaded, You may see that the part Shin'd upon will be of a much Diluter Yellow than the Shaded

part which will appear much more Richly Colour'd; and if You alter the Posture of the Glass, so that it be not held Perpendicular, but Parallel in reference to the Horizon, You may see, (which perhaps you will admire) the Shaded part look of a Golden Colour, but the other that the Sun shines freely on, will appear considerably Blew, and as you remove any part of the Glass thus held Horizontally into the Sun-beams or Shade, it will in the twinkling of an Eye seem to pass from one of the above mention'd Colours to the other, the

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Sun-beams Trajected through it upon a sheet of White Paper held near it, do colour it with a Yellow, somewhat bordering upon a Red, but yet the Glass may be so oppos'd to the Sun, that it may upon Paper project a mix'd Colour here and there more inclin'd to Yellow, and here and there more to Blew. The other Phœnomena of this odd Glass, I fear it would be scarce worth while to Record, and therefore I shall rather advertise You, *First*, that in the trying of these Experiments / with it, you must take notice that one of the sides has either alone, or at least principally its Superficial parts dispos'd to the Reflection of the Blew Colour above nam'd, and that therefore you must have a care to keep that side nearest to the Eye. And next, that we have our selves made Glasses not unfit to exhibit an Experiment not unlike that I have been speaking of, by laying upon pieces of Glass some very finely foliated Silver, and giving it by degrees a much stronger Fire than is requisite or usual for the Tinging of Glasses of other Colours. And this Experiment, not to mention that it was made without a Furnace in which Artificers that Paint Glass are wont to be very Curious, is the more considerable, because, that though a Skilfull Painter could not deny to me that 'twas with Silver he Colour'd his Glasses Yellow; yet he told me, that when to Burn them (as they speak) he layes on the plates of Glass nothing but a *Calx* of Silver Calcin'd without Corrosive Liquors, and Temper'd with Fair Water, the Plates are Ting'd of a fine Yellow that looks of a Golden Colour, which part soever of it you turn to or from the Light; whereas (whether it be what an Artificer would call Over-doing, or Burning, or else the imploying the Silver / Crude that makes the Difference,) we have found more than once, that some Pieces of Glass prepar'd as we have related, though held against the Light they appear'd of a Transparent Yellow, yet look'd on with ones back turn'd to the Light they exhibited an Untransparent Blew.

### *Experiment XII.*

If you will allow me, *Pyrophilus*, for the avoiding of Ambiguity, to imploy the Word Pigments, to signifie such prepared materials (as Cochinele, Vermilion, Orpiment,) as Painters, Dyers and other Artificers make use of to impart or imitate particular Colours, I shall be the better understood in divers passages of the following papers, and particularly when I tell you, That the mixing of Pigments being no inconsiderable part of the Painters Art, it may seem an Incroachment in me to meddle with it. But I think I may easily be excus'd (though I do not altogether pass it by) if I restrain my self to the making of a Transient mention of some few of their Practices about this matter; and that only so far forth, as may warrant me to observe to you, that there are but few Simple and Primary Colours (if I may so call them) / from whose Various Compositions all the rest do as it were Result. For though Painters can imitate the Hues (though not always the Splendor) of those almost Numberless differing Colours that are to be met with in the Works of Nature, and of Art, I have not yet found, that to exhibit this strange Variety they need imploy any

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more than *White*, and *Black*, and *Red*, and *Blew*, and *Yellow*; these *five*, Variously *Compounded*, and (if I may so speak) *Decompounded*, being sufficient to exhibit a Variety and Number of Colours, such, as those that are altogether Strangers to the Painters Pallets, can hardly imagine.

Thus (for Instance) Black and White differingly mix'd, make a Vast company of Lighter and Darker Grays.

Blew and Yellow make a huge Variety of Greens.

Red and Yellow make Orange Tawny.

Red with a little White makes a Carnation.

Red with an Eye of Blew, makes a Purple; and by these simple Compositions again Compounded among themselves, the Skilfull Painter can produce what kind of Colour he pleases, and a great many more than we have yet Names for. But, as I intimated above, 'tis not my Design / to prosecute this Subject, though I thought it not unfit to take some Notice of it, because we may hereafter have occasion to make use of what has been now deliver'd, to illustrate the Generation of Intermediate Colours; concerning which we must yet subjoyn this Caution, that to make the rules about the Emergency of Colours, fit to be Relied upon, the Corpuscles whereof the Pigments consist must be such as do not Destroy one anothers Texture, for in case they do, the produced Colour may be very Different from that which would Result from the Mixture of other harmless Pigments of the same Colours, as I shall have Occasion to shew ere long.

### *Experiment XIII.*

It may also give much light to an Enquirer into the Nature of Colours, to know that not only in Green, but in many (if not all) other Colours, the Light of the Sun passing through Diaphanous Bodies of differing Hues may be tinged of the same Compound Colour, as if it came from some Painters Colours of the same Denomination, though this later be exhibited by Reflection, and be (as the / former Experiment declares) manifestly Compounded of material Pigments. Wherefore to try the Composition of Colours by Trajection, we provided several Plates of Tinged Glass, which being laid two at a time one on the top of another, the Object look'd upon through them both, appear'd of a Compounded Colour, which agrees well with what we have observ'd in the second Experiment, of Looking against the Light through differinglly Colour'd Papers. But we thought the Experiment would be more Satisfactory, if we procur'd the Sun-beams to be so Ting'd in their passage through Plates of Glass, as to exhibit the Compounded Colour upon a Sheet of White Paper. And though by reason of the Thickness of the Glasses, the Effect was but Faint, even when the Sun was High and Shin'd forth clear, yet, we easily remedied that by Contracting the Beams we cast on them by means of a Convex Burning-glass, which where it made the Beams much converge Increas'd the Light enough to make the Compounded Colour very manifest upon the Paper. By this means we

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observ'd, that the Beams trajected through Blew and Yellow compos'd a Green, that an intense and moderate Red did with Yellow make differing / degrees of Saffron, and Orange Tawny Colours, that Green and Blew made a Colour partaking of both, such as that which some Latin Writers call *Pavonaceus*,<sup>†\*</sup> that Red and Blew made a Purple, to which we might add other Colours, that we produc'd by the Combinations of Glasses differinglly Ting'd, but that I want proper Words to express them in our Language, and had not when we made the Tryals, the Opportunity of consulting with a Painter, who perchance might have Suppli'd me with some of the terms I wanted.

I know not whether it will be requisite to subjoyn on this Occasion, what I tried concerning Reflections from Colour'd Glasses, and other Transparent Bodies, namely, that having expos'd four or five sorts of them to the Sun, and cast the Reflected Beams upon White Paper held near at hand, the Light appear'd not manifestly Ting'd, but as if it had been Reflected from the Impervious parts of a Colourless Glass, only that Reflected from the Yellow was here and there stain'd with the same Colour, as if those Beams were not all Reflected from the Superficial, but some from the Internal parts of the Glass; upon which Occasion you may take notice, that a Skilfull Tradesman, who makes such Colour'd / Glass told me; that where as the Red Pigment was but Superficial, the Yellow penetrated to the very midst of the Plate. But for further Satisfaction, not having the Opportunity to



Foliate those Plates, and so turn them into Looking-glasses, we Foliated a Plate of *Muscovy* Glass, and then laying on it a little Transparent Varnish of a Gold Colour, we expos'd it to the Sun-beams, so as to cast them upon a Body fit to receive them, on which the Reflected Light, appearing, as we expected, Yellow, manifested that Rebouncing from the Specular part of the *Selenitis*, it was Ting'd in its return with the Colour of the Transparent Varnish through which it pass'd.

#### *Experiment XIV.*

After what we have said of the Composition of Colours, it will now be seasonable to annex some Experiments that we made in favour of those Colours, that are taught in the Schools not to be Real, but only Apparent and Phantastical; For we found by Tryals, that these Colours might be Compounded, both with True and Stable Colours, and with one another, as well as unquestionably Genuine and Lasting Colours, and that the Colours / resulting from such Compositions, would respectively deserve the same Denominations.

For first, having by the Trajection of the Sun-beams through a Glass-prism thrown an Iris on the Floor, I found that by placing a Blew Glass at a convenient distance betwixt the Prism and the Iris, that part of the Iris that was before Yellow, might be made to appear Green, though not of a Grass Green, but of one more Dilute and Yellowish. And it seems not improbable, that the

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narrow Greenish List (if I may so call it) that is wont to be seen between the Yellow and Blew parts of the Iris, is made by the Confusion of those two Bordering Colours.

Next, I found, that though the want of a sufficient Liveliness in either of the Compounding Colours, or a light Error in the manner of making the following Tryals, was enough to render some of them Unsuccessfull, yet when all necessary Circumstances were duely observ'd; the Event was answerable to our Expectation and Desire.

And (as I formerly Noted) that Red and Blew compound a Purple, so I could produce this last nam'd Colour, by casting at some Distance from the Glass the Blew / part of the Prismatical Iris (as I think it may be call'd for Distinction sake) upon a Lively Red, (for else the Experiment succeeds not so well.) And I remember, that sometimes when I try'd this upon a piece of Red Cloath, *that* part of the Iris which would have been Blew, (as I try'd by covering that part of the Cloath with a piece of White Paper) and Compounded with the Red, wherewith the Cloath was Imbued before, appear'd of a fair Purple, did, when I came to View it near at hand, look very Odly, as if there were some strange Reflection or Refraction or both made in the Hairs of which that Cloath was composed.

Casting likewise the Prismatical Iris upon a very Vivid Blew, I found that part of it, which would else have been the Yellow, appear Green. (Another somewhat differing Tryal, and yet fit to confirm this, you will find in the fifteenth Experiment.)

But it may seem somewhat more strange, that though the Prismatical Iris being made by the Refraction of Light through a Body that has no Colour at all, must according to the Doctrine of the Schools consist of as purely Emphatical Colours, as may be, yet even these may be Compounded with one another, as well as Real Colours in / the Grossest Pigments. For I took at once two Triangular Glasses, and one of them being kept fixt in the same Posture, that the Iris it projected on the Floor might not Waver, I cast on the same Floor another Iris with the other Prism, and Moving it too and fro to bring what part of the second Iris I pleas'd, to fall upon what part of the first I thought fit, we did sometimes (for a small Errour suffices to hinder the Success) obtain by this means a Green Colour in that part of the more Stable Iris, that before was Yellow, or Blew, and frequently by casting those Beams, that in one of the Iris's made the Blew upon the Red parts of the other Iris, we were able to produce a lovely Purple, which we can Destroy or Recompose at pleasure, by Severing

and Reapproaching the Edges of the two Iris's.

### *Experiment XV.*

On this occasion, *Pyrophilus*, I shall add, that finding the Glass-prism to be the usefulest Instrument Men have yet imploy'd about the Contemplation of Colours, and considering that Prisms hitherto in use are made of Glass Transparent and Colourless, I thought it would not be amiss to try, / what change

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the Superinduction of a Colour, without the Destruction of the Diaphaneity, would produce in the Colours exhibited by the Prism. But being unable to procure one to be made of Colour'd Glass, and fearing also that if it were not carefully made, the Thickness of it would render it too Opacous, I endeavoured to substitute one made of Clarify'd Rosin, or of Turpentine brought (as I elsewhere teach<sup>†\*</sup>) to the consistence of a Transparent Gum. But though these Endeavours were not wholly lost, yet we found it so difficult to give these Materials their true Shape, that we chose rather to Varnish over an ordinary Prism with some of those few Pigments that are to be had Transparent; as accordingly we did first with Yellow, and then with Red, or rather Crimson, made with Lake temper'd with a convenient Oyl, and the Event was, That for want of good Transparent Colours, (of which you know there are but very few) both the Yellow and the Red made the Glass so Opacous, (though the Pigment were laid on but upon two Sides of the Glass, no more being absolutely necessary) that unless I look'd upon an Inlightned Window, or the Flame of a Candle, or some other Luminous or very Vivid object, / I could scarce discern any Colours at all, especially when the Glass was cover'd with Red. But when I did look on such Objects, it appear'd (as I expected) that the Colour of the Pigment had Vitiated or Drown'd some of those which the Prism would according to its wont have exhibited, and mingling with others, Alter'd them: as I remember, that both to my Eyes, and others to whom I show'd it, when the Prism was cover'd with Yellow, it made those Parts of bright Objects, where the Blew would else have been Conspicuous, appear of a light Green. But, *Pyrophilus*, both the Nature of the Colours, and the Degree of Transparency, or of Darkness in the Pigment, besides divers other Circumstances, did so vary the *Phænomena* of these Tryals, that till I can procure small Colour'd Prisms, or Hollow ones that may be filled with Tincted Liquor, or obtain some better Pigments than those I was reduc'd to imploy, I shall forbear to Build any thing upon what has been deliver'd, and shall make no other use of it, than to invite you to prosecute the Inquiry further. /

### *Experiment XVI.*

And here, *Pyrophilus*, since we are treating of Emphatical Colours, we shall add what we think not unworthy your Observation, and not unfit to afford some Exercise to the Speculative. For there are some Liquors, which though Colourless themselves, when they come to be Elevated, and Dispers'd into Exhalations, exhibit a conspicuous Colour, which they lose again, when they come to be Reconjoyn'd into a Liquor, as good Spirit of *Nitre*, or upon its account strong *Aqua-fortis*, though devoid of all appearance of Redness whilst they continue in the form of a Liquor, if a little Heat chance to turn the Minute parts of them into Vapours, the Steam will appear of a Reddish

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or deep Yellow Colour, which will Vanish when those Exhalations come to resume the form of a Liquor.

And not only if you look upon a Glass half full of *Aqua-fortis*, or Spirit of *Nitre*, and half full of *Nitrous* steams proceeding from it, you will see the Upper part of the Glass of the Colour freshly

mention'd, if through it you look upon the Light. But which is much more considerable, I / have tried, that putting *Aqua-fortis* in a long clear Glass, and adding a little Copper or some such open Metall to it, to excite Heat and Fumes, the Light trajected through those Fumes, and cast upon a sheet of White Paper, did upon that appear of the Colour that the Fumes did, when directly Look'd upon, as if the Light were as well Ting'd in its passage through these Fumes, as it would have been by passing through some Glass or Liquor in which the same Colour was Inherent.

To which I shall further add, that having sometimes had the Curiosity to observe whether the Beams of the Sun near the Horizon trajected through a very Red Sky, would not (though such Rednesses are taken to be but Emphatical Colours) exhibit the like Colour, I found that the Beams falling within a Room upon a very White Object, plac'd directly opposite to the Sun, disclos'd a manifest Redness, as if they had pass'd through a Colour'd *Medium*.

### *Experiment XVII.*

The emergency, *Pyrophilus*, of Colours upon the Coalition of the Particles of such Bodies as were neither of them of the Colour of that Mixture whereof they are the / Ingredients, is very well worth our attentive Observation, as being of good use both Speculative and Practical; For much of the Mechanical use of Colours among Painters and Dyers, doth depend upon the Knowledge of what Colours may be produc'd by the Mixtures of Pigments so and so Colour'd. And (as we lately intimated) 'tis of advantage to the contemplative Naturalist, to know how many and which Colours are Primitive (if I may so call them) and Simple, because it both eases his Labour by confining his most sollicitious Enquiry to a small Number of Colours upon which the rest depend, and assists him to judge of the nature of particular compounded Colours, by shewing him from the Mixture of what more Simple ones, and of what Proportions of them to one another, the particular Colour to be consider'd does result. But because to insist on the Proportions, the Manner and the Effects of such Mixtures would oblige me to consider a greater part of the Painters Art and Dyers Trade, than I am well acquainted with, I confin'd my self to make Trial of *several ways to produce Green*, by the composition of Blew and Yellow. And shall in this place both Recapitulate most of the things I have Dispersedly deliver'd / already concerning that Subject, and Recruit them.

And first, whereas Painters (as I noted above) are wont to make Green by tempering Blew and Yellow, both of them made into a soft Consistence, with

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either Water or Oyl, or some Liquor of Kin to one of those two, according as the Picture is to be Drawn with those they call *Water Colours*, or those they term *Oyl Colours*, I found that by choosing fit Ingredients, and mixing them in the form of Dry Powders, I could do, what I could not if the Ingredients were temper'd up with a Liquor; But the Blew and Yellow Powders must not only be finely Ground, but such as that the Corpuscles of the one may not be too unequal to those of the other, lest by their Disproportionate Minuteness the Smaller cover and hide the Greater. We us'd with good success a slight Mixture of the fine Powder of Bise, with that of Orpiment, or that of good Yellow Oker, I say a *slight* Mixture, because we found that an *exquisite* Mixture did not do so well, but by lightly mingling the two Pigments in several little Parcels, those of them in which the Proportion and Manner of Mixture was more Lucky, afforded us a good Green. /

2. We also learn'd in the Dye-houses, that Cloth being Dy'd Blew with Woad, is afterwards by the Yellow Decoction of Woud-wax or Wood-wax Dy'd into a Green Colour.

3. You may also remember what we above Related, where we intimated, that having in a Darkn'd Room taken two Bodies, a Blew and a Yellow, and cast the Light Reflected from the one upon the other, we likewise obtain'd a Green.

4. And you may remember, that we observ'd a Green to be produc'd, when in the same Darkn'd Room we look'd at the Hole at which alone the Light enter'd, through the Green and Yellow parts of a sheet of Marbl'd Paper laid over one another.

5. We found too, that the Beams of the Sun being trajected through two pieces of Glass, the one Blew and the other Yellow, laid over one another, did upon a sheet of White paper on which they were made to fall, exhibit a lovely Green.

6. I hope also, that you have not already forgot, what was so lately deliver'd, concerning the composition of a Green, with a Blew and Yellow; of which most Authors would call the one a *Real*, and the other an *Emphatical*./

7. And I presume, you may have yet fresh in your memory, what the fourteenth Experiment informs you, concerning the exhibiting of a Green, by the help of a Blew and Yellow, that were both of them Emphatical.

8. Wherefore we will proceed to take notice, that we also devis'd a way of trying whether or no Metalline Solutions though one of them at least had its Colour Adventitious, by the mixture of the *Menstruum* employd' to dissolve it, might not be made to compound a Green after the manner of other Bodies. And though this seem'd not easie to be perform'd by reason of the Difficulty of finding Metalline Solutions of the Colour requisite, that would mix without Præcipitating each other; yet after a while having consider'd the matter, the first Tryal afforded me the following Experiment. I took a High Yellow solution of good Gold in *Aqua-Regis*, (made of *Aqua-fortis*, and as I remember half its weight of Spirit of Salt) To this I put a due Proportion of a

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deep and lovely Blew Solution of Crude Copper, (which I have elsewhere taught to be readily Dissoluble in strong Spirit of Urine) and these two Liquors though at first they seem'd a little to Curdle one another, yet being thoroughly mingl'd by Shaking, / they presently, as had been Conjectur'd, united into a Transparent Green Liquor, which continu'd so for divers days that I kept it in a small Glass wherein 'twas made, only letting fall a little Blackish Powder to the Bottom. The other *Phænomena* of this Experiment belong not to this place, where it may suffice to take notice of the Production of a Green, and that the Experiment was more than once repeated with Success.

9. And lastly, to try whether this way of compounding Colours would hold ev'n in Ingredients actually melted by the Violence of the Fire, provided their Texture were capable of safely induring Fusion, we caus'd some Blew and Yellow Ammel to be long and well wrought together in the Flame of a Lamp, which being Strongly and Incessantly blown on them kept them in some degree of Fusion, and at length (for the Experiment requires some Patience as well as Skil) we obtain'd the expected Ammel of a Green Colour.

I know not, *Pyrophilus*, whether it be worth while to acquaint you with the ways that came into my Thoughts, whereby in some measure to explicate the first of the mention'd ways of making a Green; for I have sometimes Conjectur'd, that the mixture / of the Bise and the Orpiment produc'd a Green by so altering the Superficial Asperity, which each of those Ingredients had apart, that the Light Incident on the mixture was Reflected with differing Shades, as to Quantity, or Order, or both, from those of either of the Ingredients, and such as the Light is wont to be Modify'd with, when it Reflects from Grass, or Leaves, or some of those other Bodies that we are wont to call Green. And sometimes too I have doubted, whether the produced Green might not be partly at least deriv'd from this, That the Beams that Rebound from the Corpuscles of the Orpiment, giving one kind of stroak upon the *Retina*, whose Perception we call Yellow, and the Beams Reflected from the Corpuscles of the Bise, giving another stroak upon the same *Retina*, like to Objects that are Blew, the Contiguity and Minuteness of these Corpuscles may make the Appulse of the Reflected Light fall upon the *Retina* within so narrow a Compass, that the part they Beat upon being but as it were a Physical

point, they may give a Compounded stroak, which may consequently exhibit a Compounded and new Kind of Sensation, as we see that two Strings of a Musical Instrument being struck together, making two / Noises that arrive at the Ear at the same time as to Sense, yield a Sound differing from either of them, and as it were Compounded of both; Insomuch that if they be Discordantly tun'd, though each of them struck apart would yield a Pleasing Sound, yet being struck together they make but a Harsh and troublesome Noise. But this not being so fit a place to prosecute Speculations, I shall not insist, neither upon these Conjectures nor any others, which the Experiment we have been mentioning may have suggested to me. And I shall leave it to you, *Pyrophilus*, to derive what Instruction you can from comparing together the Various ways

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whereby a Yellow and a Blew can be made to Compound a Green. That which I now pretend to, being only to shew that the first of those mention'd ways, (not to take at present notice of the rest) does far better agree with our Conjectures about Colours, than either with the Doctrine of the Schools, or with that of the *Chymists*, both which seem to be very much Disfavour'd by it.

For first, since in the Mixture of the two mention'd Powders I could by the help of a very excellent *Microscope* (for ordinary ones will scarce serve the turn) discover that which seem'd to the naked Eye a Green / Body, to be but a heap of Distinct, though very small Grains of Yellow Orpiment and Blew Bise confusedly enough Blended together, it appears that the Colour'd Corpuscles of either kind did each retain its own Nature and Colour; By which it may be guess'd, what meer Transposition and Juxtaposition of Minute and Singly unchang'd Particles of Matter can do to produce a new Colour; For that this Local Motion and new Disposition of the small parts of the Orpiment did Intervene is much more manifest than it is easie to Explicate how they should produce this new Green otherwise than by the new Manner of their being put together, and consequently by their new Disposition to Modifie the Incident Light by Reflecting it otherwise than they did before they were Mingl'd together.

Secondly, The Green thus made being (if I may so speak) Mechanically produc'd, there is no pretence to derive it from I know not what incomprehensible Substantial Form, from which yet many would have us believe that Colours must flow; Nor does this Green, though a Real and Permanent, not a Phantastical and Vanid Colour, seem to be such an Inherent Quality as they would have it, since not only each part of / the Mixture remains unalter'd in Colour, and consequently of a differing Colour from the Heap they Compose, but if the Eye be assisted by a *Microscope* to discern things better and more distinctly than before it could, it sees not a Green Body, but a Heap of Blew and Yellow Corpuscles.

And in the third place, I demand what either Sulphur, or Salt, or Mercury has to do in the Production of this Green; For neither the Bise nor the Orpiment were indu'd with that Colour before, and the bare Juxtaposition of the Corpuscles of the two Powders that work not upon each other, but might if we had convenient Instruments be separated, unalter'd, cannot with any probability be imagin'd either to Increase or Diminish any of the three Hypostatical Principles, (to which of them soever the *Chymists* are pleas'd to ascribe Colours) nor does there here Intervene so much as Heat to afford them any colour to pretend, that at least there is made an Extraversion (as the *Helmontians* speak) of the Sulphur or of any of the two other supposed Principles;<sup>†\*</sup> But upon this Experiment we have already Reflected enough, if not more than enough for once. /

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### *Experiment XVIII.*

But here, *Pyrophilus*, I must advertise you, that 'tis not every Yellow and every Blew that being mingl'd will afford a Green; For in case one of the Ingredients do not Act only as endow'd with such



a Colour, but as having a power to alter the Texture of the Corpuscles of the other, so as to Indispose them to Reflect the Light, as Corpuscles that exhibit a Blew or a Yellow are wont to Reflect it, the emergent Colour may be not Green, but such as the change of Texture in the Corpuscles of one or both of the Ingredients qualifies them to shew forth; as for instance if you let fall a few Drops of Syrrup of Violets upon a piece of White Paper, though the Syrrup being spread will appear Blew, yet mingling with it two or three Drops of the lately mention'd Solution of Gold, I obtain'd not a Green but a Reddish mixture, which I expected from the remaining Power of the Acid Salts abounding in the Solution, such Salts or Saline Spirits being wont, as we shall see anon, though weakn'd, so to work upon that Syrrup as to change it into a Red or Reddish Colour. And to confirm that for which I allege the former / Experiment, I shall add this other, that having made a very strong and high-colour'd Solution of Filings of Copper with Spirit of Urine, though the *Menstruum* seem'd Glutted with the Metall, because I put in so much Filings that many of them remain'd for divers days Undissolv'd at the Bottom, yet having put three or four Drops of Syrrup of Violets upon White Paper, I found that the deep Blew Solution proportionably mingl'd with this other Blew Liquor did not make a Blew mixture, but, as I expected, a fair Green, upon the account of the Urinous Salt that was in the *Menstruum*.

### *Experiment XIX.*

To shew the *Chymists*, that Colours may be made to Appear or Vanish, where there intervenes no Accession or Change either of the Sulphureous, or the Saline, or the Mercurial principle (as they speak) of Bodies: I shall not make use of the Iris afforded by the Glass-prism, nor of the Colours to be seen in a fair Morning in those drops of Dew that do in a convenient manner Reflect and Refract the Beams of Light to the Eye; But I will rather mind them of what they may observe in their / own Laboratories, namely, that divers, if not all, Chymical Essential Oyls, as also good Spirit of Wine, being shaken till they have good store Bubbles, those Bubbles will (if attentively consider'd) appear adorn'd with various and lovely Colours, which all immediately Vanish, upon the relapsing of the Liquor that affords those Bubbles their Skins, into the rest of the Oyl; or Spirit of Wine, so that a Colourless Liquor may be made in a trice to exhibit variety of Colours, and may lose them in a moment without the Accession or Diminution of any of its Hypostatical Principles. And, by the way, 'tis not unworthy our notice, that some Bodies, as well Colourless, as Colour'd, by being brought to a great Thinness of parts, acquire Colours though they had none before, or Colours differing from them they were before endued with: For, not to insist on the Variety of Colours, that Water, made somewhat Glutinous by Sope, acquires, when 'tis

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blown into such Sphærical Bubbles as Boys are wont to make and play with; Turpentine (though it have a Colour deep enough of its own) may (by being blown into after a certain manner) be brought to afford Bubbles adorn'd with variety of Orient Colours, which though / they Vanish after some while upon the breaking of the Bubbles, yet they would in likelihood always exhibit Colours upon their *Superfices*, (though not always the same in the same Parts of them, but Vary'd according to the Incidence of the Sight, and the Position of the Eye) if their Texture were durable enough: For I have seen one that was Skill'd at fashioning Glasses by the help of a Lamp, blowing some of them so strongly as to burst them, whereupon it was found, that the Tenacity of the Metall was such, that before it broke it suffer'd it self to be reduc'd into Films so extremely thin, that being kept clean they constantly shew'd on their Surfaces (but after the manner newly mention'd) the varying Colours of the Rain-bow, which were exceedingly Vivid, as I had often opportunity to observe in some, that I caus'd purposely to be made, to keep by me.

But lest it should be objected, that the above mention'd Instances are drawn from Transparent Liquors, it may possibly appear, not impertinent to add, what I have sometimes thought upon, and several times tried, when I was considering the Opinions of the *Chymists* about Colours. I took then a Feather of a convenient Bigness / and Shape, and holding it at a fit distance betwixt my Eye and

the Sun when he was near the Horizon, me thought there appear'd to me a Variety of little Rain-bows, with differing and very vivid Colours, of which none was constantly to be seen in the Feather; the like *Phænomenon* I have at other times (though not with altogether so good success) produc'd, by interposing at a due distance a piece of Black Ribband betwixt the almost setting Sun and my Eye, not to mention the Trials I have made to the same purpose, with other Bodies.

### *Experiment XX.*

Take good Syrrup of Violets, Imprægnated with the Tincture of the flowers, drop a little of it upon a White Paper (for by that means the Change of Colour will be more conspicuous, and the Experiment may be practis'd in smaller Quantities) and on this Liquor let fall two or three drops of Spirit either of Salt or Vinegar, or almost any other eminently Acid Liquor, and upon the Mixture of these you shall find the Syrrup immediatly turn'd Red, and the way of Effecting such a Change has not been unknown to divers Persons / who have produc'd the like, by Spirit of Vitriol, or juice of Limmons, but have Groundlessly ascrib'd the Effect to some Peculiar Quality of those two Liquors, whereas, (as we have already intimated) almost any Acid Salt will turn Syrrup of Violets Red. But to improve the Experiment, let me add what has not (that I know of) been hitherto observ'd, and has, when we first shew'd it them, appear'd something strange, even to those that have been inquisitive into the Nature of Colours; namely, that if instead of Spirit of Salt, or that of Vinegar, you drop upon the Syrrup of Violets a little Oyl of Tartar

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*per Deliquium*, or the like quantity of Solution of Potashes, and rubb them together with your finger, you shall find the Blew Colour of the Syrrup turn'd in a moment into a perfect Green, and the like may be perform'd by divers other Liquors, as we may have occasion elsewhere to Inform you.

### *Annotation upon the twentieth Experiment.*

The use of what we lately deliver'd concerning the way of turning Syrrup of Violets, Red or Green, may be this; That, though it be a far more common and procurable / Liquor than the Infusion of *Lignum Nephriticum*, it may yet be easily substituted in its Room, when we have a mind to examine, whether or no the Salt predominant in a Liquor or other Body, wherein 'tis Loose and Abundant, belong to the Tribe of *Acid* Salts or not. For if such a Body turn the Syrrup of a Red or Reddish Purple Colour, it does for the most part argue the Body (especially if it be a distill'd Liquor) to abound with Acid Salt. But if the Syrrup be made Green, that argues the Predominant Salt to be of a Nature repugnant to that of the Tribe of Acids. For, as I find that either Spirit of Salt, or Oyl of Vitriol, or *Aqua-fortis*, or Spirit of Vinegar, or Juice of Lemmons, or any of the Acid Liquors I have yet had occasion to try, will turn Syrrup of Violets, of a *Red*, (or at least, of a *Reddish* Colour, so I have found, that not only the Volatile Salts of all Animal Substances I have us'd, as Spirit of Harts-horn, of Urine, of Sal-Armoniack, of Blood, &c. but also all the Alcalizate Salts I have imploy'd, as the Solution of Salt of Tartar, of Pot-ashes, of common Wood-ashes, Lime-water, &c. will immediately change the Blew Syrrup, into a perfect Green. And by the same way (to hint that upon / the by) I elsewhere show you, both the changes that Nature and Time produce, in the more Saline parts of some Bodies, may be discover'd, and also how ev'n such Chymically prepar'd Bodies, as belong not either to the Animal Kingdome, or to the Tribe of *Alkali's*, may have their new and superinduc'd Nature successfully Examin'd. In this place I shall only add, that not alone the Changing the Colour of the Syrrup, requires, that the Changing Body be more strong, of the Acid, or other sort of Salt that is Predominant in it, than is requisite for the working upon the Tincture of *Lignum Nephriticum*; but that in this also, the Operation of the formerly mention'd Salts upon our Syrrup, differs from their Operation upon our Tinctures, that in this Liquor, if the Cæruleous Colour be *Destroy'd* by an Acid Salt, it may be *Restor'd* by one that is either Volatile, or Lixivate; whereas in Syrrup of Violets, though one of these contrary Salts will *destroy* the Action of the other, yet neither of them will *restore* the Syrrup to its native Blew; but each of them will

Change it into the Colour which it self doth (if I may so speak) affect, as we shall have Occasion to show in the Notes on the twenty fifth Experiment. /

### *Experiment XXI.*

There is a Weed, more known to Plowmen than belov'd by them, whose Flowers from their Colour are commonly call'd *Blew-bottles*, and *Corn-weed*

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from their Growing among Corn.<sup>†@</sup> These Flowers some Ladies do, upon the account of their Lovely Colour, think worth the being Candied, which when they are, they will long retain so fair a Colour, as makes them a very fine Sallad in the Winter. But I have try'd, that when they are freshly gather'd, they will afford a Juice, which when newly express'd, (for in some cases 'twill soon enough degenerate) affords a very deep and pleasant Blew. Now, (to draw this to our present Scope) by dropping on this fresh Juice, a little Spirit of Salt, (that being the Acid Spirit I had then at hand) it immediately turn'd (as I predicted) into a Red. And if instead of the Sowr Spirit I mingled with it a little strong Solution of an Alcalizate Salt, it did presently disclose a lovely Green; the same Changes being by those differing sorts of Saline Liquors, producible in this *Natural juice*, that we lately mention'd to / have happen'd to that *factitious Mixture*, the Syrrup of Violets. And I remember, that finding this Blew Liquor, when freshly made, to be capable of serving in a Pen for an Ink of that Colour, I attempted by moistning one part of a piece of White Paper with the Spirit of Salt I have been mentioning, and another with some Alcalizate or Volatile Liquor, to draw a Line on the leisurely dry'd Paper, that should, ev'n before the Ink was dry, appear partly Blew, partly Red, and partly Green: But though the latter part of the Experiment succeeded not well, (whether because Volatile Salts are too Fugitive to be retain'd in the Paper, and Alcalizate ones are too Unctuous, or so apt to draw Moisture from the Air, that they keep the Paper from drying well) yet the former Part succeeded well enough; the Blew and Red being Conspicuous enough to afford a surprizing Spectacle to those, I acquaint not with (what I willingly allow you to call) the *Trick*.

### *Annotation upon the one and twentieth Experiment.*

But lest you should be tempted to think (*Pyrophilus*) that Volatile or Alcalizate / Salts change Blews into Green, rather upon the score of the easie Transition of the former Colour into the latter, than upon the account of the Texture, wherein most Vegetables, that afford a Blew, seem, though otherwise differing, to be Allied, I will add, that when I purposely dissolv'd Blew Vitriol in fair Water, and thereby imbu'd sufficiently that Liquor with that Colour, a Lixivate Liquor, and a Urinous Salt being Copiously pour'd upon distinct Parcels of it, did each of them, though perhaps with some Difference, turn the Liquor not Green, but of a deep Yellowish Colour, almost like that of Yellow Oker, which Colour the Precipitated Corpuscles retain'd, when they had Leisurely subsided to the Bottom. What this Precipitated Substance is, it is not needfull now to Enquire in this place; and in another, I have shown you, that notwithstanding its Colour, and its being Obtainable from an Acid *Menstruum* by the help of Salt of Tartar, it is yet far enough from being the true Sulphur of Vitriol.

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### *Experiment XXII.*

Our next Experiment (*Pyrophilus*) will perhaps seem to be of a contrary Nature / to the two former, made upon Syrrup of Violets, and Juice of Blew-bottles. For as in them by the Affusion of Oyl of Tartar, a Blewish Liquor is made Green, so in this, by the sole Mixture of the same Oyl, a Greenish Liquor becomes Blew. The hint of this Experiment was given us by the practice of some *Italian*

Painters, who being wont to Counterfeit *Ultra-marine Azure* (as they call it) by Grinding Verdigrease with Sal-Armoniack, and some other Saline Ingredients, and letting them Rot (as they imagine) for a good while together in a Dunghill, we suppos'd, that the change of Colour wrought in the Verdigrease by this way of Preparation, must proceed from the Action of certain Volatile and Alcalizate Salts, abounding in some of the mingled Concretes, and brought to make a further Dissolution of the Copper abounding in the Verdigrease, and therefore we Conjectur'd, that if both the Verdigrease, and such Salts were dissolv'd in fair Water, the small Parts of both being therein more subdivided, and set at liberty, would have better access to each other, and thereby Incorporate much the more suddenly; And accordingly we found, that if upon a strong Solution of good French Verdigrease (for 'tis that we / are wont to imploy, as the best) you pour a just quantity of Oyl of Tartar, and shake them well together, you shall immediately see a notable Change of Colour, and the Mixture will grow thick, and not transparent, but if you stay a while, till the Grosser part be Precipitated to, and settled in the Bottom, you may obtain a clear Liquor of a very lovely Colour, and exceeding delightfull to the Eye. But, you must have a care to drop in a competent Quantity of Oyl of Tartar, for else the Colour will not be so Deep, and Rich; and if instead of this Oyl you imploy a clear *Lixivium* of Pot-ashes, you may have an Azure somewhat Lighter or Paler than, and therefore differing from, the former. And if instead of either of these Liquors, you make use of Spirit of Urine, or of Harts-horn, you may according to the Quantity and Quality of the Spirit you pour in, obtain some further Variety (though scarce considerable) of Cæruleous Liquors. And yet lately by the help of this Urinous Spirit we made a Blew Liquor, which not a few Ingenious Persons, and among them, some, whose Profession makes them very Conversant with Colours, have looked upon with some wonder. But these Azure Colour'd Liquors / should be freed from the Subsiding matter, which the Salts of Tartar or Urine precipitate out of them, rather by being Decanted, than by Filtration. For by the latter of these ways we have sometimes found, the Colour of them very much Impair'd, and little Superiour to that of the grosser Substance, that it left in the Filtre.

### *Experiment XXIII.*

That Roses held over the Fume of Sulphur, may quickly by it be depriv'd of their Colour, and have as much of their Leaves, as the Fume works upon, burn'd pale, is an Experiment, that divers others have tried, as well as I. But (*Pyrophilus*) it may seem somewhat strange to one that has never consider'd

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the Compounded nature of Brimstone, That, whereas the Fume of Sulphur will, as we have said, Whiten the Leaves of Roses; That Liquor, which is commonly call'd Oyl of Sulphur *per Campanam*, because it is suppos'd to be made by the Condensation of these Fumes in Glasses shap't like Bells, into a Liquor, does powerfully heighten the Tincture of Red Roses, and make it more Red and Vivid, as we have easily tried by putting some Red-Rose Leaves, / that had been long dried, (and so had lost much of their Colour) into a Vial of fair Water. For a while after the Affusion of a convenient Quantity of the Liquor we are speaking of, both the Leaves themselves, and the Water they were Steep'd in, discover'd a very fresh and lovely Colour.

### *Experiment XXIV.*

It may (*Pyrophilus*) somewhat serve to Illustrate, not only the Doctrine of *Pigments*, and of *Colours*, but divers other Parts of the *Corpuscular Philosophy*; as that explicates Odours, and many other things, not as the Schools by Aery Qualities, but by Real, though extremely Minute Bodies; to examine, how much of a Colourless Liquor, a very small Parcel of a Pigment may Imbue with a *discernable* Colour. And though there be scarce any thing of Preciseness to be expected from such Trials, yet I presum'd, that (at least) I should be able to show a much further Subdivision of the



Parts of Matter into *Visible* Particles, than I have hitherto found taken notice of, and than most men would imagine; no Body, that I know of, having yet attempted to reduce this Matter to any Measure.  
/

The Bodies, the most promising for such a purpose, might seem to be the Metalls, especially Gold, because of the Multitude, and Minuteness of its Parts, which might be argu'd from the incomparable Closeness of its Texture: But though we tried a Solution of Gold made in *Aqua Regia* first, and then in fair Water; yet in regard we were to determine the Pigment we imploy'd, not by *Bulk* but *Weight*, and because also, that the Yellow Colour of Gold is but a faint one in Comparison of the deep Colour of *Cochineel*, we rather chose this to make our Trials with. But among divers of these it will suffice to set down one, which was carefully made in Vessels conveniently Shap'd; (and that in the presence of a Witness, and an Assistant) the Sum whereof I find among my *Adversaria*, Registered in the following Words. To which I shall only premise, (to lessen the wonder of so strange a diffusion of the Pigment) That *Cochineel* will be better Dissolv'd, and have its Colour far more heightn'd by Spirit of Urine, than (I say not by common Water, but) by Rectify'd Spirit of Wine it self.

The Note I spoke off is this. [One Grain of *Cochineel* dissolv'd in a pretty Quantity of Spirit of Urine, and then dissolv'd / further by degrees in fair Water, imparted a discernable, though but a very faint Colour, to about six Glass-fulls of Water, each of them containing about forty three Ounces and an half, which amounts to above a hundred twenty five thousand times its own Weight.]<sup>†\*</sup>

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### *Experiment XXV.*

It may afford a considerable Hint (*Pyrophilus*) to him, that would improve the Art of Dying, to know what change of Colours may be produc'd by the three several sorts of Salts already often mention'd, (some or other of which may be procur'd in quantity at reasonable Rates) in the Juices, Decoctions, Infusions, and (in a word) the more soluble parts of Vegetables. And, though the design of this Discourse be the Improvement of Knowledge, not of Trades: yet thus much I shall not scruple to intimate here, That the Blew Liquors, mention'd in the twentieth and one and twentieth Experiments, are far from being the only Vegetable Substances, upon which Acid, Urinous, and Alcalizate Salts have the like Operations to those recited in those two Experiments. For Ripe *Privet Berries* (for instance) being crush'd / upon White Paper, though they stain it with a Purplish Colour, yet if we let fall on some part of it two or three drops of Spirit of Salt, and on the other part a little more of the strong Solution of Pot-ashes, the former Liquor immediately turn'd that part of the Thick Juice or Pulp, on which it fell, into a lovely Red, and the latter turn'd the other part of it into a delightfull Green. Though I will not undertake, that those Colours in that Substance shall not be much more Orient, than Lasting; and though (*Pyrophilus*) this Experiment may seem to be almost the same with those already deliver'd concerning Syrrup of Violets, and the Juice of Blew-bottles, yet I think it not amiss to take this Occasion to inform you, that this Experiment reaches much farther, than perhaps you yet imagine, and may be of good Use to those, whom it concerns to know, how Dying Stuffs may be wrought upon by Saline Liquors. For, I have found this Experiment to succeed in so many Various Berries, Flowers, Blossoms, and other finer Parts of Vegetables, that neither my Memory, nor my Leisure serves me to enumerate them. And it is somewhat surprizing to see, by how Differingly-colour'd Flowers, or Blossoms, (for example) the Paper being / stain'd, will by an Acid Spirit be immediately turn'd Red, and by any *Alcaly* or any Urinous Spirit turn'd Green; insomuch that ev'n the crush'd Blossoms of *Meserion*, (which I gather'd in Winter and frosty Weather) and those of Pease, crush'd upon White Paper, how remote soever their Colours be from Green, would in a moment pass into a deep Degree of that Colour, upon the Touch of an Alcalizate Liquor. To which let us add, That either of those new Pigments (if I may so call them) may by the

Affusion of enough of a contrary Liquor, be presently chang'd from Red into Green, and from Green into Red, which Observation will hold also in Syrrup of Violets, Juices of Blew-bottles, &c.

### **Annotation.**

After what I have formerly deliver'd to evince, That there are many Instances, wherein new Colours are produc'd or acquir'd by Bodies, which *Chymists* are wont to think destitute of Salt, or to whose change of Colours no new Accession of Saline Particles does appear to contribute, I think we may safely

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enough acknowledge, / that we have taken notice of so many Changes made by the Intervention of Salts in the Colours of Mix'd Bodies, that it has lessen'd our Wonder, That though *many Chymists* are wont to ascribe the Colours of such Bodies to their Sulphureous, and *the rest* to their Mercurial Principle; yet *Paracelsus* himself directs us in the Indagation of Colours, to have an Eye principally upon Salts, as we find in that passage of his, wherein he takes upon him to Oblige his Readers much by Instructing them, of what things they are to expect the Knowledge from each of the three distinct Principles of Bodies. *Alias* (says he) *Colorum similis ratio est: De quibus brevem institutionem hanc attendite, quod scilicet colores omnes ex Sale prodeant. Sal enim dat colorem, dat Balsamum.*

And a little beneath.<sup>†@</sup> *Iam natura Ipsa colores protrahit ex sale, cuique speciei dans illum, qui ipsi competit, &c.* After which he concludes; *Itaque qui rerum omnium corpora cognoscere vult, huic opus est, ut ante omnia cognoscat Sulphur, Ab hoc, qui desiderat novisse Colores is scientiam istorum petat à Sale, Qui scire vult Virtutes, is scrutetur arcana Mercurii. Sic nimirum fundamentum hauserit Mysteriorum, in quolibet crescenti indagandorum, / prout natura cuilibet speciei ea ingessit.* But though *Paracelsus* ascribes to each of his belov'd Hypostatical Principles, much more than I fear will be found to belong to it; yet if we please to consider Colours, not as *Philosophers*, but as *Dyers*, the concurrence of Salts to the striking and change of Colours, and their Efficacy, will, I suppose, appear so considerable, that we shall not need to quarrel much with *Paracelsus*, for ascribing in this place (for I dare not affirm that he uses to be still of one Mind) the Colours of Bodies to their Salts, if by Salts he here understood, not only Elementary Salts, but such also as are commonly taken for Salts, as Allom, Crystals of Tartar, Vitriol, &c. because the Saline principle does chiefly abound in them, though indeed they be, as we elsewhere declare, mix'd Bodies, and have most of them, besides what is Saline, both Sulphureous, Aqueous, and Gross or Earthy parts.

But though (*Pyrophilus*) I have observ'd a Red and Green to be produc'd, the former, by Acid Salts, the later by Salts not Acid, in the exprest Juices of so many differing Vegetable Substances, that the Observation, if persued, may prove (as I said) of good Use: yet to show you how much e'vn these Effects depend upon the / particular Texture of Bodies, I must subjoyn some cases wherein I (who am somewhat backwards to admit Observations for Universal) had the Curiosity to discover, that the Experiments would not

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Uniformly succeed, and of these Exceptions, the chief that I now remember, are reducible to the following three.

### **Experiment XXVI.**

And, (first) I thought fit to try the Operation of Acid Salts upon Vegetable Substances, that are already and by their own Nature Red. And accordingly I made Trial upon Syrrup of Clove-july-flowers, the clear express'd Juice of the succulent Berries of *Spina Cervina*, or Buckthorn (which I

had long kept by me for the sake of its deep Colour) upon Red Roses, Infusion of Brazil, and divers other Vegetable Substances, on some of which crush'd (as is often mention'd) upon White Paper, (which is also to be understood in most of these Experiments, if no Circumstance of them argue otherwise) Spirit of Salt either made no considerable Change, or alter'd the Colour but from a Darker to a Lighter Red. How it will succeed in many other Vegetable Juices, / and Infusions of the same Colour, I have at present so few at hand, that I must leave you to find it out your self. But as for the Operation of the other sorts of Salts upon these Red Substances, I found it not very Uniform, some Red, or Reddish Infusions, as of Roses, being turn'd thereby into a dirty Colour, but yet inclining to Green. Nor was the Syrrup of Clove-july-flowers turn'd by the Solution of Pot-ashes to a much better, though somewhat a Greener, Colour. Another sort of Red Infusions was by an *Alcaly* not turn'd into a Green, but advanc'd into a Crimson, as I shall have occasion to note ere long. But there were other sorts, as particularly the lovely Colour'd juice of Buckthorn Berries, that readily pass'd into a lovely Green.

### *Experiment XXVII.*

Among other Vegetables, which we thought likely to afford Exceptions to the General Observation about the differing Changes of Colours produc'd by Acid and Sulphureous Salts, we thought fit to make Trial upon the Flowers of *Jasmin*, they being both White as to Colour, and esteem'd to be of a more Oyly nature than other Flowers. Whereupon having taken / the White parts only of the Flowers, and rubb'd them somewhat hard with my Finger upon a piece of clean Paper, it appear'd very little Discolour'd. Nor had Spirit of Salt, wherewith I moisten'd one part of it, any considerable Operation upon it. But Spirit of Urine, and somewhat more effectually a strong Alcalizate Solution, did immediately turn the almost Colourless Paper moisten'd by the Juice of the *Jasmin*, not as those Liquors are wont to do, when put upon the Juices of other Flowers, of a good Green, but of a Deep, though somewhat Greenish Yellow, which Experiment I did afterwards at several times repeat with the like success. But it seems not that a great degree of Unctuousness is necessary to the Production of the like Effects, for when we try'd the Experiment with the Leaves of those purely White Flowers that

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appear about the end of Winter, and are commonly call'd *Snow drops*, the event, was not much unlike that, which, we have been newly mentioning.<sup>†\*</sup>

### *Experiment XXVIII.*

Another sort of Instances to show, how much changes of Colours effected by Salts, depend upon the particular Texture of the / Colour'd Bodies, has been afforded me by several *Yellow* Flowers, and other Vegetables, as Mary-gold Leaves, early Primroses, fresh Madder, &c. For being rubb'd upon White Paper, till they imbued it with their Colour, I found not, that by the addition of Alcalizate Liquors, nor yet by that of an Urinous Spirit, they would be turn'd either Green or Red: nor did so Acid a Spirit, as that of Salt, considerably alter their Colour, save that it seem'd a little to Dilute it. Only in some early Prim-roses it destroy'd the greatest part of the Colour, and made the Paper almost White agen. And Madder also afforded some thing peculiar, and very differing from what we have newly mention'd: For having gather'd some Roots of it, and, (whilst they were recent) express'd upon White Paper the Yellow Juice, an Alcalizate Solution drop'd upon it did not turn it either Green or White, but Red. And the bruis'd Madder it self being drench'd with the like Alcalizate Solution, exchang'd also its Yellowishness for a Redness. /

### *An admonition touching the four preceding Experiments.*

Having thus (*Pyrophilus*) given you divers Instances, to countenance the General observation



deliver'd in the twenty fifth Experiment, and divers Exceptions whereby it ought to be Limited; I must leave the further Inquiry into these Matters to your own Industry. For not remembring at present many of those other Trials, long since made to satisfy my self about Particulars, and not having now the Opportunity to repeat them, I must content my Self to

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have given you the Hint, and the ways of prosecuting the search your Self; and only declare to you in general, that, As I have made many Trials, unmention'd in this Treatise, whose Events were agreeable to those mention'd in the twenty fifth Experiment, so (to name now no other Instances) what I have try'd with Acid and Sulphureous Salts upon the Pulp of Juniper Berries, rubb'd upon White Paper, inclines me to think, That among that vast Multitude, and strange Variety of Plants that adorn the face of the Earth, perhaps many other Vegetables may be found, on which such *Menstruums* may not / have such Operations, as upon the Juice of Violets, Pease-blossoms, &c. no nor upon any of those three other sorts of Vegetables, that I have taken notice of in the three foregoing Experiments. It sufficiently appearing ev'n by these, that the effects of a Salt upon the Juices of particular Vegetables do very much depend upon their particular Textures.

### *Experiment XXIX.*

It may be of some Use towards the discovery of the nature of these Changes, which the Alimental Juice receives in some Vegetables, according to the differing degrees of their Maturity, and according to the differing kinds of Plants of the same Denomination, to observe what Operation Acid, Urinous, and Alcalizate Salts will have upon the Juices of the several sorts of the Vegetable Substances I have been mentioning.

To declare my meaning by an Example, I took from the same Cluster, one Blackberry full Ripe, and another that had not yet gone beyond a Redness, and rubbing a piece of White Paper, with the former, I observ'd, that the Juice adhering to it was of a dark Reddish Colour, full of little / Black Specks, and that this Juice by a drop of a strong *Lixivium*, was immediately turn'd into a Greenish Colour deep enough, by as much Urinous Spirit into a Colour much of Kin to the former, though somewhat differing, and fainter; and by a drop of Spirit of Salt into a fine and lightsome Red: where as the Red Berry being in like manner rubb'd upon Paper, left on it a Red Colour, which was very little alter'd by the Acid Spirit newly nam'd, and by the Urinous and Lixivate Salts receiv'd changes of Colour differing from those that had been just before produc'd in the dark Juice of the Ripe Blackberry.

I remember also, that though the Infusion of Damask-Roses would as well, though not so much, as that of Red, be heightned by Acid Spirits to an intense degree of Redness, and by Lixivate Salts be brought to a Darkish Green; yet having for Trials sake taken a Rose, whose Leaves, which were large and numerous, like those of a Province Rose, were perfectly Yellow, though in a Solution of Salt of Tartar, they afforded a Green Blewish Tincture, yet I did not by an Acid Liquor obtain a Red one; all that the Saline Spirit I imploy'd, perform'd, being (if I much mis-remember / not) to Dilute somewhat the Yellowness of the Leaves. I would also have tried the Tincture

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of Yellow Violets, but could procure none.<sup>†\*</sup> And if I were in those Islands of

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*Banda*, which are made Famous as well as Rich, by being the almost only places, where Cloves will prosper, I should think it worth my Curiosity to try, what Operation the three differing Kinds of Salts, I have so often mention'd, would have upon the Juice of this Spice, (express'd at the several



Seasons of it) as it grows upon the Tree. Since good Authors inform us, (of what is remarkable) that these whether Fruits, or Rudiments of Fruits, are at first *White*, afterward *Green*, and then *Reddish*, before they be beaten off the Tree, after which being Dry'd before they are put up, they grow *Blackish* as we see them. And one of the recentest *Herbarists* informs us, that the Flower grows upon the top of the Clove it self, consisting of four small Leaves, like a Cherry Blossom, but of an excellent *Blew*. But (*Pyrophilus*) to return to our own Observations, I shall add, that I the rather choose, to mention to you an Example drawn from Roses, because that though I am apt to think, as I elsewhere advertise, that something may be guess'd at about / some of the Qualities of the Juices of Vegetables, by the Resemblance or Disparity that we meet with in the Changes made of their Colours, by the Operation of the same kinds of Salts; yet that those Conjectures should be very warily made, may appear among other things, by the Instance I have chosen to give in Roses. For though, (as I formerly told you) the Dry'd Leaves, both of the Damask, and of Red ones, give a Red Tincture to Water sharpen'd with Acid Salts, yet the one sort of Leaves is known to have a Purgative faculty, and the other are often, and divers ways, imploy'd for Binding.†@

And I also choose (*Pyrophilus*) to subjoyn this twenty ninth Experiment to those that precede it, about the change of the Colours of Vegetables by Salts, for these two reasons: The first, that you may not easily entertain Suspitions,

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if in the Trials of an Experiment of some of the Kinds formerly mention'd, you should meet with an Event somewhat differing from what my Relations may have made you expect. And the second, That you may hereby be invited to discern, that it may not be amiss to take notice of the particular Seasons wherein you gather the Vegetables which / in Nicer Experiments you make use of. For, if I were not hindred both by haste and some justifiable Considerations, I could perhaps add considerable Instances, to those lately deliver'd, for the making out of this Observation; but for certain reasons I shall at present substitute a remarkable passage to be met with in that Laborious Herbarist Mr. *Parkinson*, where treating of the Virtues of the (already divers times mention'd) Buckthorn Berries, he subjoyns the following account of several Pigments that are made of them, not only according to the several ways of Handling them, but according to the differing Seasons of Maturity, at which they are Gather'd; *Of these Berries*, (says he) *are made three several sorts of Colours as they shall be gather'd, that is, being gather'd while they are Green, and kept Dry, are call'd Sap-berries, which being steep'd into some Allom-water, or fresh bruise'd into Allom-water, they give a reasonable fair Yellow Colour which Painters use for their Work, and Book-binders to Colour the edges of Books, and Leather-dressers to Colour Leather, as they use also to make a Green Colour, call'd Sap-green, taken from the Berries when they are Black, being bruise'd and put into a Brass or Copper Kettle or Pan, and there suffer'd to abide three or four / Days, or a little heated upon the Fire, and some beaten Allom put unto them, and afterwards press'd forth, the Juice or Liquor is usually put in great Bladders tied with strong thred at the Head and hung up untill it be Dry, which is dissolv'd in Water or Wine, but Sack (he affirms) is the best to preserve the Colour from Starving, (as they call it) that is, from Decaying, and make it hold fresh the longer. The third Colour (where of none (says he) that I can find have made mention but only Tragus) is a Purplish Colour, which is made of the Berries suffer'd to grow upon the Bushes untill the middle or end of November, that they are ready to drop from the Trees.*

And, I remember (*Pyrophilus*) that I try'd, with a success that pleas'd me well enough, to make such a kind of Pigment, as Painters call Sap-green, by a way not unlike that, deliver'd here by our Author, but I cannot now find any thing relating to that matter among my loose Papers. And my Trials were made so many years ago, that I dare not trust my Memory for Circumstances, but will rather tell you, that in a noted Colour-shop, I brought them by Questions to confess to me, that they made their Sap-green much after the ways by our *Botanist* here mention'd. And on this occasion / I shall add an Observation, which though it does not strictly belong to this place, may well enough be mention'd here, namely, that I find by an account given us by the Learned *Clusius*, of *Alaternus*, that

even the Grosser Parts of the same Plant, are some of them one Colour, and some another; For speaking of that Plant, he tells us, that the *Portugalls* use the Bark to Dye their Nets into a Red Colour, and with the Chips of the Wood, which are Whitish, they Dye a Blackish Blew.<sup>†\*</sup>

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### *Experiment XXX.*

Among the Experiments that tend to shew that the change of Colours in Bodies may proceed from the Vary'd Texture of their Parts, and the consequent change of their Disposition to Reflect or Refract the Light, that sort of Experiments must not be left unmention'd, which is afforded us by Chymical Digestions. For, if *Chymists* will believe several famous Writers about what they call the Philosophers Stone, they must acknowledge that the same Matter, seald up Hermetically in a Philosophical Egg, will by the continuance of Digestion, or if they will have it so (for it is not Material in our case which of the two it be) / of Decoction, run through a great Variety of differing Colours, before it come to that of the Noblest *Elixir*; whether that be Scarlet, or Purple, or what ever other Kind of Red. But without building any thing on so Obstruse and Questionable an Operation, (which yet may be pertinently represented to those that believe the thing) we may observe, that divers Bodies digested in carefully-clos'd Vessels, will in tract of time, change their Colour: As I have elsewhere mention'd my having observ'd ev'n in Rectify'd Spirit of Harts-horn, and as is evident in the Precipitations of Amalgams of Gold, and Mercury, without Addition, where by the continuance of a due Heat the Silver-Colour'd Amalgam is reduc'd into a shining Red Powder. Further Instances of this Kind you may find here and there in divers places of my other Essays. And indeed it has been a thing, that has much contributed to deceive many *Chymists*, that there are more Bodies than one, which by Digestion will be brought to exhibit that Variety and Succession of Colours, which they imagine to be Peculiar to what they call the *True matter of the Philosophers*. But concerning this, I shall referr you to what you may elsewhere find in the Discourse written touching the / passive Deceptions of *Chymists*,<sup>†\*</sup> and more about the Production of Colours by Digestion you will meet with presently. Wherefore I shall now make only this Observation from what has been deliver'd, That in these Operations there appears not any cause to attribute the new Colours emergent to the Action of a new Substantial form, nor to any Increase or Decrement of either the Salt, Sulphur, or Mercury of the Matter that acquires new Colours: For the Vessels are clos'd, and these Principles according to the *Chymists* are Ingenerable and Incorruptible; so that the Effect seems to proceed from hence, that the Heat agitating and shuffling the Corpuscles of the Body expos'd to it, does in process of time so change its Texture, as that the Transposed parts do Modifie the incident Light otherwise, than they did when the Matter appear'd of another Colour.

### *Experiment XXXI.*

Among the several changes of Colour, which Bodies acquire or disclose by Digestion, it is very remarkable, that *Chymists* find a Redness rather than any other Colour in most of the Tinctures they Draw, and ev'n in the more Gross

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Solutions they / make of almost all Concretes, that abound either with Mineral or Vegetable Sulphur, though the *Menstruum* imploy'd about these Solutions or Tinctures be never so Limpid or Colourless.

This we have observ'd in I know not how many Tinctures drawn with Spirit of Wine from *Jalap*, *Guaicum*, and several other Vegetables; and not only in the Solutions of *Amber*, *Benzoin*, and divers other Concretes made with the same *Menstruum*, but also in divers Mineral Tinctures. And, not to urge that familiar Instance of the Ruby of Sulphur, as *Chymists* upon the score of its Colour, call the

Solution of Flowers of Brimstone, made with the Spirit of Turpentine, nor to take notice of other more known Examples of the aptness of Chymical Oyls, to produce a Red Colour with the Sulphur they extract, or dissolve; not to insist (I say) upon Instances of this nature, I shall further represent to you, as a thing remarkable, that, both Acid and Alcalizate Salts, though in most other cases of such contrary Operations, in reference to Colours, will with many Bodies that abound with Sulphureous, or with Oyl parts, produce a Red; as is manifest partly in the more Vulgar Instances of the Tinctures, or Solutions of / Sulphur made with *Lixiviums*, either of Calcin'd Tartar or Pot-ashes, and other Obvious examples, partly by this, that the true Glass of Antimony extracted with some Acid Spirits, with or without Wine, will yield a Red Tincture, and that I know an Acid Liquor, which in a moment will turn Oyl of Turpentine into a deep Red. But among the many Instances I could give you of the easie Production of Redness by the Operation of Saline Spirit, as well as of Spirit of Wine; I remember two or three of those I have tried, which seem remarkable enough to deserve to be mention'd to you apart.

### ***Experiment XXXII.***

But before we set them down, it will not perhaps appear impertinent to premise;

That there seems to be a manifest Disparity betwixt Red Liquors, so that some of them may be said to have a Genuine Redness in comparison of others, that have a Yellowish Redness: For if you take (for example) a good Tincture of *Chochineel*, dilute it never so much with fair Water, you will not (as far as I can judge by what I have tried) be able to make it a Yellow Liquor. Insomuch that a Single / drop of a rich Solution of *Cochineel* in Spirit of Urine, being Diluted with above an Ounce of fair Water, exhibited no Yellowishness at all, but a fair (though somewhat faint) Pinck or Carnation; and even when *Cochineel* was by degrees Diluted much beyond the newly mention'd Colour, by the way formerly related to you in the twenty fourth Experiment, I remember not, that there appear'd in the whole Trial any Yellow. But if you take Balsom of Sulphur (for Instance) though it may appear in a Glass, where it has a good Thickness, to be of a deep Red, yet if you shake the Glass, or pour a few drops on a sheet of White Paper, spreading them on it with your Finger, the Balsom that falls back along the sides of the

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Glass, and that which stains the Paper, will appear Yellow, not Red. And there are divers Tinctures, such as that of Amber made with Spirit of Wine, (to name now no more) that will appear either Yellow or Red, according as the Vessels that they fill, are Slender or Broad.

### ***Experiment XXXIII.***

But to proceed to the Experiments I was about to deliver; *First*, Oyl or Spirit / of Turpentine, though clear as fair Water, being Digested upon the purely White Sugar of Lead, has, in a short time, afforded us a high Red Tincture, that some Artists are pleas'd to call the Balsom of *Saturn*, which they very much (and probably not altogether without cause) extoll as an excellent Medicine in divers Outward affections.

### ***Experiment XXXIV.***

*Next*, take of common Brimstone finely powdred five Ounces, of Sal-Armoniack likewise pulveriz'd an equal weight, of beaten Quick-lime six Ounces, mix these Powders exquisitely, and Distill them through a Retort plac'd in Sand by degrees of Fire, giving at length as intense a Heat as you well can in Sand, there will come over (if you have wrought well) a Volatile Tincture of Sulphur, which may probably prove an excellent Medicine, and should have been mention'd among the other Preparations of Sulphur, which we have elsewhere imparted to you,<sup>†\*</sup> but that it is very pertinent to



our present Subject, The change of Colours. For though none of the Ingredients be Red, the Distill'd Liquor will be so: and this Liquor if it / be well Drawn, will upon a little Agitation of the Vial first unstop'd (especially if it be held in a Warmer hand) send forth a copious Fume, not Red, like that of Nitre, but White; And sometimes this Liquor may be so Drawn, that I remember, not long since, I took pleasure to observe in a parcel of it, that Ingredients not Red, did not only yield by Distillation a Volatile Spirit that was Red, but though that Liquor did upon the bare opening of the Bottle it was kept in, drive us away with the plenty and Sulphureous sent of a White steam which it sent forth, yet the Liquor it self being touch'd by our Fingers, did immediately Dye them Black.

### *Experiment XXXV.*

The third and *last* Experiment I shall now mention to shew, how prone Bodies abounding in Sulphureous parts are to afford a Red Colour, is one, wherein by the Operation of a Saline Spirit upon a White or Whitish Body, which according to the *Chymists* should be altogether Sulphureous, a Redness may be produc'd, not (as in the former Experiments) slowly, but in the twinkling of an Eye. We took then of the Essential Oyl of Anniseeds, / which has this Peculiarity, that in Cold weather it loses its Fluidity and the

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greatest part of its Transparency, and looks like a White or Whitish Oyntment, and near at hand seems to consist of a Multitude of little soft Scales: Of this Coagulated Stuff we spread a little with a Knife upon a piece of White Paper, and letting fall on it, and mixing with it a drop or two of Oyl of Vitriol, immediately (as we fore-saw) there emerg'd together with some Heat and Smoak, a Blood-Red Colour, which therefore was in a trice produc'd by two Bodies, whereof the one had but a Whitish Colour, and the other (if carefully rectify'd) had no Colour at all.

### *Experiment XXXVI.*

But on this Occasion (*Pyrophilus*) we must add once for all, that in many of the above-recited Experiments, though the changes of Colour happen'd as we have mention'd them: yet the emergent or produc'd Colour is oft times very subject to Degenerate, both quickly and much. Notwithstanding which, since the Changes, we have set down, do happen presently upon the Operation of the Bodies upon each other, or at the times by us specify'd; / *that* is sufficient both to justifie our Veracity, and to shew what we Intend; it not being Essential to the Genuineness of a Colour to be Durable. For a fading Leaf, that is ready to Rot, and moulder into Dust, may have as true a Yellow, as a Wedge of Gold, which so obstinately resists both Time and Fire. And the reason, why I take occasion from the former Experiment to subjoyn this general Advertisement, is, that I have several times observ'd, that the Mixture resulting from the Oyls of Vitriol, and of Anniseeds, though it acquire a thicker consistence than either of the Ingredients had, has quickly lost its Colour, turning in a very short time into a dirty Gray, at least in the Superficial parts, where 'tis expos'd to the Air; which last Circumstance I therefore mention, because that, though it seem probable, that this Degeneration of Colours may oft times and in divers cases proceed from the further Action of the Saline Corpuscles, and the other Ingredients upon one another, yet in many cases much of the Quick change of Colours seems ascribeable to the Air, as may be made probable by several reasons: The first whereof may be fetcht from the newly recited Example of the two Oyls; The next may be, that we have sometimes observ'd / long Window-Curtains of light Colours, to have that part of them, which was expos'd to the Air, when the Window was open, of one Colour, and the lower part, that was sheltred from the Air by the Wall, of another Colour: And the third Argument may be fetch'd from divers Observations, both of others, and our own; For of that Pigment so well known in Painters Shops, by the name of *Turnsol*, our Industrious *Parkinson*, in the particular account he gives of the Plant that bears it, tells us also, <sup>†</sup>@ That *the Berries when they are at their full Maturity, have within them between the outer Skin and the inward Kirnel or Seed, a certain Juice or Moisture, which being*



*Green Colour, but presently changeth into a kind of Blewish Purple, upon the Cloath or Paper, and the same Cloath afterwards wet in Water, and wrung forth, will Colour the Water into a Claret Wine Colour, and these* (concludes he) *are those Raggs of Cloath, which are usually call'd Turnsol in the Druggists or Grocers Shops.* And to this Observation of our *Botanist* we will add an Experiment of our own, (made before we met with That) which, though in many Circumstances, very / differing, serves to prove the same thing; for having taken of the deeply Red Juice of *Buckthorn* Berries, which I bought of the Man that uses to sell it to the Apothecaries, to make their Syrrup *de Spina Cervina*, I let some of it drop upon a piece of White Paper, and having left it there for many hours, till the Paper was grown dry again, I found what I was inclin'd to suspect, namely, That this Juice was degenerated from a deep Red to a dirty kind of Greyish Colour, which, in a great part of the stain'd Paper seem'd not to have so much as an Eye of Red: Though a little Spirit of Salt or dissolv'd *Alcaly* would turn this unpleasant Colour (as formerly I told you it would change the not yet alter'd Juice) into a Red or Green. And to satisfie my self, that this Degeneration of Colour did not proceed from the Paper, I drop'd some of the deep Red or Crimson Juice upon a White glaz'd Tile, and suffering it to dry on there, I found that ev'n in that Body, on which it could not Soak, and by which it could not be Wrought, it nevertheless lost its Colour.<sup>†\*</sup> And these Instances (*Pyrophilus*) I am the more carefull to mention

to you, that you may not be much Surpris'd or Discourag'd, if you should sometimes miss of performing / punctually what I affirm my self to have done in point of changing Colours; since in these Experiments the over-sight or neglect of such little Circumstances, as in many others would not be perhaps considerable, may occasion the mis-carrying of a Trial. And I was willing also to take this occasion of Advertising you in the repeating of the Experiments mention'd in this Treatise, to make use of the Juices of Vegetables, and other things prepar'd for your Trials, as soon as ever they are ready, lest one or other of them grow less fit, if not quite unfit by delay; and to estimate the Event of the Trials by the Change, that is produc'd presently upon the due and sufficient Application of Actives to Passives, (as they speak) because in many cases the effects of such Mixtures may not be lasting, and the newly produc'd Colour may in a little time degenerate. But, (*Pyrophilus*) I forgot to add to the two former Observations lately made about Vegetables, a third of the same Import, made in Mineral substances, by telling you, That the better to satisfie a Friend or two in this particular, I sometimes made, according to some Conjectures of mine, this Experiment; That having dissolv'd good Silver in *Aqua-fortis*, and Precipitated it with Spirit of Salt, upon /

the first Decanting of the Liquor, the remaining Matter would be purely White; but after it had lain a while uncover'd, that part of it, that was Contiguous to the Air, would not only lose its Whiteness, but appear of a very Dark and almost Blackish Colour, I say that part that was Contiguous to the Air, because if that were gently taken off, the Subjacent part of the same Mass would appear very White, till that also, having continu'd a while expos'd to the Air, would likewise Degenerate. Now whether the Air perform these things by the means of a Subtile Salt, which we elsewhere show it not to be destitute of, or by a peircing Moisture, that is apt easily to insinuate it self into the Pores of some Bodies, and thereby change their Texture, and so their Colour: Or by solliciting the Avolation of certain parts of the Bodies, to which 'tis Contiguous; or by some other way, (which possibly I may elsewhere propose and consider) I have not now the leisure to discourse. And for the same reason, though I could add many other Instances, of what I formerly noted touching the emergency of Redness upon the Digestion of many Bodies, insomuch that I have often seen upon the Borders of *France* (and probably we may have the like in / *England*) a sort of Pears, which digested for some time with a little Wine, in a Vessel exactly clos'd, will in not many hours appear throughout of a deep Red Colour, (as also that of the Juice, wherein they are Stew'd, becomes) but ev'n on pure

and white Salt of Tartar, pure Spirit of Wine, as clear as Rock-water, will (as we elsewhere declare) by long Digestion acquire a Redness; Though I say such Instances might be Multiply'd, and though there be some other Obvious changes of Colours, which happen so frequently, that they cannot but be as well Considerable as Notorious; such as is the Blackness of almost all Bodies burn'd in the open Air: yet our haste invites us to resign you the Exercise of enquiring into the Causes of these Changes. And certainly, the reason both *why* the Soots of such differing Bodies are almost all of them all Black, *why* so much the greater part of Vegetables should be rather Green than of any other Colour, and particularly (which more directly concerns this place) *why* gentle Heats do so frequently in Chymical Operations produce rather a Redness than another Colour in digested *Menstruums*, not only Sulphureous, as Spirit of Wine, but Saline, as Spirit of Vinegar, may be very well worth / a serious Inquiry; which I shall therefore recommend to *Pyrophilus* and his Ingenious Friends.

### ***Experiment XXXVII.***

It may seem somewhat strange, that if you take the Crimson Solution of *Cochineel*, or the Juice of Black Cherries, and of some other Vegetables that afford the like Colour, (which because many take but for a deep Red, we do with them sometimes call it so) and let some of it fall upon a piece of Paper, a drop or two of an Acid Spirit, such as Spirit of Salt, or *Aqua-fortis*, will immediately turn it into a fair Red. Whereas if you make an Infusion of Brazil in fair Water, and drop a little Spirit of Salt or *Aqua-fortis* into it, that

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will destroy its Redness, and leave the Liquor of a Yellow, (sometimes Pale) I might perhaps plausibly enough say on this occasion, that if we consider the case a little more attentively, we may take notice, that the action of the Acid Spirit seems in both cases, but to weaken the Colour of the Liquor on which it falls. And so though it destroy Redness in the Tincture of Brazil, as well as produce Red in the Tincture of *Chochineel*, its Operations may be Uniform / enough, since as Crimson seems to be little else than a very deep Red, with (perhaps) an Eye of Blew, so some kinds of Red seem (as I have lately noted) to be little else than heightned Yellow. And consequently in such Bodies, the Yellow seems to be but a diluted Red. And accordingly Alcalizate Solutions and Urinous Spirits, which seem dispos'd to Deepen the Colours of the Juices and Liquors of most Vegetables, will not only restore the Solution of *Cochineel* and the Infusion of Brazil to the Crimson, whence the Spirit of Salt had chang'd them into a truer Red; but will also (as I lately told you) not only heighthen the Yellow Juice of Madder into Red, but advance the Red Infusion of Brazil to a Crimson. But I know not whether it will not be much safer to derive these Changes from vary'd Textures, than certain kinds of Bodies; and you will perhaps think it worth while, that I should add on this occasion, That it may deserve some Speculation, why, notwithstanding what we have been observing, though Blew and Purple seem to be deeper Colours than Red, and therefore the Juices of Plants of either of the two former Colours may (congruously enough to what has been just now noted) be turn'd Red by / Spirit of Salt or *Aqua-fortis*, yet Blew Syrrup of Violets and some Purples should both by Oyl of Tartar and Spirit of Urine be chang'd into Green, which seems to be not a deeper but a more diluted Colour than Blew, if not also than Purple.

### ***Experiment XXXVIII.***

It would much contribute to the History of Colours, if *Chymists* would in their Laboratories take a heedfull notice, and give us a faithfull account of the Colours observ'd in the Steams of Bodies either Sublim'd or Distill'd, and of the Colours of those Productions of the Fire, that are made up by the Coalition of those Steams. As (for Instance) we observe in the Distilling of pure Salt-peter, that at a certain season of the Operation, the Body, though it seem either Crystalline, or White, affords very Red Fumes: whereas though Vitriol be Green or Blew, the Spirit of it is observ'd to come over

in Whitish Fumes. The like Colour I have taken notice of in the Fumes of several other Concretes of differing Colours, and Natures, especially when Distill'd with strong Fires. And we elsewhere note, that ev'n Soot, as Black as it is, has fill'd our Receivers / with such copious White Fumes, that they seem'd to have had their In-sides wash'd with Milk. And no less observable may be, the Distill'd Liquors, into which such Fumes convene, (for though we will not deny, that by skill and care a Reddish Liquor may be obtain'd from Nitre) yet the common Spirit of it, in the making ev'n of which store of these Red Fumes

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are wont to pass over into the Receiver, appears not to be at all Red. And besides, that neither the Spirit of Vitriol, nor that of Soot is any thing White; And, besides also, that as far as I have observ'd, most (for I say not all) of the Empyreumatical Oyls of Woods, and other Concretes, are either of a deep Red, or of a Colour between Red and Black; besides this, I say, 'tis very remarkable, that notwithstanding that great Variety of Colours to be met with in the Herbs, Flowers, and other Bodies wont to be Distill'd in *Balneo*: yet (as far at least as our common Distillers Experience reacheth) all the Waters and Spirits that first come over by that way of Distillation, leave the Colours of their Concretes behind them, though indeed there be one or two Vegetables not commonly taken notice of, whose Distill'd Liquors I elsewhere observe to carry over / the Tincture of the Concrete with them. And as in Distillations, so in Sublimations, it were worth while to take notice of what comes up, in reference to our present scope, by purposely performing them (as I have in some cases done) in conveniently shap'd Glasses, that the Colour of the ascending Fumes may be discern'd; For it may afford a Naturalist good Information to observe the Congruities or the Differences betwixt the Colours of the ascending Fumes, and those of the *Flowers*, they compose by their Convention. For it is evident, that these *Flowers*, do many of them in point of Colour, much differ, not only from one another, but oft times from the Concretes that afforded them. Thus, (not here to repeat what I formerly noted of the Black Soots of very differing Colour'd Bodies) though Camphire and Brimstone afford *Flowers* much of their own Colour, save that those of Brimstone are wont to be a little Paler, than the Lumps that yielded them; yet ev'n of Red *Benzoin*, that sublim'd Substance, which *Chymists* call its *Flowers*, is wont to be White or Whitish. And to omit other Instances, ev'n one and the same Black Mineral, Antimony, may be made to afford *Flowers*, some of them Red, and some Grey, and, which is more strange, / some of them purely White. And 'tis the Prescription of some Glass-men by exquisitely mingling a convenient proportion of Brimstone, Sal-Armoniack, and Quick-silver, and Subliming them together, to make a Sublimate of an excellent Blew; and though having caus'd the Experiment to be made, we found the produc'd Sublimate to be far from being of a lovely Colour, (as was promis'd) that there and there, it seem'd Blewish, and at least was of a Colour differing enough from either of the Ingredients, which is sufficient for our present purpose. But a much finer Colour is promis'd by some of the Empiricks, that pretend to Secrets, who tell us, that Orpiment, being Sublim'd, will afford among the Parts of it that fly Upward, some little Masses, which, though the Mineral it self be of a good Yellow, will be Red enough to emulate Rubies, both in Colour and Translucency. And this Experiment may, for ought I know, sometimes succeed; for I remember, that having in a small Bolt-head purposely sublim'd some powder'd Orpiment, we could in the Lower part of the Sublimate discern here and there some Reddish Lines, though much of the Upper part of the Sublimate consisted of a matter, which was not alone purely / Yellow, but transparent almost like a

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Powder. And we have also this way obtain'd a Sublimate, the Lower part whereof though it consisted not of Rubies, yet the small pieces of it, which were Numerous enough, were of a pleasant Reddish Colour, and Glister'd very prettily. But to insist on such kind of Trials and Observations, (where the ascending Fumes of Bodies differ in Colour from the Bodies themselves) though it might indeed Inrich the History of Colours, would Robb me of too much of the little time I have to dispatch what I have further to tell you concerning them.

### *Experiment XXXIX.*

Take the dry'd Buds (or Blossoms) of the Pomegranate Tree, (which are commonly call'd in the Shops *Balaustiums*) pull off the Reddish Leaves, and by a gentle Ebbullition of them in fair Water, or by a competent Infusion of them in like Water well heated, extract a faint Reddish Tincture, which if the Liquor be turbid, you may Clarifie it by Filtrating it. Into this, if you pour a little good Spirit of Urine, or some other Spirit abounding in the like sort of Volatile Salts, the Mixture will / presently turn of a dark Greenish Colour; but if instead of the fore-mention'd Liquor, you drop into the simple Infusion a little rectify'd Spirit of Sea-Salt, the Pale and almost Colourless Liquor will immediately not only grow more Transparent, but acquire a high Redness, like that of Rich Claret Wine, which so suddenly acquir'd Colour, may as quickly be Destroy'd and turn'd into a dirty Blewish Green, by the affusion of a competent quantity of the above-mention'd Spirit of Urine.

### *Annotation.*

This Experiment may bring some Light to, and receive some from a couple of other Experiments, that I remember I have met with in the Ingenious *Gassendus's* Animadversions upon *Epicurus's* Philosophy, whilst I was turning over the Leaves of those Learned Commentaries;†\* (my Eyes being too weak to let me read such Voluminous Books quite thorough). And I the less scruple (notwithstanding my contrary Custom in this Treatise) to set down these Experiments of another, because I shall a little improve the latter of them, and because by comparing there with that which I have last recited, we may be assisted to Conjecture / upon what account it is, that Oyl of Vitriol heightens the Tincture of Red-rose Leaves, since Spirit of Salt, which is a highly Acid *Menstruum*, but otherwise differing enough from Oyl of Vitriol, does the same thing. Our Authors Experiments then, as we made them, are these; We took about a Glass-full of luke-warm Water, and in it immerg'd a quantity of the Leaves of *Senna*, and presently upon the Immersion there did not appear any Redness in the Water, but dropping into it a little Oyl of

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Tartar, the Liquor soon discover'd a Redness to the watchfull Eye, whereas by a little of that Acid Liquor of Vitriol, which is like the former, undeservedly called Oyl, such a Colour would not be extracted from the infused *Senna*. On the other side we took some Red-rose Leaves dry'd, and having shaken them into a Glass of fair Water, they imparted to it no Redness, but upon the affusion of a little Oyl of Vitriol the Water was immediately turn'd Red, which it would not have been, if instead of Oyl of Vitriol, we had imployed Oyl of Tartar to produce that Colour: That these were *Gassendus* his Experiments, I partly remember, and was assur'd by a Friend, who lately Transcribed them out of *Gassendus* his Book, which I / therefore add, because I have not now that Book at hand. And the design of *Gassendus* in these Experiments our Friend affirms to be, to prove, that of things not Red a Redness may be made only by Mixture, and the Varied position of parts, wherein the Doctrine of that Subtil Philosopher doth not a little Authorize, what we have formerly delivered concerning the Emergency and Change of Colours. But the instances, that we have out of him set down, seem not to be the most Eminent, that may be produced of this truth: For our next Experiment will shew the production of several Colours out of Liquors, which have not any of them any such Colour, nor indeed any discernable one at all; and whereas though our Author tells us, that there was no Redness either in the Water, or the Leaves of *Senna*, or the Oyl of Tartar; And though it be true, that the Predominant Colour of the Leaves of *Senna* be another than Red, yet we have try'd, that by steeping that Plant a Night even in Cold water, it would afford a very deep Yellow or Reddish Tincture without the help of the Oyl of Tartar, which seems to do little more than assist the Water to extract more nimbly a plenty of that Red Tincture, wherewith the Leaves of *Senna* / do of themselves abound, and having taken off the Tincture of *Senna*, made only with fair Water, before it grew to be Reddish, and Decanted it from the Leaves, we could not perceive, that by dropping some Oyl of Tartar into it, that Colour was considerable, though it were a little heightned into a Redness,



which might have been expected, if the particles of the Oyl did eminently Co-operate, otherwise than we have expressed, to the production of this Redness.

And as for the Experiment with Red-rose Leaves, the same thing may be alleged, for we found that such Leaves by bare Infusion for a Night and Day in fair Water, did afford us a Tincture bordering at least upon Redness, and that Colour being conspicuous in the Leaves themselves, would not by some seem so much to be produc'd as to be extracted by the affusion of Oyl of Vitriol. And the Experiment try'd with the dry'd Leaves of Damask-roses succeeded but imperfectly, but that is indeed observable to our Authors purpose, that Oyl of Tartar will not perform in this Experiment what Oyl of Vitriol doth; but because this last named Liquor is not so easily to be had, give me leave to Advertise you, that the Experiment will succeed, / if instead of it you imploy *Aqua-fortis*. And though some Trials of our own formerly made, and others easily deducible from what we have already deliver'd, about the

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different Families and Operations of Salt, might enable us to present you an Experiment upon Red-rose Leaves, more accommodated to our Authors purpose, than that which he hath given us; yet our Reverence to so Candid a Philosopher, invites us rather to improve his Experiment, than substitute another in its place. Take therefore of the Tincture of Red-rose Leaves, (for with Damask-rose Leaves the Experiment succeedeth not well) made as before hath been taught with a little Oyl of Vitriol, and a good quantity of fair Water, pour off this Liquor into a clear Vial, half fill'd with Limpid water, till the Water held against the Light have acquir'd a competent Redness, without losing its Transparency, into this Tincture drop leisurely a little good Spirit of Urine, and shaking the Vial, which you must still hold against the Light, you shall see the Red Liquor immediately turn'd into a fine Greenish Blew, which Colour was not to be found in any of the Bodies, upon whose Mixture it emerg'd, and this Change is the more observable, because in many Bodies / the Degenerating of Blew into Red is usual enough, but the turning of Red into Blew is very unfrequent. If at every drop of Spirit of Urine you shake the Vial containing the Red Tincture, you may delightfully observe a pretty variety of Colours in the passage of that Tincture from a Red to a Blew, and sometimes we have this way hit upon such a Liquor, as being look't upon against and from the Light, did seem faintly to emulate the above-mention'd Tincture of *Lignum Nephriticum*.<sup>†\*</sup> And if you make the Tincture of Red-roses very high, and without Diluting it with fair Water, pour on the Spirit of Urine, you may have a Blew so deep, as to make the Liquor Opacous, but being dropt upon White Paper the Colour will soon disclose it self. Also having made the Red, and consequently the Blew Tincture very Transparent, and suffer'd it to rest in a small open Vial for a Day or two, we found according to our Conjecture, that not only the Blew but the Red Colour also was Vanish'd; the clear Liquor being of a bright Amber Colour, at the bottom of which subsided a Light, but Copious feculency of almost the same Colour, which seems to be nothing but the Tincted parts of the Rose Leaves drawn out by the Acid / Spirits of the Oyl of Vitriol, and Precipitated by the Volatile Salt of the Spirit of Urine, which makes it the more probable, that the Redness drawn by the Oyl of Vitriol, was at least as well an extraction of the Tinging parts of the Roses, as a production of Redness; and lastly, if you be destitute of Spirit of Urine, you may change the Colour of the Tincture of Roses with many other Sulphureous Salts, as a strong Solution of Pot-ashes, Oyl of Tartar, &c. which yet are seldome so free from Feculency, as the Spirituous parts of Urine becomes by repeated Distillation.

### *Annotation.*

On this occasion, I call to mind, that I found, a way of producing, though not the same kind of Blew, as I have been mentioning, yet a Colour near of

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Kin to it, namely, a fair Purple, by imploying a Liquor not made Red by Art, instead of the Tincture of Red-roses, made within an Acid Spirit; And my way was only to take Log-wood, (a Wood very well known to Dyers) having by Infusion the Powder of it a while in fair Water made that Liquor Red, I dropt into it a *Tantillum*<sup>†\*</sup> of an Urinous Spirit, as that of Sal-Armoniack, / (and I have done the same thing with an *Alcali*) by which the Colour was in a moment turn'd into a Rich, and lovely Purple. But care must be had, that you let not fall into a Spoonfull above two or three Drops, lest the Colour become so deep, as to make the Liquor too Opacous. And (to answer the other part of *Gassendus* his Experiment) if instead of fair Water, I infus'd the Log-wood in Water made somewhat sour by the Acid Spirit of Salt, I should obtain neither a Purple Liquor, nor a Red, but only a Yellow one.

### *Experiment XL.*

The Experiment I am now to mention to you, *Pyrophilus*, is that which both you, and all the other *Virtuosi* that have seen it, have been pleas'd to think very strange; and indeed of all the Experiments of Colours, I have yet met with, it seems to be the fittest to recommend the Doctrine propos'd in this Treatise, and to shew that we need not suppose, that all Colours must necessarily be Inherent Qualities, flowing from the Substantial Forms of the Bodies they are said to belong to, since by a bare Mechanical change of Texture in the Minute parts of Bodies, two Colours may in / a moment be Generated quite *De novo*, and utterly Destroy'd. For there is this difference betwixt the following Experiment, and most of the others deliver'd in these Papers, that in this, the Colour that a Body already had, is not chang'd into another, but betwixt two Bodies, each of them apart devoid of Colour, there is in a moment generated a very deep Colour, and which if it were let alone, would be permanent; and yet by a very small Parcel of a third Body, that has no Colour of its own, (lest some may pretend I know not what Antipathy betwixt Colours) this otherwise permanent Colour will be in another trice so quite Destroy'd, that there will remain no foot-steps either of it or of any other Colour in the whole Mixture.

The Experiment is very easie, and it is thus perform'd: Take good common Sublimate, and fully satiate with it what quantity of Water you please, Filtre the Solution carefully through clean and close Paper, that it may drop down as Clear and Colourless as Fountain water. Then when you'll shew the Experiment, put of it about a Spoonfull into a small Wine-glass, or any other convenient Vessel made of clear Glass, and dropping in three or four / drops of good Oyl of Tartar, *per Deliquium*, well Filtred that it may likewise be without Colour, these two Limpid Liquors will in the twinkling of an Eye turn into an Opacous mixture of a deep Orange Colour, which by keeping the Glass continually shaking in your hand, you must preserve from settling too soon to

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the Bottom; And when the Spectators have a little beheld this first Change, then you must presently drop in about four or five drops of Oyl of Vitriol, and continuing to shake the Glass pretty strongly, that it may the Nimbler diffuse it self, the whole Colour, if you have gone Skilfully to work, will immediately disappear, and all the Liquor in the Glass will be Clear and Colourless as before, without so much as a Sediment at the Bottom. But for the more gracefull Trial of this Experiment, 'twill not be amiss to observe, First, That there should not be taken too much of the Solution of Sublimate, nor too much of the Oyl of Tartar drop'd in, to avoid the necessity of putting in so much Oyl of Vitriol as may make an Ebullition, and perhaps run over the Glass. Secondly, That 'tis convenient to keep the Glass always a little shaking, both for the better mixing of the Liquors, and to keep the Yellow Substance from Subsiding, which / else it would in a short time do, though when 'tis subsided it will retain its Colour, and also be capable of being depriv'd of it by the Oyl newly mention'd. Thirdly, That if any Yellow matter stick at the sides of the Glass, 'tis but inclining the Glass, till the clarify'd Liquor can wash alongst it, and the Liquor will presently imbibe it, and deprive it of its Colour.

Many have somewhat wondred, how I came to light upon this Experiment, but the Notions or Conjectures I have about the differing Natures of the several Tribes of Salts, having led me to devise the Experiment, it will not be difficult for me to give you the Chymical Reason, if I may so speak, of the *Phænomenon*. Having then observ'd, that *Mercury* being dissolv'd in some *Menstruums*, would yield a dark Yellow Precipitate, and supposing that, as to this, common Water, and the Salts that stick to the *Mercury* would be equivalent to those Acid *Menstruums*, which work upon the *Quick-silver*, upon the account of their Saline particles, I substituted a Solution of Sublimate in fair Water, instead of a Solution of *Mercury* in *Aqua-fortis*, or Spirit of *Nitre*, that simple Solution being both clearer and free from that very offensive smell, which accompanies the Solutions / of *Mercury* made with those other corrosive Liquors; then I consider'd, that That, which makes the Yellow Colour, is indeed but a Precipitate made by the means of the Oyl of Tartar, which we drop in, and which, as *Chymists* know, does generally precipitate Metalline Bodies corroded by Acid Salts; so that the Colour in our case results from the Coalition of the Mercurial particles with the Saline ones, wherewith they were formerly associated, and with the Alcalizate particles of the Salt of Tartar that swim up and down in the Oyl. Wherefore considering also, that very many of the effects of Lixivate Liquors, upon the Solutions of other Bodies, may be destroy'd by Acid *Menstruums*, as I elsewhere more particularly declare, I concluded, that if I chose a very potently Acid Liquor, which by its Incisive power might undo the work of the Oyl of Tartar, and disperse again those Particles, which the other had by Precipitation associated, into such minute Corpuscles as were before singly Inconspicuous, they would become Inconspicuous again, and consequently leave the Liquor as Colourless as before the Precipitation was made.

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This, as I said, *Pyrophilus*, seems to be the Chymical reason of this Experiment, that / is such a reason, as, supposing the truth of those Chymical Notions I have elsewhere I hope evinc'd, may give such an account of the *Phænomena* as Chymical Notions can supply us with; but I both here and elsewhere make use of this way of speaking, to intimate that I am sufficiently aware of the difference betwixt a Chymical Explication of a *Phænomenon*, and one that is truly Philosophical or Mechanical; as in our present case, I tell you something, when I tell you that the Yellowness of the Mercurial Solution, and the Oyl of Tartar is produc'd by the Precipitation occasion'd by the affusion of the latter of those Liquors, and that the destruction of the Colour proceeds from the Dissipation of that Curdl'd matter, whose Texture is destroy'd, and which is dissolv'd into Minute and Invisible particles by the potently Acid *Menstruum*, which is the reason, why there remains no Sediment in the Bottom, because the infused Oyl takes it up, and resolves it into hidden or invisible Parts, as Water does Salt or Sugar. But when I have told you all this, I am far from thinking I have told all that such an Inquisitive Person as your self would know, for I presume you would desire as well as I to learn (at least) why the Particles of the / *Mercury*, of the Tartar, and of the Acid Salts convening together, should make rather an Orange Colour than a Red, or a Blew, or a Green, for 'tis not enough to say what I related a little before, that divers Mercurial Solutions, though otherwise made, would yield a Yellow precipitate, because the Question will recurr concerning them; and to give it a satisfactory answer, is, I freely acknowledge, more than I dare as yet pretend to.

But to confirm my conjecture about the Chymical reason of our Experiment, I may add, that as I have (*viz.* pag. 34th. of this Treatise<sup>†\*</sup>) elsewhere (on another occasion) told you, with Saline Liquors of another kind and nature than Salt of Tartar, (namely, with Spirit of Urine, and Liquors of kin to that) I can make the *Mercury* precipitate out of the first simple Solution quite of another Colour than that hitherto mention'd; Nay, if instead of altering the Precipitating Liquor, I alter'd the Texture of the Sublimate in such a way as my Notions about Salt requir'd, I could produce the same *Phænomenon*. For having purposely Sublim'd together Equal parts (or thereabout) of Sal-Armoniack and Sublimate, first diligently Mix'd, the ascending Flowers being dissolv'd in fair Water, / and Filtred, gave a Solution Limpid and Colourless, like that of the other Sublimates, and yet an *Alcaly* drop'd into this Liquor did not turn it Yellow but White. And upon the same Grounds we may with *Quick-silver*, without the help of common Sublimate, prepare another sort of Flowers

dissoluble in Water without Discolouring it, with which I could likewise do what I newly mention'd; to which I shall add, (what possibly you'l somewhat wonder at) That so much does the Colour depend upon the Texture resulting from the Convention of the several sorts of Corpuscles, that though in our Experiment, Oyl of Vitriol destroys the Yellow Colour, yet with *Quick-silver* and fair Water, by the help of Oyl of

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Vitriol alone, we may easily make a kind of Precipitate of a fair and permanent Yellow, as you will e're long (in the forty second Experiment of this third Part) be taught. And I may further add, that I chose Oyl of Vitriol, not so much for any other or peculiar Quality, as for its being, when 'tis well rectify'd, (which 'tis somewhat hazardous to bring it to be) not only devoid of Colour and in Smells, but extremely Strong and Incisive; For though common and undeplegmed *Aqua-fortis* will not perform / the same thing well, yet that which is made exceeding Strong by being carefully Deplegm'd, will do it pretty well, though not so well as Oyl of Vitriol which is so Strong, that even without Rectification it may for a need be made use of. I will not here tell you what I have try'd, that I may be able to deprive at pleasure the Precipitate that one of the Sulphureous Liquors had made, by the copious Affusion of the other: Because I found, though this Experiment is too ticklish to let me give a full account of it in few words, I shall therefore tell you, that it is not only for once, that the other above-mention'd Experiment may be made, the same Numerical parcels of Liquor being still employ'd in it; for after I have Clarify'd the Orange Colour'd Liquor, by the addition of as little of the Oyl of Vitriol as will suffice to perform the effect, I can again at pleasure re-produce the Opacous Colour, by the dropping in of fresh Oyl of Tartar, and destroy it again by the Re-affusion of more of the Acid *Menstruum*; and yet oftner if I please, can I with these two contrariant Liquors recall and disperse the Colour, though by reason of the addition of so much new Liquor, in reference to the Mercurial particles, the Colour will at length appear more dilute and faint. /

#### *An improvement of the fortieth Experiment.*

And, *Pyrophilus*, to confirm yet further the Notions that led me to think on the propos'd Experiment, I shall acquaint you with another, which when I had conveniency I have sometimes added to it, and which has to the Spectators appear'd little less Odd than the first; And though because the Liquor, requisite to make the Trial succeed well, must be on purpose prepar'd anew a while before, because it will not long retain its fitness for this work, I do but seldome annex this Experiment to the other, yet I shall tell you how I devis'd it, and how I make it. If you boyl Crude Antimony in a strong and clear *Lixivium*, you shall separate a Substance from it, which some Modern *Chymists* are pleas'd to call its Sulphur, but how deservedly I shall not here examine, having elsewhere done it in an Opportune place; wherefore I shall now but need to take notice, that when this suppos'd Sulphur (not now to call it rather a kind of *Crocus*) is let fall by the Liquor upon its Refrigeration, it often settles in Flakes, or such like parcels of a Yellow Substance, (which being by the precedent / dissolution reduc'd into Minute parts, may peradventure be made to take Fire much more easily than the Grosser Powder of unprepar'd Antimony would have done.) Considering therefore, that common Sulphur boyl'd in a *Lixivium* may be Precipitated out of it by Rhenish-wine or

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White-wine, which are Sowrish Liquors, and have in them, as I elsewhere shew, an Acid Salt; and having found also by Trial, that with other Acid Liquors I could Precipitate out of Lixivate Solvents some other Mineral concretions abounding with Sulphureous parts, of which sort is crude Antimony, I concluded it to be easie to Precipitate the Antimony dissolv'd, as was lately mention'd, with the Acid Oyl of Vitriol; and though common Sulphur yields a White Precipitate, which the *Chymists* call *Lac Sulphuris*, yet I suppos'd the Precipitated Antimony would be of a deep Yellow Colour, as well, if made with Oyl of Vitriol, as if made only by Refrigeration and length of Time. From this 'twas easie to deduce this Experiment, that if you put into one Glass some of the freshly

Impregnated and Filtrated Solution of Antimony, and into another some of the Orange-Colour'd Mixture, (which I formerly shew'd you how to make with a / Mercurial Solution and Oyl of Tartar) a few drops of Oyl of Vitriol dropp'd into the last mention'd Glass, would, as I told you before, turn the Deep Yellow mixture into a Cleer Liquor; whereas a little of the same Oyl dropp'd out of the same Viol into the other Glass would presently (but not without some ill sent) turn the moderately cleer Solution into a Deep Yellow Substance. But this, as I said, succeeds not well, unless you employ a *Lixivium* that has but newly dissolv'd Antimony, and has not yet let it fall. But yet in Summer time, if your *Lixivium* have been duly Impregnated and well Filtred after it is quite cold, it will for some dayes (perhaps much longer than I had occasion to try) retain Antimony enough to exhibit, upon the Affusion of the Corrosive Oyl, as much of a good Yellow Substance as is necessary to satisfie the Beholders of the Possibility of the Experiment.

### *Reflections upon the XL. Experiment Compared with the X. and XX.*

The Knowledge of the Distinction of Salts which we have propos'd, whereby they are discriminated into *Acid*, *Volatile*, / or *Salsuginous* (if I may for Distinction sake so call the Fugitive Salts of Animal Substances) and *fix'd* or *Alcalizate*, may possibly (by that little part which we have already deliver'd, of what we could say of its Applicableness) appear of so much Use in Natural Philosophy (especially in the Practick part of it) that I doubt not but it will be no Unwelcome Corollary of the Preceding Experiment, if by the help of it I teach you to distinguish, which of those Salts is Predominant in Chymical Liquors, as well as whether any of them be so or not. For though in our Notes upon the X. and XX. Experiments I have shown you a way by means of the Tincture of *Lignum Nephriticum*, or of Syrrup of Violets, to discover whether a propounded Salt be Acid or not, yet you can thereby only find in general that such and such Salts belong not to the Tribe of Acids, but cannot determine whether they belong to the Tribe of Urinous Salts (under which for distinction sake I comprehend all those Volatile Salts of Animal or other

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Substances that are contrary to Acids) or to that of Alcalies. For as well the one as the other of these Salino-Sulphurous Salts will restore the Cæruleous Colour to the Tincture of *Lignum Nephriticum*, and turn that of Syrrup of Violets / into Green. Wherefore this XL. Experiment does opportunely supply the deficiency of those. For being solicitous to find out some ready wayes of discriminating the Tribes of Chymical Salts, I found that all those I thought fit to make Tryal of, would, if they were of a Lixivate Nature, make with Sublimate dissolv'd in Fair Water an *Orange Tawny* Precipitate; whereas if they were of an Urinous Nature the Precipitate would be *White* and Milky. So that having alwayes by me some Syrrup of Violets and some Solution of Sublimate, I can by the help of the first of those Liquors discover in a trice, whether the propounded Salt or Saline Body be of an Acid Nature or no, if it be I need (you know) inquire no further; but if it be not, I can very easily, and as readily distinguish between the other two kinds of Salts, by the White or Orange-Colour that is immediately produc'd, by letting fall a few Drops or Grains of the Salt to be examin'd, into a spoonfull of the cleer Solution of Sublimate. For Example, it has been suppos'd by some eminently Learned, That when Sal Armoniack being mingled with an Alcaly is forc'd from it by the Fire in close Vessels, the Volatile Salt that will thereby be obtain'd (if the Operation be skilfully perform'd,) / is but a more fine and subtile sort of Sal Armoniack, which, 'tis presum'd, this Operation do's but more exquisitely purifie, than common Solutions, Filtrations, and Coagulations. But this Opinion may be easily shown to be Erroneous, as by other Arguments, so particularly by the lately deliver'd Method of distinguishing the Tribes of Salts. For the Saline Spirit of Sal Armoniack, as it is in many other manifest Qualities very like the Spirit of Urine, so like, that it will in a trice make Syrrup of Violets of a Lovely Green, turn a Solution of good Verdigrease into an Excellent Azure, and make the Solution of a Sublimate yield a White Precipitate, insomuch that in most (for I say not all of the Experiments) where I Aim onely at producing a sudden change of Colour, I scruple not to use Spirit of Sal Armoniack when it is at hand, instead of Spirit of Urine, as indeed it seems chiefly to consist (besides the flegm that helps to make it fluid) of the Volatile



Urinous Salt (yet not excluding that of Soot) that abounds in the Sal Armoniack and is set at liberty from the Sea Salt wherewith it was formerly associated, and clogg'd, by the Operation of the Alcaly, that divides the Ingredients of Sal Armoniack, and retains that Sea Salt with it self. What use may be / made of the like way of exploration in that inquiry which puzzles so many Modern Naturalists, whether the Rich Pigment (which we have often had occasion to mention) belongs to the Vegetable or Animal Kingdome, you may find in another place where I give you some account of what I try'd about Cochineel. But I think it needless to exemplifie here our Method by any other Instances, many such being to be met with in divers parts of this Treatise; but I will rather advertise you, that, by this way of examining Chymical Liquors, you may not onely in most Cases conclude *Affirmatively*, but in some Cases *Negatively*. As since Spirit of Wine,

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and as far as I have try'd, those Chymical Oyles which Artists call Essential, did not (when I us'd them as I had us'd the several Families of Salts upon that Syrrup) turn Syrrup of Violets Red or Green, nor the Solution of Sublimate White or Yellow, I inferr'd it may thence be probably argued, that either they are destitute of Salt, or have such as belongs not to either of the three Grand families already often mention'd.

When I went to examine the Spirit of Oak or of such like Concretes forced over through a Retort, I found by this means amongst others, that (as I elsewhere show) those Chymists are / much mistaken in it, that account it a simple Liquor, and one of their Hypostatical Principles:<sup>†</sup>\* for not to mention what flegm it may have, I found that with a few drops of one of this sort of Spirits mix'd with a good proportion of Syrrup of Violets, I could change the Colour and make it Purplish, by the affinity of which Colour to Redness, I conjectur'd that this Spirit had some Acid Corpuscles in it, and accordingly I found that as it would destroy the Blewness of a Tincture of *Lignum Nephriticum*, so being put upon Corals it would Corrode them, as common Spirit of Vinegar, and other Acid Liquors are wont to do. And farther to examine whether there were not a great part of the Liquor that was not of an Acid nature, having separated the Sour or Vinegar-like part from the rest, which (if I mistake not) is far the more Copious, we concluded as we had conjectured, the other or remaining part, though it had a strong taste as well as smell, to be of a nature differing from that of either of the three sorts of Salts above mention'd, since it did as little as Spirit of Wine, and Chymical Oyls, alter the Colour either of Syrrup of Violets or Solution of Sublimate, whence we also inferr'd that the change that had been made of that Syrrup into a Purple Colour, was / effected by the Vinegar, that was one of the two Ingredients of the Liquor, which was wont to pass for a Simple or Uncompounded Spirit. And, upon this account, 'twas of the Spirit of Oak (and the like Concretes) freed from it's Vinegar that I elsewhere told you, that I had not then observ'd it, (and I have repeated the Tryal but very lately) to destroy the Cæruleous Tincture of *Lignum Nephriticum*. But this onely, *en passant*; for the Chief thing I had to add was this, That by the same way may be examin'd and discover'd, divers changes that are produc'd in Bodies either by Nature only, or by Art; either of them, being able by changing the Texture of some Concretes I could name, to qualifie them to Operate after a New manner upon the above mention'd Syrrup, or Solution, or both. And by this means, to tell you that upon the by, I have been able to discover, that there may be made Bodies, which though they run *per Deliquium*, as readily as Salt of Tartar, belong in other respects, not to the family of Alcaliz, much less to that of Salsuginous, or that of Acid Salts. Perhaps too, I may know a way of making a highly operative Saline Body that shall neither change the Colour of Syrrup of Violets, nor Precipitate the Solution of Sublimate; And, I can / likewise if I please conceal by what Liquors I perform such changes of Colour, as I have been mentioning

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to you, by quite altering the Texture of some ordinary Chymical productions, the Exploration of which is the main use of the fortieth Experiment, which I think teaches not a little, if it teach us to discover the nature of those things (in reference to Salt) that are obtain'd by the ordinary Chymical Analysis of mix'd Bodyes, though perhaps there may be other Bodyes prepar'd by Chymistry which



may have the same Effects in the change of Colours; and yet be produc'd not from what Chymists call the Resolution of Bodies, but from their Composition. But the discoursing of things of this nature is more proper for another place. I shall now onely add, what might perhaps have been more seasonably told you before; That the Reason why the way of Exploration of Salts hitherto deliver'd, succeeds in the Solution of Sublimate, depends upon the particular Texture of that Solution, as well as upon the differing Natures of the Saline Liquors imploy'd to Precipitate it. For Gold dissolv'd in *Aqua Regia*, whether you Precipitate it with Oyl of Tartar which is an Alcaly, or with Spirit of Urine, or Sal Armoniack which belongs to the family of / Volatile Salts, will either way afford a Yellow substance: though with such an Acid Liquor, as, I say not Spirit of Salt, the Body that yields it, being upon the matter an Ingredient of *Aqua Regis*, but Oyl of Vitriol it self, I did not find that I could Precipitate the Metall out of the Solution, or destroy the Colour of it, though the same Oyl of Vitriol would readily Precipitate Silver dissolv'd in *Aqua-fortis*. And if you dissolve pure Silver in *Aqua-fortis*, and suffer it to shoot into Crystals, the cleer solution of these made in fair Water, will afford a very White Precipitate, whether it be made with an Alcaly, or an Acid Spirit, as that of Salt; whereas, which may seem somewhat strange, with Spirit of Sal Armoniack (that I us'd was made of Quick-lime) I could obtain no such White Precipitate; that Volatile Spirit, nor (as I remember) that of Urine, scarce doing any more than striking down a very small quantity of Matter, which was neither White nor Whitish, so that the remaining Liquor being suffer'd to evaporate till the superfluous Moisture was gone, the greatest part of the Metalline Corpuscles with the Saline ones that had imbib'd them, concoagulated into Salt, as is usual in such Solutions, wherein the Metall has not been Precipitated. /

### ***Experiment XLI.***

Of Kin to the last or fortieth Experiment is another which I remember I have sometimes shewn to *Virtuosi* that were pleas'd not to dislike it. I took Spirit of Urine made by Fermentation, and with a due proportion of Copper brought into small parts, I obtain'd a very lovely Azure Solution, and when I saw the Colour was such as was requisite, pouring into a clean Glass, about a spoonfull of this tinted Liquor, (of which I us'd to keep a Quantity by me,) I could by shaking into it some drops of Strong Oyl of Vitriol, deprive it in a trice of its Deep Colour, and make it look like Common-water.

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### ***Annotation.***

This Experiment brings into my mind this other, which oftentimes succeeds well enough, though not quite so well as the former; Namely, that if into about a small spoonfull of a Solution of good French Verdigrease made in fair Water, I drop't and shak'd some strong Spirit of Salt, or rather deflegm'd *Aqua Fortis*, the Greenness of the Solution would be made in a trice almost / totally to disappear, & the Liquor held against the Light would scarce seeme other than Cleer or Limpid, to any but an Attentive Eye, which is therefore remarkable, because we know that *Aqua-fortis* corroding Copper, which is it that gives the Colour to Verdigrease, is wont to reduce it to a Green Blew Solution. But if into the other altogether or almost Colourless Liquor I was speaking of, you drop a just quantity either of Oyl of Tartar or Spirit of Urine, you shall find that after the Ebullition is ceas'd, the mixture will disclose a lively Colour, though somewhat differing from that which the Solution of Verdigrease had at first.

### ***Experiment XLII.***

That the Colour (*Pyrophilus*) of a Body may be chang'd by a Liquor which of it self is of no Colour, provided it be Saline, we have already manifested by a multitude of instances. Nor doth it seem so strange, because Saline Particles swimming up and down in Liquors, have been by many observ'd

to be very operative in the Production and change of Colours. But divers of our Friends that are not acquainted with Chymical Operations have thought it very strange that a White Body, and a Dry one / too, should immediately acquire a rich new Colour upon the bare affusion of Spring-Water destitute as well of adventitious Salt as of Tincture. And yet (*Pyrophilus*) the way of producing such a change of Colours may be easily enough lighted on by those that are conversant in the Solutions of Mercury. For we have try'd, that though by Evaporating a Solution of Quick-Silver in *Aqua-fortis*, and abstracting the Liquor till the remaining matter began to be well, but not too strongly dried, fair Water pour'd on the remaining *Calx* made it but somewhat Yellowish; yet when we took good Quick-Silver, and three or four times its weight of Oyl of Vitriol, in case we in a Glass Retort plac'd in Sand drew off the Saline *Menstruum* from the Metalline Liquor, till there remain'd a dry *Calx* at the bottome, though this Precipitate were a Snow White Body, yet upon pouring on it a large quantity of fair Water, we did almost in a moment perceive it to pass from a Milky Colour to one of the loveliest Light Yellows that ever we had beheld. Nor is the Turbith Mineral, that Chymists extol for its power to Salivate, and for other vertues, of a Colour much inferiour to this, though it be often made with a differing proportion of the Ingredients, / a more troublesome way. For *Beguinus*, who calls it *Mercurius præcipitatus optimus*,<sup>†@</sup> takes to one part of Quick-Silver, but two of Liquor, and

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that is Rectifi'd Oyl of Sulphur, which is (in *England* at least) far more scarce and dear than Oyl of Vitriol; he also requires a previous Digestion, two or three Cohobations, and frequent Ablutions with hot Distill'd Water, with other prescriptions, which though they may conduce to the Goodness of the Medicine, which is that he aims at, are troublesome, and, our Tryals have inform'd you unnecessary to the *obtaining the Lemmon Colour* which he regards not. But though we have very rarely seen either in Painters shops, or elsewhere a finer Yellow than that which we have divers times this way produc'd (which is the more considerable, because durable and pleasant Yellows are very hard to be met with, as may appear by the great use which Painters are for its Colours sake fain to make of that pernicious and heavy Mineral, Orpiment) yet I fear our Yellow is too costly, to be like to be imploy'd by Painters, unless about Choice pieces of Work, nor do I know how well it will agree with every Pigment, especially, with Oyl'd Colours. And whether this / Experiment, though it have seem'd somewhat strange to most we have shown it to, be really of another Nature than those wherein Saline Liquors are imploy'd, may, as we formerly also hinted, be so plausibly doubted, that whether the Water pour'd on the *Calx*, do barely by imbibing some of its Saline parts alter its Colour by altering its Texture, or whether by dissolving the Concoagulated Salts, it does become a Saline *Menstruum*, and, as such, work upon the Mercury, I freely leave to you (*Pyrophilus*) to consider. And that I may give you some Assistance in your Enquiry, I will not only tell you, that I have several times with fair Water wash'd from this *Calx*, good store of strongly tasted Corpuscles, which by the abstraction of the *Menstruum*, I could reduce into Salt; but I will also subjoyn an Experiment, which I devis'd, to shew among other things, how much a real and permanent Colour may be as it were drawn forth by a Liquor that has neither Colour, nor so much as Saline or other Active parts, provided it can but bring the parts of the Body it imbibes to convene into clusters dispos'd after the manner requisite to the exhibiting of the emergent Colour. The Experiment was this. /

### ***Experiment XLIII.***

We took good common Vitriol, and having beaten it to Powder, and put it into a Crucible, we kept it melted in a gentle heat, till by the Evaporation of some parts, and the shuffling of the rest, it had quite lost its former Colour, what remain'd we took out, and found it to be a friable *Calx*, of a dirty Gray. On this we pour'd fair Water, which it did not Colour Green or Blew, but only seem'd to make a muddy mixture with it, then stopping the Vial wherein the Ingredients were put, we let it stand in a quiet place for some dayes, and after many hours the water having dissolv'd a good part of the

imperfectly calcin'd Body, the Vitriolate Coruscles swimming to and fro in the Liquor, had time by their opportune Occursions to constitute many little Masses of Vitriol, which gave the water they impregnated a fair Vitriolate

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Colour; and this Liquor being pour'd off, the remaining dirty Powder did in process of time communicate the like Colour, but not so deep, to a second parcel of cleer Water that we pour'd on it. But this Experiment *Pyrophilus* is, (to give you that hint by the way) of too Luciferous a Nature to be fit to be / fully prosecuted, now that I am in haste, and willing to dispatch what remains. And we have already said of it, as much as is requisite to our present purpose.

#### *Experiment XLIV.*

It may (*Pyrophilus*) somewhat contribute towards the shewing how much some Colours depend upon the less or greater mixture, and (as it were,) Contemperation of the Light with shades, to observe, how that sometimes the number of Particles, of the same Colour, receiv'd into the Pores of a Liquor, or swimming up and down in it, do seem much to vary the Colour of it. I could here present you with particular instances to show, how in many (if not most) consistent Bodyes, if the Colour be not a Light one, as White, Yellow, or the like, the closeness of parts in the Pigments makes it look Blackish, though when it is display'd and laid on thinly, it will perhaps appear to be either Blew, or Green, or Red. But the Colours of consistent Pigments, not being those which the Preamble of this Experiment has lead you to expect Examples in, I shall take the instances I am now to give you, rather from Liquors than Dry Bodyes. If then you put a little fair Water into a / cleer and slender Vial, (or rather into one of those pipes of Glass, which we shall by and by mention;) and let fall into it a few drops of a strong Decoction or Infusion of *Cochineel*, or (for want of that) of *Brazil*; you may see the tinted drops descend like little Clouds into the Liquor; through which, if, by shaking the Vial, you diffuse them, they will turn the water either of a Pinck Colour, or like that which is wont to be made by the washing of raw flesh in fair Water; by dropping a little more of the Decoction, you may heighten the Colour into a fine Red, almost like that which ennobles Rubies; by continuing the affusion, you may bring the Liquor to a kind of a Crimson, and afterwards to a Dark and Opacous Redness, somewhat like that of Clotted Blood. And in the passage of the Liquor from one of these Colours to the other, you may observe, if you consider it attentively, divers other less noted Colours belonging to Red, to which it is not easie to give Names; especially considering how much the proportion of the Decoction to the fair Water, and the strength of that Decoction, together with that of the trajected Light and other Circumstances, may vary the Phœnomena of this Experiment. For the convenienter making whereof, we use / instead of a Vial, any slender Pipe of Glass of about a foot or more in length, and about the thickness of a mans little finger; For, if leaving one end of this Pipe open, you Seal up the other Hermetically, (or at least stop it exquisitely with a Cork well fitted to it, and over-laid with hard Sealing Wax melted, and rubb'd upon it;) you shall have a Glass, wherein may be observ'd the Variations of the Colours of Liquors much better than in large Vials, and wherein Experiments of this

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Nature may be well made with very small quantities of Liquor. And if you please, you may in this Pipe produce variety of Colours in the various parts of the Liquor, and keep them swimming upon one another unmix'd for a good while. And some have marveil'd to see, what variety of Colours we have sometimes (but I confess rather by chance than skill) produc'd in those Glasses, by the bare infusion of Brazil, variously diluted with fair Water, and alter'd by the Infusion of several Chymical Spirits and other Saline Liquors devoid themselves of Colour, and when the whole Liquor is reduc'd to an Uniform degree of Colour, I have taken pleasure to make that very Liquor seem to be of Colours gradually differing, by filling with it Glasses of a Conical figure, (whether the Glass have / its basis in the ordinary position, or turn'd upwards.) And yet you need not Glasses of an

extraordinary shape to see an instance of what the vari'd mixture of Light and Shadow can do in the diversifying of the Colour. For if you take but a large round Vial, with a somewhat long and slender Neck, and filling it with our Red Infusion of Brazil, hold it against the Light, you will discern a notable Disparity betwixt the Colour of that part of the Liquor which is in the Body of the Vial, and that which is more pervious to the Light in the Neck. Nay, I remember, that I once had a Glass and a Blew Liquor (consisting chiefly (or only, if my memory deceive me not,) of a certain Solution of Verdigrease) so fitted for my purpose, that though in other Glasses the Experiment would not succeed, yet when that particular Glass was fill'd with that Solution, in the Body of the Vial it appear'd of a Lovely Blew, and in the neck, (where the Light did more dilute the Colour,) of a manifest Green; and though I suspected there might be some latent Yellowness in the substance of the neck of the Glass, which might with the Blew compose that Green, yet was I not satisfi'd my self with my Conjecture, but the thing seem'd odd to me, as well as to divers curious / persons to whom it was shown. And I lately had a Broad piece of Glass, which being look'd on against the Light seem'd cleer enough, and held from the Light appear'd very lightly discolour'd, and yet it was a piece knock'd off from a great lump of Glass, to which if we rejoyn'd it, where it had been broken off, the whole Mass was as green as Grass. And I have several times us'd Bottles and stopples that were both made (as those, I had them from assur'd me) of the very same Metall, and yet whilst the bottle appear'd but inclining towards a Green, the Stopple (by reason of its great thickness) was of so deep a Colour that you would hardly believe they could possibly be made of the same materials. But to satisfie some Ingenious Men, on another occasion, I provided my self of a flat Glass (which I yet have by me,) with which if I look against the Light with the Broad side obverted to the eye, it appears like a good ordinary window Glass; but if I turn the Edge of it to my Eye, and place my Eye in a convenient posture in reference to the Light, it may contend for deepness of Colour with an Emerald. And this Greenness puts me in mind of a certain thickish, but not consistent Pigment I have sometimes made, and can show you when you please, / which being dropp'd on a piece of White Paper appears, where any quantity of it is fallen, of a

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somewhat Crimson Colour, but being with ones finger spread thinly on the Paper does presently exhibit a fair Green, which seems to proceed only from its disclosing its Colour upon the Extenuation of its Depth into Superficies, if the change be not somewhat help'd by the Colours degenerating upon one or other of the Accounts formerly mention'd. Let me add, that having made divers Tryals with that Blew substance, which in Painters Shops is call'd *Litmase*, we have sometimes taken Pleasure to observe, that being dissolv'd in a due proportion of fair Water, the Solution either oppos'd to the Light, or dropp'd upon White paper, did appear of a deep Colour betwixt Crimson and Purple; and yet that being spread very thin on the Paper and suffer'd to dry on there, the Paper was wont to appear Stain'd of a Fine Blew. And to satisfie my selfe, that the diversity came not from the Paper, which one might suspect capable of inbibing the Liquor, and altering the Colour, I made the Tryal upon a flat piece of purely White Glass'd Earth, (which I sometimes make use of about Experiments of Colours) with an Event not unlike the former. /

And now I speak of *Litmass*, I will add; that having this very day taken a piece of it, that I had kept by me these several years; to make Tryals about Colours, and having let fall a few drops of the strong Infusion of it in fair water, into a fine Crystal Glass, shap'd like an inverted Cone, and almost full of fair Water, I had now (as formerly) the pleasure to see, and to show other, how these few tinted drops variously dispersing themselves through the Limpid Water, exhibited divers Colours, or varieties of Purple and Crimson. And when the Corpuscles of the Pigment seem'd to have equally diffus'd themselves through the whole Liquor, I then by putting two or three drops of Spirit of Salt, first made an odd change in the Colour of the Liquor, as well as a visible commotion among its small parts, and in a short time chang'd it wholly into a very Glorious Yellow, like that of a Topaz. After which if I let fall a few drops of the strong and heavy Solution of Pot-ashes, whose weight would quickly carry it to the sharp bottome of the Glass; there would soon appear four very pleasant and distinct Colours; Namely, a Bright, but Dilute Colour at the picked bottome of the



Glass; a Purple, a little higher; a deep and glorious Crimson, (which Crimson / seem'd to terminate the operation of the Salt upward) in the confines betwixt the Purple and the Yellow; and an Excellent Yellow, the same that before enobled the whole Liquor, reaching from thence to the top of the Glass. And if I pleas'd to pour very gently a little Spirit of Sal Armoniack, upon the upper part of this Yellow, there would also be a Purple or a Crimson, or both, generated there, so that the unalter'd part of the Yellow Liquor appear'd intercepted betwixt the two Neighbouring Colours.

My scope in this 3d. Experiment (*Pyrophilus*) is manifold, as first to invite you to be wary in judging of the Colour of Liquors in such Glasses as are therein recommended to you, and consequently as much, if not more, when you imploy other Glasses. Secondly, That you may not think it strange, that I often content my self to rub upon a piece of White paper, the Juice of Bodies I would examine, since not onely I could not easily procure a sufficient Quantity of the juices in divers of them; but in several Cases the Tryals of the

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quantities of such Juices in Glasses would make us more lyable to mistakes, than the way that in those cases I have made use of. Thirdly, I hope you will by these and divers other / particulars deliver'd in this Treatise, be easily induc'd to think that I may have set down many Phœnomena very faithfully, and just as they appear'd to me, and yet by reason of some unheeded circumstance in the conditions of the matter, and in the degree of Light, or the manner of trying the Experiment, you may find some things to vary from the Relations I make of them. Lastly, I design'd to give you an opportunity to free your self from the amazement which possesses most Men, at the Tricks of those Mountebancks that are commonly call'd Water-drinkers. For though not only the vulgar, but ev'n many persons that are far above that Rank, have so much admir'd to see, a man after having drunk a great deal of fair water, to spurt it out again in the form of Claret Wine, Sack, and Milk, that they have suspected the intervening of Magick, or some forbidden means to effect what they conceived above the power of Art; yet having once by chance had occasion to oblige a Wanderer that made profession of that and other Jugling Tricks, I was easily confirm'd by his Ingenious confession to me, That this so much Admir'd Art, indeed consisted rather in a few Tricks, than in any great Skill, in altering the Nature and Colours of things. And I am easy / to be perswaded, that there may be a great deal of Truth in a little Pamphlet Printed divers years ago in English,<sup>†\*</sup> wherein the Author undertakes to discover, and that (if I mistake not) by the confession of some of the Complices themselves, That a famous Water-drinker then much Admir'd in *England*, perform'd his pretended Transmutations of Liquors by the help of two or three inconsiderable preparations and mixtures of not unobvious Liquors, and chiefly of an Infusion of Brazil variously diluted and made Pale or Yellowish, (and otherwise alter'd) with Vinegar, the rest of their work being perform'd by the shape of the Glasses, by Craft and Legerdemane. And for my part, that which I marvel at in this business, is, the Drinkers being able to take down so much Water, and spout it out with that violence; though Custome and a Vomit seasonably taken before hand, may in some of them much facilitate the work. But as for the changes made in the Liquors, they were but few and slight in comparison of those, that the being conversant in Chymical Experiments, and dextrous in applying them to the Transmuting of Colours, may easily enough enable a man to make, as ev'n what has been newly deliver'd in this, and the foregoing Experiment; especially if we add / to it the things contained in the XX, the XXXIX and the XL. Experiments, may perhaps have already perswaded You.

### *Experiment XLV.*

You may I presume (*Pyrophilus*) have taken notice, that in this whole Treatise, I purposely decline (as far as I well can) the mentioning of Elaborate Chymical Experiments, for fear of frightening you by their tediousness and difficulty;

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but yet in confirmation of what I have been newly telling you about the possibility of Varying the Colours of Liquors, better than the Water-drinkers are wont to do, I shall add, that *Helmont* used to make a preparation of Steel, which a very Ingenious Chymist, his Sons Friend, whom you know,<sup>†\*</sup> sometimes employs for a succedaneum to the Spaw-waters, by Diluting this *Essentia Martis Liquida* (as he calls it) with a due proportion of Water. Now that for which I mention to you this preparation, (which as he communicated to me, I know he will not refuse to *Pyrophilus*) is this, that though the Liquor (as I can shew you when you please) be almost of the Colour of a German (not an Oriental) Amethyst, and consequently remote enough from Green, / yet a very few drops being let fall into a Large proportion of good Rhenish, or (in want of that) White Wine (which yet do's not quite so well) immediately turn'd the Liquor into a lovely Green, as I have not without delight shown several curious Persons. By which *Phænomenon* you may learn, among other things, how requisite it is in Experiments about the changes of Colours heedfully to mind the Circumstances of them; for Water will not, as I have purposely try'd, concurr to the production of any such Green, nor did it give that Colour to moderate Spirit of Wine, wherein I purposely dissolv'd it, and Wine it self is a Liquor that few would suspect of being able to work suddenly any such change in a Metalline preparation of this Nature; and to satisfy my self that this new Colour proceeds rather from the peculiar Texture of the Wine, than from any greater Acidity, that Rhenish or White-wine (for that may not absurdly be suspected) has in comparison of Water; I purposely sharpen'd the Solution of this Essence in fair Water, with a good quantity of Spirit of Salt, notwithstanding which, the mixture acquir'd no Greenness. And to vary the Experiment a little, I try'd, that if into a Glass of Rhenish Wine made Green by this Essence, / I dropp'd an Alcalizate Solution, or Urinous Spirit, the Wine would presently grow Turbid, and of an odd Dirty Colour: But if instead of dissolving the Essence in Wine, I dissolv'd it in fair Water sharpen'd perhaps with a little Spirit of Salt, then either the Urinous Spirit of Sal Armoniack, or the Solution of the fix'd Salt of Pot-ashes would immediately turn it of a Yellowish Colour, the fix'd or Urinous Salt Precipitating the Vitriolate substance contain'd in the Essence. But here I must not forget to take notice of a circumstance that deserves to be compar'd with some part of the foregoing Experiment, for whereas our Essence imparts a Greenness to Wine, but not to Water, the Industrious *Olaus Wormius* in his late *Musæum*<sup>†@</sup> tells us of a rare kind of Turn-Sole which he calls *Bezetta Rubra* given him by an Apothecary that knew not how it was made, whose lovely Redness would be easily communicated to Water, if it

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were immers'd in it; but scarce to Wine, and not at all to Spirit of Wine, in which last circumstance it agrees with what I lately told you of our Essence, notwithstanding their disagreement in other particulars./

### *Experiment XLVI.*

We have often taken notice, as of a remarkable thing, that Metalls as they appear to the Eye, before they come to be farther alter'd by other Bodyes, do exhibit Colours very different from those which the Fire and the *Menstruum*, either apart, or both together, do produce in them; especially considering that these Metalline Bodyes are after all these disguises reducible not only to their former Metalline Consistence and other more radical properties, but to their Colour too, as if Nature had given divers Metalls to each of them a double Colour, an *External*, and an *Internal*; But though upon a more attentive Consideration of this difference of Colours, it seem'd probable to me, that divers (for I say not all) of those Colours which we have just now call'd *Internal*, are rather produc'd by the Coalition of Metalline Particles with those of the Salts, or other Bodyes employ'd to work on them, than by the bare alteration of the parts of the Metalls themselves: and though therefore we may call the obvious Colours, Natural or Common, & the others Adventitious, yet because such changes of Colours, form whatsoever cause they be resolv'd to / proceed may be properly enough taken in to illustrate our present Subject, we shall not scruple to take notice of some of them, especially because there are among them such as are produc'd without the interven-vention of

Saline *Menstruums*. Of the Adventitious Colours of Metalline Bodies the Chief sorts seem to be these three. The first, such Colours as are produc'd without other Additaments by the Action of the fire upon Metalls. The next such as emerge from the Coalition of Metalline Particles with those of some *Menstruum* imploy'd to Corrode a Metall or Precipitate it; And the last, The Colours afforded by Metalline Bodyes either Colliquated with, or otherwise Penetrating into, other Bodies, especially fusible ones. But these (*Pyrophilus*,) are only as I told you, the *Chief* sorts of the adventitious Colours of Metalls, for there may others belong to them, of which I shall hereafter have occasion to take notice of some, and of which also there possibly may be others that I never took notice of.

And to begin with the first sort of Colours, 'tis well enough known to Chymists, that Tin being Calcin'd by fire alone is wont to afford a White *Calx*, and Lead Calcin'd by fire alone affords that most Common Red-Powder we call *Minium*: Copper also / Calcin'd *per se*, by a long or violent fire, is wont to yield (as far as I have had occasion to take notice of it) a very Dark or Blackish Powder; That Iron likewise may by the Action of Reverberated flames be turn'd into a Colour almost like that of Saffron, may be easily deduc'd from the Preparation of that Powder, which by reason of its Colour and of the Metall 'tis made of is by Chymists call'd, *Crocus Martis per se*. And that *Mercury* made by the stress of Fire, may be turn'd into a Red

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Powder, which Chymists call Precipitate *per se*, I elsewhere more particularly declare.

### ***Annotation I.***

It is not unworthy the Admonishing you, (*Pyrophilus*,) and it agrees very well with our Conjectures about the dependence of the change of a Body's Colour upon that of its Texture, that the same Metall may by the successive operation of the fire receive divers Adventitious Colours, as is evident in Lead, which before it come to so deep a Colour as that of *Minium*, may pass through divers others. /

### ***Annotation II.***

Not only the *Calces*, but the Glasses of Metalls, Vitrify'd *per se*, may be of Colours differing from the Natural or Obvious Colour of the Metall; as I have observ'd in the Glass of Lead, made by long exposing Crude Lead to a violent fire, and what I have observ'd about the Glass or Slagg of Copper, (of which I can show you some of an odd kind of Texture,) may be elsewhere more conveniently related. I have likewise seen a piece of very Dark Glass, which an Ingenious Artificer that show'd it me profess'd himself to have made of Silver alone by an extreme *Violence* (which seems to be no more than is needfull) of the fire.

### ***Annotation III.***

Minerals also by the Action of the Fire may be brought to afford Colours very differing from their own, as I not long since noted to you about the variously Colour'd Flowers of Antimony, to which we may add the Whitish Grey-Colour of its *Calx*, and the Yellow or Reddish Colour of the Glass, where into that *Calx* may be flux'd.

And I remember, that I elsewhere told / you, that Vitriol Calcin'd with a very gentle heat, and afterwards with higher and higher degrees of it, may be made to pass through several Colours before it descends to a Dark Purplish Colour, whereto a strong fire is wont at length to reduce it. But to insist on the Colours produc'd by the Operation of fire upon several Minerals would take up farr more time than I have now to spare.

The Adventitious Colours produc'd upon Metalls, or rather with them, by Saline Liquors, are many of them so well known to Chymists, that I would not here mention them, but that besides a not unneeded Testimony, I can add something of my own, to what I shall repeat about them, and divers Experiments which are familiar to Chymists, are as yet unknown to the greatest part of Ingenious Men.

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That Gold dissolv'd in *Aqua Regia* ennobles the *Menstruum* with its own Colour, is a thing that you cannot (*Pyrophilus*,) but have often seen. The Solutions of Mercury in *Aqua-fortis* are not generally taken notice of, to give any notable Tincture to the *Menstruum*; but sometimes when the / Liquor first falls upon the Quick Silver, I have observ'd a very remarkable, though not durable, Greenness, or Blewness to be produc'd, which is a *Phænomenon* not unfit for you to consider, though I have not now the leisure to discourse upon it. Tin Corroded by *Aqua-fortis* till the *Menstruum* will work no farther on it, becomes exceeding White, but as we elsewhere note, does very easily of it self acquire the consistence, not of a Metalline *Calx*, but of a Coagulated matter, which we have observ'd with pleasure to look so like, either to curdled Milk, or curdled Whites of Eggs, that a person unacquainted with such Solutions may easily be mistaken in it. But when I purposely prepar'd a *Menstruum* that would dissolve it as *Aqua-fortis* dissolves Silver, and not barely Corrode it, and quickly let it fall again, I remember not that I took notice of any particular Colour in the Solution, as if the more Whitish Metalls did not much Tinge their *Menstruums*, though the conspicuously Colour'd Metalls as Gold, and Copper, do. For Lead dissolv'd in Spirit of Vinegar or *Aqua-fortis* gives a Solution cleer enough, and if the *Menstruum* be abstracted appears either Diaphanous or White. Of the Colour of Iron we have elsewhere said something: And 'tis worth / noting, that, though if that Metall be dissolv'd in oyl of Vitriol diluted with water, it affords a Salt or Magistery so like in colour, as well as some other Qualities, to other green Vitriol, that Chymists do not improperly call it *Vitriolum Martis*; yet I have purposely try'd, that, by changing the *Menstruum*, and pouring upon the filings of Steel, instead of oyl of Vitriol, *Aqua Fortis*, (whereof as I remember, I us'd 4 parts to one of the Metall) I obtain'd not a Green, but a Saffron Colour Solution; or rather a thick Liquor of a deep but yellowish Red. Common Silver, such as is to be met with in Coines, being dissolv'd in *Aqua fortis*, yields a Solution tinted like that of Copper, which is not to be wondred at, because in the coining of Silver, they are wont (as we elsewhere particularly inform you) to give it an Allay of Copper, and that which is sold in shops for refined silver, is not (so far as we have tryed) so perfectly free from that ignobler Metall, but that a Solution of It in *Aqua fortis*, will give a Venereal Tincture to the *Menstruum*. But we could not observe upon the solution of some Silver, which was perfectly refin'd, (such as some that we have, from which 8 or 10 times its weight of Lead has been blown off) that the *Menstruum* / though held against the Light in a Crystal Vial did manifestly disclose any Tincture, only it seem'd sometimes not to be quite destitute of a little, but very faint Blewishness.

But here I must take notice, that of all the Metalls, there is not any which doth so easily and constantly disclose its unobvious colour as Copper doth. For not only in acid *Menstruums* as *Aqua Fortis* and Spirit of Vinegar, it gives a Blewish green solution, but if it be almost any way corroded, it *appears of one of those* two colours, as may be observ'd in Verdigreese made several wayes, in that odd preparation of *Venus*, which we elsewhere teach you to

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make with Sublimate, and in the common Vitriols of *Venus* deliver'd by Chymists; and so constant is the disposition of Copper, notwithstanding the disguise Artists put upon it, to disclose the colour we have been mentioning, that we have by forcing it up with *Sal Armoniack* obtain'd a Sublimate of a Blewish Colour. Nay a famous Spagyrist affirms, that the very Mercury of it is green, but till he



teach us an intelligible way of making such a Mercury, we must content our selves to inform you, that we have had a Cupreous Body, that was Præcipitated out of a distill'd Liquor, that seem'd to be / the Sulphur of *Venus*, and seemed even when flaming, of a Greenish Colour.<sup>†\*</sup> And indeed Copper is a Metall so easily wrought upon by Liquors of several kinds, that I should tell you, I know not any Mineral, that will concurr to the production of such a variety of Colours as Copper dissolv'd in several *Menstruums*, as Spirit of Vinegar, *Aqua fortis*, *Aqua Regis*, Spirit of Nitre, or Urine, of Soot, Oyls of several kinds, and I know not how many other Liquors, if the variety of somewhat differing colours (that Copper will be made to assume, as it is wrought upon by several Liquors) were not comprehended within the Limits of Greenish Blew, or Blewish Green.

And yet I must advertise you (*Pyrophilus*) that being desirous to try if I could not make with crude Copper a Green Solution without the Blewishness that is wont to accompany its Vulgar Solutions, I bethought my self of using two *Menstruums*, which I had not known imploy'd to work on this Metall, and which I had certain Reasons to make Tryal of, as I successfully did. The one of these Liquors (if I much misremember not) was Spirit of Sugar distill'd in a Retort, which must be warily done, (if you will avoid breaking your / glasses) and the other, Oyl or Spirit of Turpentine, which affords a fine Green Solution that is useful to me on several occasions. And yet to shew that the adventitious colour may result, as well from the true and permanent Copper it self, as the Salts wherewith 'tis corroded, I shall add, that if you take a piece of good *Dantzick* Copperis, or any other Vitriol wherein *Venus* is prædominant, and having moistened it in your Mouth, or with fair water, rubb it upon a whetted knife, or any other bright piece of Steel or Iron, it will (as we have formerly told you) presently stain the Steel with a Reddish colour, like that of Copper, the reason of which, we must not now stay to inquire.

### *Annotation I.*

I presume you may have taken notice (*Pyrophilus*) that I have borrowed some of the Instances mention'd in this 47th Experiment, from the Laboratories of Chymists, and because in some (though very few) other passages of this Essay, I have likewise made use of Experiments mention'd also by some Spagyricall Writers, I think it not amiss to represent to you on this Occasion once for all, some things besides / those which I intimated in the præamble of this present Experiment; For besides, that 'tis very allowable for a Writer to repeat an Experiment which he invented not, in case he improve it; And besides that

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many Experiments familiar to Chymists are unknown to the generality of Learned Men, who either never read Chymical processes, or never understood their meaning, or never durst believe them; besides these things, I say, I shall represent, That, as to the few Experiments I have borrowed from the Chymists, if they be very Vulgar, 'twould perhaps be difficult to ascribe each of them its own Author, and 'tis more than the generality of Chymists themselves can do: and if they be not of very known and familiar practise among them, unless the Authors wherein I found them had given me cause to believe, themselves had try'd them, I know not why I might not set them down, as a part of the *Phænomena* of Colours which I present you; Many things unanimously enough deliver'd as matters of fact by (I know not how many Chymical Writers) being not to be rely'd on, upon the single Authority of such Authors: For Instance as some Spagyrist deliver (perhaps amongst several deceitful processes) that *Saccarum Saturni* / with Spirit of Turpentine will afford a Balsom, so *Beguinus* and many more tell us,<sup>†\*</sup> that the same Concrete (*Saccarum Saturni*) will yield an incomparably fragrant Spirit, and a pretty Quantity of two several Oyles, and yet since many have complain'd, as well as I have done, that they could find no such odoriferous, but rather an ill-sented Liquor, and scarce any oyl in their Distillation of that sweet Vitriol, a wary person would as little build any thing on what they say of the former Experiment, as upon what they averr of the later, and therefore I scrupled not to mention this Red Balsom of which I have not seen any, (but what I made)

among my other Experiments about redness.

### *Annotation II.*

We have sometimes had the Curiosity to try what Colours Minerals, as Tinglass, Antimony, Spelter, &c. would yield in several *Menstruums*, nor have we forbore to try the Colours of Stones, of which that famous one, (which *Helmont* calls *Paracelsus's Ludus*<sup>†\*</sup>) though it be digg'd out of the Earth and seem a true stone, has afforded in *Menstruums* capable to dissolve so solid a stone, sometimes a Yellowish, / sometimes a Red solution of both which I can show you.<sup>†\*</sup> But though I have from Minerals obtain'd with several *Menstruums* very differing Colours, and some such as perhaps you would be surpriz'd

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to see drawn from such Bodies: yet I must now pass by the particulars, being desirous to put an End to this Treatise, before I put an end to your Patience and my own.

### *Annotation III.*

And yet before I pass to the next Experiment, I must put you in mind, that the Colours of Metals may in many cases be further alter'd by employing, either præcipitating Salts, or other convenient Substances to act upon their Solutions. Of this you may remember, that I have given you several Instances already, to which may be added such as these, That if Quicksilver be dissolv'd in *Aqua fortis*, and Præcipitated out of the Solution, either with water impregnated with Sea salt, or with the spirit of that Concrete, it falls to the Bottom in the form of a white powder, whereas if it be Præcipitated with an Alcaly, it will afford a Yellowish or tawny powder, and if there be no Præcipitation made, and the *Menstruum* be drawn off with a convenient / fire, the corroded Mercury will remain in the Bottom, in the form of a substance that may be made to appear of differing Colours by differing degrees of Heat; As I remember that lately having purposely abstracted *Aqua fortis* from some Quicksilver that we had dissolv'd in it, so that there remain'd a white *Calx*, exposing that to several degrees of Fire, and afterwards to a naked one, we obtain'd some new Colours, and at length the greatest part of the *Calx* lying at the Bottome of the Vial, and being brought partly to a Deep Yellow, and partly to a Red Colour, the rest appear'd elevated to the upper part and neck of the Vial, some in the form of a Reddish, and some of an Ash-Colour Sublimate. But of the differing Colours which by differing wayes and working of Quick Silver with Fire, and Saline Bodies, may be produc'd in Precipitates, I may elsewhere have occasion to take further notice. I also told you not long since, that if you corrode Quick-silver with Oyl of Vitriol instead of *Aqua-fortis*, and abstract the *Menstruum*, there will remain a White *Calx* which by the Affusion of Fair Water presently turns into a Lemmon Colour. And ev'n the *Succedaneum* to a *Menstruum* may sometimes serve the turn to change the Colours of a Metal. The lovely / Red which Painters call Vermillion, is made of Mercury, which is of the Colour of Silver, and of Brimstone which is of

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Kin to that of Gold, Sublim'd up together in a certain proportion, as is vulgarly known to Spagyrist.

### *Experiments XLVIII.*

The third chief sort of the Adventitious Colours of Metals, is, that which is produc'd by associating them (especially when Calcin'd) with other fusible Bodies, and Principally Venice, and other fine Glass devoid of Colour.

I have formerly given you an Example, whereby it may appear, that a Metal may impart to Glass a Colour much differing from its own, when I told you, how with Silver, I had given Glass a lovely Golden Colour. And I shall now add, that I have Learn'd from one of the Chief Artificers that sells Painted Glass, that those of his Trade Colour it Yellow with a preparation of the *Calx* of Silver. Though having lately had occasion among other Tryals to mingle a few grains of Shell-silver (such as is imploy'd with the Pensil and Pen) with a convenient proportion of powder'd Crystal Glass, having kept them two or three / hours infusion, I was surpriz'd to find the Colliquated Mass to appear upon breaking the Crucible of a lovely Saphirine Blew, which made me suspect my Servant might have brought me a wrong Crucible, but he constantly affirm'd it to be the same wherein the Silver was put, and considerable Circumstances countenanc'd his Assertion, so that till I have opportunity to make farther Tryal, I cannot but suspect, either that Silver which is not (which is not very probable) brought to a perfect Fusion and Colliquation with Glass, may impart to it other Colours than when Neal'd upon it, or else (which is less unlikely) that though Silver Beaters usually chuse the finest Coyn they can get, as that which is most extensive under the Hammer, yet the Silver-leaves of which this Shel-silver was made, might retain so much Copper as to enable it to give the predominant Tincture to the Glass.

For, I must proceed to tell you (*Pyrophilus*) as another instance of the Adventitious Colours of Metals, that which is something strange, Namely, That though Copper Calcin'd *per se* affords but a Dark and basely Colour'd *Calx*, yet the Glass-men do with it, as themselves inform me, Tinge their Glass green. And I remember, that when once we took some crude Copper, / and by frequent Ignition quenching it in Water had reduc'd it to a Dark and Ill-colour'd Powder, and afterwards kept it in Fusion in about a 100. times its weight of fine Glass, we had, though not a Green, yet a Blew colour'd Mass, which would perhaps have been Green, if we had hit right upon the Proportion of the Materials, and the Degree of Fire, and the time wherein it ought to be kept in Fusion, so plentifully does that Metal abound in a Venereal Tincture, as Artists call it, and in so many wayes does it disclose that Richness. But though Copper do as we have said give somewhat near the like Colour to Glass, which it does to *Aqua-fortis*, yet it seems worth inquiry, whether those new Colours which Mineral Bodies disclose in melted Glass, proceed from the Coalition of the Corpuscles of the Mineral with the Particles of the Glass as such, or from the Action (excited or actuated by fire) of

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the Alcalizate Salt (which is a main Ingredient of Glass,) upon the Mineral Body, or from the concurrence of both these Causes, or else from any other. But to return to that which we were saying, we may observe that *Putty* made by calcining together a proportion of Tin and Lead, as it is it self a White *Calx*, so does it turn the *Pitta di Crystallo* (as the / Glassmen call the matter of the Purer sort of Glass, wherewith it is Colliquated into a White Mass, which if it be opacous enough is employ'd, as we elsewhere declare, for White Amel. But of the Colours which the other Metals may be made to produce in Colourless Glass, and other Vitrifiable Bodies, that have native Colours of their own, I must leave you to inform your self upon Tryal, or at least must forbear to do it till another time, considering how many Annotations are to follow, upon what has in this and the two former Experiments been said already.

### *Annotation I.*

When the Materials of Glass being melted with Calcin'd Tin, have compos'd a Mass Undiaphanous and White, this White Amel is as it were the Basis of all those fine Concretes that Goldsmiths and several Artificers imploy in the curious Art of Enamelling. For this White and Fusible substance will receive into it self, without spoyling them, the Colours of divers other Mineral substances, which like it will indure the fire. /

### *Annotation II.*

So that as by the present (XLVIII.) Experiment it appears, that divers Minerals will impart to fusible Masses, Colours differing from their own; so by the making and compounding of Amels, it may appear, that divers Bodies will both retain their Colour in the fire, and impart the *same* to some others wherewith they were vitrifi'd, and in such Tryals as that mention'd in the 17. Experiment, where I told you, that ev'n in Amels a Blew and Yellow will compound a Green. 'Tis pretty to behold, not only that some Colours are of so fix'd a Nature, as to be capable of mixture without receiving any detriment by the fire, that do's so easily destroy or spoyl those of other Bodies; but Mineral Pigments may be mingled by fire little less regularly and successfully, than in ordinary Dyeing Fatts, the vulgar Colours are wont to be mingled by the help of Water.

### *Annotation III.*

'Tis not only Metalline, but other Mineral Bodies, that may be imploy'd, to give Tinctures unto Glass (and 'tis worth noting / how small a quantity of some Mineral substances, will Tinge a Comparatively vast proportion of Glass, and we have sometimes attempted to Colour Glass, ev'n with Precious Stones, and had cause to think the Experiment not cast away. And 'tis known

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by them that have look'd into the Art of Glass, that the Artificers use to tinge their Glass Blew, with that Dark Mineral *Zaffora*, (some of my Tryals on which I elsewhere acquaint you)<sup>†\*</sup> which some would have to be a Mineral Earth, others a Stone, and others neither the one, nor the other, but which is confessedly of a Dark, but not a Blew Colour, though it be not agreed of what particular Colour it is. 'Tis likewise though a familiar yet a remarkable practise among those that Deal in the making of Glass, to imploy (as some of themselves have inform'd me) what they call Manganess, and some Authors call *Magnesia* (of which I make particular mention in another Treatise) to exhibit in Glass not only other Colours than its own, (which is so like in Darkness or Blackishness to the Load-stone, that 'tis given by Mineralists, for one of the Reasons of its Latine Name) but Colours differing from one another. For though they use it, (which is somewhat strange) to Clarifye their Glass, and free / it from that Blewish Greenish Colour, which else it would too often be subject to, yet they also imploy it in certain proportions, to tinge their Glass both with a Red colour, and with a Purplish or Murry, and putting in a greater Quantity, they also make with it that deep obscure Glass which is wont to pass for Black, which agrees very well with, and may serve to confirm what we noted near the beginning of the 44th Experiment, of the seeming Blackness of those Bodies that are overcharg'd with the Corpuscles of such Colours, as Red, or Blew, or Green, &c. And as by several Metals and other Minerals we can give various Colours to Glass, so on the other side, by the differing Colours that Mineral Oars, or other Mineral Powders being melted with Glass disclose in it, a good Conjecture may be oftentimes made of the Metall or known Mineral, that the Oar propos'd, either holds, or is most of kin to. And this easie way of examining Oars, may be in some cases of good use, and is not ill deliver'd by *Glauber*, to whom I shall at present refer you, for a more particular account of it:<sup>†\*</sup> unless your Curiosity command also what I have observ'd about these matters; only I must here advertise you, that great circumspection is / requisite to keep this way from proving fallacious, upon the account of the variations of Colour that may be produc'd by the differing proportions that may be us'd betwixt the Oar and the Glass, by the Richness or Poorness of the Oar it self, by the Degree of Fire, and (especially) by the Length of Time, during which the matter is kept in fusion; as you will easily gather from what you will quickly meet with in the following Annotation upon this present 48th Experiment.

### *Annotation IV.*

There is another way and differing enough from those already mention'd, by which Metalls may be brought to exhibit adventitious Colours: For by This, the Metall do's not so much impart a Colour to another Body, as receive a Colour from it, or rather both Bodies do by the new Texture resulting



their mition produce a new Colour. I will not insist to this purpose upon the Examples afforded us by yellow Orpiment, and common Sea Salt, from which, sublim'd together, Chymists unanimously affirm their White or Crystalline Arsenick to be made: But 'tis not unworthy our noting, That though Yellow / Orpiment be acknowledg'd to be the Copiousest by far of the two Ingredients of Arsenick, yet this last nam'd Body being duely added to the highest Colour'd Metall Copper, when 'tis in fusion, gives it a whiteness both within and without. Thus *Lapis Calaminaris* changes and improves the Colour of Copper by turning it into Brass. And I have sometimes by the help of Zinck duely mix'd after a certain manner, given Copper one of the Richest Golden Colours that ever I have seen the Best true Gold Ennobled with. But pray have a care that such Hints fall not into any hands that may mis-employ them.

### *Annotation V.*

Upon the Knowledge of the differing wayes of making Minerals and Metalls produce their adventitious Colours in Bodies capable of Vitrification, depends the pretty Art of making what Chymists by a Barbarous Word are pleas'd to call *Amanses*, that is counterfeit, or factitious Gemms, as Emeralds, Rubies, Saphires, Topazes, and the like. For in the making of these, though pure Sand or Calcin'd Crystal give the Body, yet 'tis for the most part some Metalline or Mineral *Calx*, mingled in a / smal proportion that gives the Colour. But though I have many years since taken delight, to divert my self with this pleasing Art, and have seen very pretty Productions of it, yet besides that I fear I have now forgot most of the little Skill I had in it, this is no place to entertain you with what would rather take up an intire Discourse, than be comprehended in an Annotation; wherefore the few things which I shall here take notice of to you, are only what belong to the present Argument, Namely,

First, That I have often observ'd that Calcin'd Lead Colliquated with fine White Sand or Crystal, reduc'd by ignitions and subsequent extinctions in Water to a subtile Powder, will of it self be brought by a due Decoction to give a cleer Mass Colour'd like a *German* Amethyst. For though this glass of Lead, is look'd upon by them that know no better way of making *Amanses*, as the grand Work of them all, yet which is an inconvenience that much blemishes this way, the Calcin'd Lead it self does not only afford matter to the *Amanses*, but has also as well as other Metals a Colour of its own, which as I was saying, I have often found to be like that of *German* (as many call them) not Eastern Amethysts.

Secondly, that nevertheless this Colour / may be easily over-powr'd by those of divers other Mineral Pigments (if I may so call them) so that with a glass of Lead, you may Emulate (for Instance) the fresh and lovely Greenness of an Emerald, though in divers cases the Colour which the Lead it self upon Vitrification tends to, may vitiate that of the Pigment, which you would introduce into the Mass.

Thirdly, That so much ev'n these Colours depend upon Texture, that in the Glass of Lead it self made of about three parts of *Lytharge* or *Minium* Colliquated with one of very finely Powder'd Crystal or Sand, we have taken pleasure to make the mixture pass through differing Colours, as we kept it more or less in the Fusion. For it was not usually till after a pretty long Decoction that the Mass attain'd to the Amethystin Colour.

Fourthly and lastly, That the degrees of Coction and other Circumstances may so vary the Colour produc'd in the same mass, that in a Crucible that was not great I have had fragments of the same

## Annotation VI.

You may remember (*Pyrophilus*) that when I mention'd the three sorts of adventitious Colours of Metals, I mention'd them but as the chief, not the only. For these may be other wayes, which though they do not in so strict a sense belong to the adventitious Colours of Metals, may not inconveniently be reduc'd to them. And of these I shall name now a couple, without denying that there may be more.

The first may be drawn from the practise of those that Dye Scarlet. For the famousest Master in that Art, either in *England* or *Holland*, has confess'd to me, that neither others, nor he can strike that lovely Colour which is now wont to be call'd the *Bow-Dye*, without their Materials by Boyl'd in Vessels, either made of, or lin'd with a particular Metall. But of what I have known attempted in this kind, I must not as yet for fear of prejudicing or displeasing others give you any particular Account.<sup>†@</sup>

The other way (*Pyrophilus*) of making Metals afford unobvious Colours, is by imbuing divers Bodies with Solutions of them made in their proper *Menstruum's*, As (for / Instance) though Copper plentifully dissolv'd in *Aqua fortis*, will imbue several Bodies with the Colour of the Solution; Yet some other Metalls will not (as I elsewhere tell you) and have often try'd. Gold dissolv'd in *Aqua Regia*, will, (which is not commonly known) Dye the Nails and Skin, and Hafts of Knives, and other things made of Ivory, not with a Golden, but a Purple Colour, which though it manifest it self but slowly, is very durable, and scarce ever to be wash'd out. And if I misremember not, I have already told you in this Treatise, that the purer Crystals of fine Silver made with *Aqua fortis*, though they appear White, will presently Dye the Skin and Nails, with a Black, or at least a very Dark Colour, which Water will not wash off, as it will ordinary Ink from the same parts. And divers other Bodies may the same way be Dy'd, some of a Black, and others of a Blackish Colour.

And as Metalline, so likewise Mineral Solutions may produce Colours differing enough from those of the Liquors themselves. I shall not fetch an

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Example of this, from what we daily see happen in the powdring of Beef, which by the Brine imploy'd about it (especially if the flesh be / over salted) do's oftentimes appear at our Tables of a Green, and sometimes of a Reddish Colour, (deep enough) nor shall I insist on the practise of some that deal in Salt Petre, who, (as I suspected, and as themselves acknowledg'd to me) do, with the mixture of a certain proportion of that, and common Salt, give a fine Redness, not only to Neats Tongues, but which is more pretty as well as difficult, to such flesh, as would otherwise be purely White; These Examples, I say, I shall decline insisting on, as chusing rather to tell you, that I have several times try'd, that a Solution of the Sulphur of Vitriol, or ev'n of common Sulphur, though the Liquor appear'd clear enough, would immediately tinge a piece of new Coin, or other clean Silver, sometimes with a Golden, sometimes with a deeper, and more Reddish colour, according to the strength of the Solution, and the quantity of it, that chanc'd to adhere to the Metall, which may take off your wonder that the water of the hot Spring at *Bath*, abounding with dissolv'd Substances of a very Sulphureous Nature, should for a while, as it were gild, the new or clean pieces of Silver coyn, that are for a due time immers'd in it. And to these may be added those formerly mention'd Examples / of the adventitious Colours of Mineral Bodies; which brings into my mind, that, ev'n Vegetable Liquors, whether by degeneration, or by altering the Texture of the Body that imbibes them, may stain other Bodies with Colours differing enough from their own, of which very good Herbarists have afforded us a notable Example, by affirming that the Juice of *Alcanna* being green (in which state I could never here procure it) do's yet Dye the Skin and Nails of a Lasting Red. But I see this Treatise is like to prove too bulky without the addition of further Instances of this Nature.

Meeting the other day, *Pyrophilus*, in an *Italian* book, that treats of other matters,<sup>†\*</sup> with a way of preparing what the Author calls a *Lacca* of Vegetables, by which the *Italians* mean a kind of Extract fit for Painting, like that rich *Lacca* in English commonly call'd *Lake*, which is imploy'd by Painters as a glorious Red. And finding the Experiment not to be inconsiderable, and very defectively set down, it will not be amiss to acquaint you with what some Tryals have inform'd us, in reference to this / Experiment, which both by our Italian Author, and by divers of his Countrymen, is look'd upon as no trifling Secret.

Take then the root call'd in Latin *Curcuma*, and in English Turmerick, (which I made use of, because it was then at hand, and is among Vegetables fit for that purpose one of the most easiest to be had) and when it is beaten, put what Quantity of it you please into fair Water, adding to every pound of Water about a spoonfull or better of as strong a *Lixivium* or Solution of

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Pot-ashes as you can well make, clarifying it by Filtration before you put it to the Decocting water. Let these things boyl, or rather simmer over a soft Fire in a clean glaz'd Earthen Vessel, till you find by the Immersion of a sheet of White Paper (or by some other way of Tryal) that the Liquor is sufficiently impregnated with the Golden Tincture of the Turmerick, then take the Decoction off the Fire, and Filter or Strain it that it may be clean, and leisurely dropping into it a strong Solution of Roch Allum, you shall find the Decoction as it were curdl'd, and the tinted part of it either to emerge, to subside, or to swim up and down, like little Yellow flakes; and if you pour this mixture into a Tunnel lin'd with Cap Paper, the Liquor that Filtred formerly / so Yellow, will now pass clean thorow the Filtre, leaving its tinted, and as it were curdled parts in the Filtre, upon which fair Water must be so often pour'd, till you have Dulcifi'd the matter therein contain'd, the sign of which Dulcification is (you know) when the Water that has pass'd through it, comes from it as tasteless as it was pour'd on it. And if without Filtration you would gather together the flakes of this Vegetable Lake, you must pour a great Quantity of fair Water upon the Decoction after the affusion of the Alluminous Solution, and you shall find the Liquor to grow clearer, and the Lake to settle together at the bottom, or emerge to the top of the Water, though sometimes having not pour'd out a sufficient Quantity of fair Water, we have observ'd the Lake partly to subside, and partly to emerge, leaving all the middle of the Liquor clear. But to make this Lake fit for use, it must by repeated affusions of fresh Water, be Dulcifi'd from the adhering Salts, as well as that separated by Filtration, and be spread and suffer'd to dry leisurely upon pieces of Cloth, with Brown Paper, or Chalk, or Bricks under / them to imbibe the Moisture.<sup>†@</sup>

### *Annotation I.*

Whereas it is presum'd that the Magistery of Vegetables obtain'd this way consists but of the more Soluble and Colour'd parts of the Plants that afford it, I must take the liberty to Question the Supposition. And for my so doing, I shall give you this account.

According to the Notions (such as they were) that I had concerning Salts; Allom, though to sense a Homogeneous Body, ought not to be reckon'd among true Salts, but to be it self look'd upon as a kind of Magistery, in

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regard that as Native Vitriol (for such I have had) contains both a Saline substance and a Metall, whether Copper, or Iron, corroded by it, and associated with it; so Allom which may be of so near a kin to Vitriol, that in some places of *England* (as we are assur'd by good Authority the same stone

will / sometimes afford both) seems manifestly to contain a peculiar kind of Acid Spirit, generated in the Bowels of the Earth, and some kind of stony matter dissolv'd by it. And though in making our ordinary Allom, the Workmen use the Ashes of a Sea Weed (vulgarly call'd Kelp) and Urine: yet those that should know, inform us, that, here in *England*, there is besides the factitious Allom, Allom made by Nature without the help of those Additaments. Now (*Pyrophilus*) when I consider'd this composition of Allom, and that Alcalizate Salts are wont to Præcipitate what acid Salts have dissolv'd, I could not but be prone to suspect that the Curdled Matter, which is call'd the Magistery of Vegetables, may have in it no inconsiderable proportion of a stony substance Præcipitated out of the Allom by the *Lixivium*, wherein the Vegetable had been decocted, and to shew you, that there is no necessity, that all the curdl'd substance must belong to the Vegetable, I shall add, that I took a strong Solution of Allom, and having Filtred it, by pouring in a convenient Quantity of a strong Solution of Pot-ashes, I presently, as I expected, turn'd the mixture into a kind of white Curds, which being put to Filtre, the Paper retain'd a stony / *Calx*, copious enough, very White, and which seem'd to be of a Mineral Nature, both by some other signes, and this, that little Bits of it being put upon a live Coal, which was Gently Blown whilst they were on it, they did neither melt nor fly away, and you may keep a Quantity of this White substance for a good while, (nay for ought I can guess for a very long one) in a red hot Crucible without losing or spoiling it; nor did hot Water wherein I purposely kept another parcel of such *Calx*, seem to do any more than wash away the looser adhering Salts from the stony Substance, which therefore seem'd unlikely to be separable by ablutions (though reiterated) from the Præcipitated parts of the Vegetable, whose Lake is intended. And to shew you, that there is likewise in Allom a Body, with which the fix'd Salt of the Alcalizate Solution will concoagulate into a Saline Substance differing from either of them, I shall add, that I have taken pleasure to recover out of the slowly exhal'd Liquor, that pass'd through the filtre, and left the foremention'd *Calx* behind, a Body that at least seem'd a Salt very pretty to look on, as being very White, and consisting of an innumerable company of exceeding slender, and shining Particles, which / would in part easily melt at the flame of a Candle, and in part flye away with some little noise. But of this substance, and its odd Qualities more perhaps elsewhere; for now I shall only take notice to you, that I have likewise with Urinous Salts, such as the Spirit of Sal Armoniack, as well as with the Spirit of Urine it self, Nay, (if I much mistake not) ev'n with Stale Urine undistil'd, easily Precipitated such a White *Calx*, as I was formerly speaking of, out of a Limpid Solution of Allom, so that there is need of Circumspection in judging of the Natures of Liquors by Precipitations wherein Allom intervenes, else we may sometimes mistakingly

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imagine that to be Precipitated out of a Liquor by Allom, which is rather Precipitated out of Allom by the Liquor: And this puts me in mind to tell you, that 'tis not unpleasant to behold how quickly the Solution of Allom (or injected lumps of Allom) do's occasion the severing of the colour'd parts of the Decoction from the Liquor that seem'd to have so perfectly imbib'd them. /

## *Annot. II.*

The above mention'd way of making Lakes we have tryed not only with Turmerick, but also with Madder, which yielded us a Red Lake; and with Rue, which afforded us an extract, of (almost if not altogether) the same Colour with that of the leaves.

But in regard that 'tis Principally the Alcalizate Salt of the Pot-ashes, which enables the water to Extract so powerfully the Tincture of the Decocted Vegetables, I fear that our Author may be mistaken by supposing that the Decoction will alwayes be of the very same Colour with the Vegetable it is made off. For Lixiviate Salts, to which Pot-ashes eminently belong, though by peircing and opening the Bodies of Vegetables, they prepare and dispose them to part readily with their Tincture, yet some Tinctures they do not only draw out, but likewise alter them, as may be easily made appear by many of the Experiments already set down in this Treatise, and though Allom being of an Acid Nature, its Solutions may in some Cases destroy the Adventitious Colours



produc'd by the Alcaly, and restore the former: yet / besides that Allom is not, as I have lately shown, a meer Acid Salt, but a mixt Body, and besides, that its operations are languid in comparison of the activity of Salts freed by Distillation, or by Incineration and Dissolution, from the most of their Earthy parts, we have seen already Examples, that in divers Cases an Acid Salt will not restore a Vegetable substance to the Colour of which an Alcalizate one had depriv'd it, but makes it assume a third very differing from both, as we formerly told you, that if Syrrup of Violets were by an Alcaly turn'd Green, (which Colour, as I have try'd, may be the same way produc'd in the Violet-leaves themselves without any Relation to a Syrrup) an Acid Salt would not make it Blew again, but Red. And though I have by this way of making Lakes, made Magisteries (for such they seem to be) of Brazil, and as I remember of Cochinele it self, and of other things, Red, Yellow or Green which Lakes were enobled with a Rich Colour, and others had no bad one; yet in some the colour of the Lake seem'd rather inferiour than otherwise to that of the Plant, and in others it seem'd both very differing, and much worse; but Writing this in a time and place where I cannot provide my self of Flowres and other Vegetables to prosecute / such Tryals in a competent variety of Subjects, I am content not to be positive in delivering a judgment of this way of Lakes, till Experience, or You, *Pyrophilus*, shall have afforded me a fuller and more particular Information.

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### *Annotation III.*

And on this occasion (*Pyrophilus*) I must here (having forgot to do it sooner) advertise you once for all, that having written several of the foregoing Experiments, not only in haste but at seasons of the year, and in places wherein I could not furnish my self with such Instruments, and such a variety of Materials, as the design of giving you an Introduction into the History of Colours requir'd, it can scarce be otherwise but that divers of the Experiments, that I have set down, may afford you some matter of new Tryals, if you think fit to supply the deficiencies of some of them (especially the freshly mention'd about Lakes, and those that concern Emphatical Colours) which deficiencies for want of being befriended with accommodations I could better discern than avoid. /

### *Annotation IV.*

The use of Allom is very great as well as familiar in the Dyers Trade, and I have not been ill pleas'd with the use I have been able to make of it in preparing other pigments than those they imploy with Vegetable Juices. But the Lucriforous practises of Dyers and other Tradesmen, I do, for Reasons that you may know when you please, purposely forbear in this Essay, though not strictly from pointing at, yet from making it a part of my present work explicitly and circumstantially to deliver, especially since I now find (though late and not without some Blushes at my prolixity) that what I intended but for a short Essay, is already swell'd into almost a Volume.

### *Experiment L.*

Yet here, *Pyrophilus*, I must take leave to insert an Experiment, though perhaps you'll think its coming in here an Intrusion, For I confess its more proper place would have been among those Experiments, that were brought as proofs and applications of our Notions concerning the differences of / Salts; but not having remembred to insert it in its fittest place, I had rather take notice of it in this, than leave it quite unmention'd: partly because it doth somewhat differ from the rest of our Experiments about Colours, in the way whereby 'tis made; and partly because the grounds upon which I devis'd it, may hint to you somewhat of the Method I use in Designing and Varying Experiments about Colours, and upon this account I shall inform you, not only What I did, but Why I did it.

I consider'd then that the work of the former Experiments was either to change the Colour of a Body

into another, or quite to destroy it, without giving it a successor, but I had a mind to give you also a way, whereby to turn a Body endued with one Colour into two Bodies, of Colours, as well as consistencies, very distinct from each other, and that by the help of a Body that had it self no Colour at all. In order to this, I remembred, that finding the Acidity of Spirit of Vinegar to be wholly destroy'd by its working upon *Minium* (or calcin'd Lead) whereby the Saline particles of the *Menstruum* have

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their Taste and Nature quite alter'd, I had, among other Conjectures I had built upon that change, rightly concluded, that the Solution of Lead / in Spirit of Vinegar would alter the Colour of the Juices and Infusions of several Plants, much after the like manner that I had found Oyl of Tartar to do; and accordingly I was quickly satisfied upon Tryal, that the Infusion of Rose-leaves would by a small quantity of this Solution well mingl'd with it, be immediately turn'd into a somewhat sad Green.

And further, I had often found, that Oyl of Vitriol, though a potently Acid *Menstruum*, will yet Præcipitate many Bodies, both Mineral and others, dissolv'd not onely in *Aqua fortis* (as some Chymists have observ'd) but particularly in Spirit of Vinegar, and I have further found, that the *Calces* or Powders Præcipitated by this Liquor were usually fair and White.

Laying these things together, 'twas not difficult to conclude, that if upon a good Tincture of Red Rose-leaves made with fair Water, I dropp'd a pretty quantity of a strong and sweet Solution of *Minium*, the Liquor would be turn'd into the like muddy Green Substance, as I have formerly intimated to You, that Oyl of Tartar would reduce it to, and that if then I added a convenient quantity of good Oyl of Vitriol, this last nam'd Liquor would have two distinct operations upon the Mixture, the one, that / it would Præcipitate that resolv'd Lead in the form of a White Powder; the other, that it would Clarifie the muddy Mixture, and both restore, and exceedingly heighten the Redness of the Infusion of Roses, which was the most copious Ingredient of the Green composition, and accordingly trying the Experiment in a Wine glass sharp at the bottom (like an inverted Cone) that the subsiding Powder might seem to take up the more room, and be the more conspicuous, I found that when I had shaken the Green Mixture, that the colour'd Liquor might be the more equally dispersed, a few drops of the rectifi'd Oyl of Vitriol did presently turn the opacous Liquor into one that was cleer and Red, almost like a Rubie, and threw down good store of a Powder, which when 'twas settl'd, would have appear'd very White, if some interspers'd Particles of the red Liquor had not a little Allay'd the Purity, though not blemish'd the Beauty of the Colour. And to shew you, *Pyrophilus*, that these Effects do not flow from the Oyl of Vitriol, as it is such, but as it is a strongly Acid *Menstruum*, that has the property both to Præcipitate Lead, as well as some other Concretes out of Spirit of Vinegar, and to heighten the Colour of Red Rose-leaves, I add, that I / have done the same thing, though perhaps not quite so well with Spirit of Salt, and that I could not do it with *Aqua-fortis*, because though that potent *Menstruum* does as well as the others heighthen the Redness of Roses, yet it would not like them Precipitate Lead out of Spirit of Vinegar, but would rather have dissolv'd it, if it had not found it dissolv'd already.

And as by this way we have produc'd a Red Liquor, and a White Precipitate out of a Dirty Green magistery of Rose-leaves, so by the same Method, you may produce a fair Yellow, and sometimes a Red Liquor, and the like Precipitate, out of an Infusion of a curious Purple Colour. For you may call to mind, that in the Annotation upon the 39th. Experiment I intimated to

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you, that I had with a few drops of an Alcaly turn'd the Infusion of Logg-wood into a lovely Purple. Now if instead of this Alcaly I substituted a very strong and well Filtrated Solution of *Minium*, made with Spirit of Vinegar, and put about half as much of this Liquor as there was of the Infusion

of Logg-wood, (that the mixture might afford a pretty deal of Precipitate,) the affusion of a convenient proportion of Spirit of Salt, would (if the Liquors were well and nimbly stirr'd together) presently / strike down a Precipitate like that formerly mention'd, and turn the Liquor that swam above it, for the most part into a lovely Yellow.

But for the advancing of this Experiment a little further, I consider'd, that in case I first turn'd a spoonfull of the infusion of Logg-wood Purple, by a convenient proportion of the Solution of *Minium*, the Affusion of Spirit of Sal Armoniack, would Precipitate the Corpuscles of Lead conceal'd in the Solution of *Minium*, and yet not destroy the Purple colour of the Liquor; whereupon I thus proceeded; I took about a spoonfull of the *fresh* Tincture of Logg-wood, (for I found that if it were *stale* the Experiment would not alwayes succeed,) and having put to it a convenient proportion of the Solution of *Minium* to turn it into a deep and almost opacous Purple, I then drop'd in as much Spirit of Sal Armoniack, as I guess'd would Precipitate about half or more (but not all) of the Lead, and immediately stirring the mixture well together, I mingled the Precipitated parts with the others, so that they fell to the bottom, partly in the form of a Powder, and partly in the form of a Curdled Substance, that (by reason of the Predominancy of the Ting'd Corpuscles over / the White) retain'd as well as the Supernatant Liquor, a Blewish Purple colour sufficiently Deep, and then instantly (but yet Warily,) pouring on a pretty Quantity of Spirit of Salt, the matter first Precipitated, was, by the above specifi'd figure of the bottome of the Glass preserv'd from being reach'd by the Spirituous Salt; which hastily Precipitated upon it a new Bed (if I may so call it) of White Powder, being the remaining Corpuscles of the Lead, that the Urinous Spirit had not struck down: So that there appear'd in the Glass three distinct and very differinglly colour'd Substances; a Purple or Violet-colour'd Precipitate at the bottom, a White and Carnation (sometimes a Variouslly colour'd) Precipitate over That, and at the Top of all a Transparent Liquor of a lovely Yellow, or Red.

Thus you see, *Pyrophilus*, that though to some I may have seem'd to have lighted on this (50th.) Experiment by chance, and though others may imagine, that to have excogitated it, must have proceeded from some extraordinary insight into the nature of Colours, yet indeed, the devising of it need not be look'd upon as any great matter, especially to one that is a little vers'd in the notions, I have in these, and other Papers / hinted concerning the differences of Salts. And perhaps I might add upon more than conjecture, that these very notions and some particulars scatteringly deliver'd in this Treatise, being skilfully put together, may suggest divers matters (at least,) about Colours, that will not be altogether Despicable. But those hinted, *Pyrophilus*,

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I must now leave such as You to prosecute, having already spent farr more time than I intended to allow my self in acquainting You with particular Experiments and Observations concerning the changes of Colour, to which I might have added many more, but that I hope I may have presented You with a competent number to make out in some measure what I have at the beginning of this Essay either propos'd as my Design in this Tract, or deliver'd as my Conjectures concerning these matters. And it not being my present Designe, as I have more than once Declar'd, to deliver any Positive Hypothesis or solemn Theory of Colours, but only to furnish You with some Experiments towards the framing of such a Theory; I shall add nothing to what I have said already, but a request that you would not be forward to think I have been mistaken in any thing I have deliver'd as matter of Fact concerning the changes of Colours, in case you / should not every time you trye it, find it exactly to succeed. For besides the Contingencies to which we have elsewhere shewn some other Experiments to be obnoxious, the omission or variation of a seemingly unconsiderable circumstance, may hinder the success of an Experiment, wherein no other fault has been committed. Of which truth I shall only give you that single and almost obvious, but yet illustrious instance of the Art of Dying Scarlets, for though you should see every Ingredient that is us'd about it, though I should particularly inform You of the weight of each, and though you should be present at the kindling of the fire, and at the increasing and remitting of it, when ever the degree of Heat is to be alter'd, and though (in a word) you should see every thing done so particularly that you would

scarce harbour the least doubt of your comprehending the whole Art: Yet if I should not disclose to You, that the Vessels, that immediately contain the Tinging Ingredients, are to be made of or to be lin'd with Tin, You would never be able by all that I could tell you else (at least, if the Famousest and Candidest Artificers do not strangely delude themselves) to bring your Tincture of / Chochinele to Dye a perfect Scarlet. So much depends upon the very Vessel, wherein the Tinging matters are boyl'd, and so great an Influence may an unheeded Circumstance have on the Success of Experiments concerning Colours.

*FINIS.* /  
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## Observations about Mr. *Clayton's* Diamond

A SHORT  
ACCOUNT  
OF SOME  
OBSERVATIONS  
Made by Mr. *BOYLE*

About a *Diamond* that *Shines* in the Dark.

First enclosed in a Letter written to  
a Friend,

And now together with it annexed to the  
Foregoing Treatise, upon the score of the  
Affinity betwixt  
*Light* and *Colours*.

1664  
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In the original edition, this title-page has the imprint: '*LONDON*, Printed for *Henry Herringman*. 1664.' However, as is implied by the fact that its pagination continues that of the book as a whole, this section was never published separately.

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## A Copy of the Letter that Mr. *Boyle* wrote to Sir *Robert Morray*

A COPY  
OF THE  
LETTER

That Mr. *Boyle* wrote to Sir *Robert Morray*,<sup>†\*</sup>  
to accompany the *Observations* touching  
the *Shining Diamond*.

*SIR,*

Though Sir *Robert Morray*, and Monsieur *Zulichem*<sup>†\*</sup> be Persons that have deserv'd so well of the



Commonwealth of Learning, that I should think my self unworthy to be look'd upon as a Member of it, if I declin'd to Obey them, or to Serve them; yet I should not without Reluctancy send you the Notes, you desire for him, if I did not hope that you will transmit together with them, some Account why they are not less unworthy of his perusal: which, that you may do, I must inform you, how / the writing of them was Occasion'd, which in short was thus. As I was just going out of Town, hearing that an Ingenious Gentleman of my Acquaintance,<sup>†\*</sup> lately return'd from *Italy*, had a Diamond, that being rubb'd, would shine in the Dark, and that he was not far off, I snatch'd time from my Occasions to make him a Visit, but finding him ready to go abroad, and having in vain try'd to make the Stone yield any Light in the Day time, I borrow'd it of him for that Night, upon condition to restore it him within a Day or two at furthest, at *Gresham College*, where we appointed to attend the meeting of the Society, that was then to be at that place.<sup>†\*</sup> And hereupon I hasted that Evening out of Town, and

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finding after Supper that the Stone which in the Day time would afford no discernable Light, was really Conspicuous in the Dark, I was so taken with the Novelty, and so desirous to make some use of an opportunity that was like to last so little a while, that though at that time I had no body to assist me but a Foot-Boy, yet sitting up late, I made a shift that Night to try a pretty number of such of the things that then came into my thoughts, as were not in that place and time unpracticable. And the next Day being otherwise imploy'd, / I was fain to make use of a drowsie part of the Night to set down hastily in Writing what I had observ'd, and without having the time in the Morning, to stay the transcribing of it, I order'd the Observations to be brought after me to *Gresham College*, where you may remember, that they were together with the Stone it self shown to the Royal Society, by which they had the good Fortune not to be dislik'd, though several things were through hast omitted, some of which you will find in the Margin of the inclosed Paper. The substance of this short Narrative I hope you will let Monsieur *Zulichem* know, that he may be kept from expecting any thing of finish'd in the Observations, and be dispos'd to excuse the want of it. But such as they are, I hope they will prove (without a Clinch) Luciferous Experiments, by setting the Speculations of the Curious on work, in a diligent Inquiry after the Nature of Light, towards the discovery of which, perhaps they have not yet met with so considerable an Experiment, since here we see Light produc'd in a dead and opacous Body, and that not as in rotten Wood, or in Fishes, or as in the *Bolonian* Stone, by a Natural Corruption, or by a / Violent Destruction of the Texture of the Body, but by so slight a Mechanical operation upon its Texture, as we seem to know what it is, and as is immediately perform'd, and that several wayes without at all prejudicing the Body, or making any sensible alterations in its Manifest Qualities. And I am the more willing to expose my hasty Tryals to Monsieur *Zulichem*, and to You, because, he being upon the Consideration of Dioptricks, so odd a *Phænomenon* relateing to the Subject, as probably he treats of, Light will, I hope, excite a person to consider it, that is wont to consider things he treats of very well. And for you Sir, I hope you will both recrute and perfect the Observations you receive, For you know that I cannot add to them, having a good while since restor'd to Mr. *Clayton* the Stone, which though it be now in the hands of a Prince that so highly deserves, by understanding them, the greatest Curiosities;<sup>†\*</sup> yet he vouchsafes you that access to him as keeps me from doubting, you may easily obtain leave to make further Tryals with it, of such a Monarch as ours, that is not more inquisitive himself, than a favourer of them that are so. I doubt not but these Notes will put you in mind of the Motion you made to the Society, to impose upon / me the Task of bringing in, what I had on other occasions observ'd concerning shining Bodies.<sup>†\*</sup> But though I deny not, that I sometimes made observations about the *Bolonian*

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Stone, and try'd some Experiments about some other shining Bodies; Yet the same Reasons that reduc'd me then to be unwilling to receive ev'n their commands, must now be my Apology for not

answering your Expectations, Namely the abstruse nature of Light, and my being already overburden'd, and but too much kept imploy'd by the Urgency of the Press, as well as by more concerning and distracting Occasions. But yet I will tell you some part of what I have met with in reference to the Stone, of which I send you an account. Because I find on the one side, that a great many think it no Rarity upon a mistaken perswasion, that not only there are store of Carbuncles, of which this is one; but that all Diamonds and other Glistening Jewels shine in the Dark. Whereas on the other side there are very Learn'd Men, who (plausibly enough) deny that there are any Carbuncles or shining Stones at all.

And certainly, those Judicious men have much more to say for themselves, than the others commonly Plead, and therefore did deservedly look upon Mr. *Clayton's* Diamond / as a great Rarity. For not only *Boetius de Boot*, who is judg'd the best Author on this Subject, ascribes no such Virtue to Diamonds, but begins what he delivers of Carbuncles, with this passage.<sup>†@</sup> *Magna fama est Carbunculi. Is vulgo putatur in tenebris Carbonis instar lucere; fortassis quia Pyropus seu Anthrax appellatus a veteribus fuit. Verum hactenus nemo unquam verè asserere ausus fuit, se gemmam noctu lucentem vidisse. Garcias ab Horto proregis Indiæ Medicus, refert se allocutum fuisse, qui se vidisse affirmarent. Sed iis fidem non habuit.* And a later Author, the Diligent and Judicious *Johannes de Laet* in his Chapter of Carbuncles and of Rubies, has this passage.<sup>†\*</sup> *Quia autem Carbunculi, Pyropi & Anthraces a veteribus nominantur, vulgo creditum fuit, Carbonis instar in tenebris lucere, quod tamen nullâ gemmâ hactenus deprehensum, licet à quibusdam temerè jactetur.* And the recentest Writer I have met with on this Subject, *Olaus Wormius*, in his Account of his well furnish'd *Musæum*, do's, where he treats of Rubies, concurr with the former Writers by these Words.<sup>†@</sup> *Sunt qui*

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*Rubinum veterum Carbunculum esse existimant, sed deest una illa nota, quod / in tenebris instar Anthracis non luceat: Ast talem Carbunculum in rerum naturâ non inveniri major pars Authorum existimant. Licet unum aut alterum in India apud Magnates quosdam reperiri scribant, cum tamen ex aliorum relatione id habeant saltem, sed ipsi non viderint.* In confirmation of which I shall only add, that hearing of a Rubie, so very Vivid, that the Jewellers themselves have several times begg'd leave of the fair Lady to whom it belong'd, that they might try their choicest Rubies by comparing them with That, I had the Opportunity by the Favour of this Lady and her Husband, (both which I have the Honour to be acquainted with) to make a Trial of this famous Rubie in the Night, and in a Room well Darkn'd, but not only could not discern any thing of Light, by looking on the Stone before any thing had been done to it, but could not by all my Rubbing bring it to afford the least Glimmering of Light.

But, Sir, though I be very backward to admit strange things for truths, yet I am not very forward to reject them as impossibilities, and therefore I would not discourage any from making further Inquiry, whether or no there be Really in *Rerum natura*,<sup>†\*</sup> any such thing as a true Carbuncle or Stone that without Rubbing will shine / in the Dark. For if such a thing can be found, it may afford no small Assistance to the Curious in the Investigation of Light, besides the Nobleness and Rarity of the thing it selfe. And though *Vartomannus* was not an Eye witness of what he relates,<sup>†\*</sup> that the King of *Pegu*, one of the Chief Kings of the *East-Indies*, had a true Carbuncle of that Bigness and Splendour, that it shin'd very Gloriously in the Dark, and though *Garcias ab Horto*, the *Indian Vice-Roys* Physician,<sup>†\*</sup> speaks of another Carbuncle, only upon the Report of one, that he Discours'd with, who affirmed himself to have seen it; yet as we are not sure that these Men that gave themselves out to be Eye-witnesses speak true, yet they may have done so for ought we know to the contrary. And I could present you with a much considerabler Testimony to the same purpose, if I had the permission of a Person concern'd, without whose leave I must not do it. I might tell you that *Marcus Paulus Venetus* (whose suppos'd Fables, divers of our later Travellours and Navigatours have since found to be truths) speaking of the King of *Zeilan* that then was, tells us,<sup>†@</sup> that he was said to have the best Rubie in the World, a Palm long and as / big as a mans Arm, without spot,

Officer, sent and offer'd the value of a City for it; But the King answer'd, he would not give it for the Treasure of the World, nor part with it, having been his Ancestours. And I could add, that in the Relation made by two *Russian* Cossacks of their Journey into *Catay*, written to their Emperour,<sup>†@</sup> they mention'd their having been told by the people of those parts, that their King had a Stone, which Lights as the Sun both Day and Night, call'd in their Language *Sarra*, which those Cossacks interpret a Ruby. But these Relations are too uncertain for me to build any thing upon, and therefore I shall proceed to tell you, that there came hither about two years since out of *America*, the Governour of one of the Principal Colonies there, an Ancient *Virtuoso*, and one that has the Honour to be a member of the Royal Society;<sup>†\*</sup> this Gentleman finding some of the chief Affairs of his Country committed to another and me, made me divers Visits, and in one of them when I enquir'd what Rare Stones they had in those parts of the *Indies* he belong'd to, he told me, that the *Indians* had a Tradition / that in a certain hardly accessible Hill, a pretty way up in the Country, there was a Stone which in the Night time shin'd very vividly, and to a great distance, and he assur'd me, that though he thought it not fit to venture himself so far among those Savages, yet he purposely sent thither a bold *Englishman*, with some Natives to be his guides, and that this Messenger brought him back word, that at a distance from the Hillock he had plainly perceiv'd such a shining Substance as the *Indians* Tradition mention'd, and being stimulated by Curiosity, had slighted those Superstitious Fears of the Inhabitants, and with much ado by reason of the Difficulty of the way, had made a shift to clamber up to that part of the Hill, where, by a very heedful Observation, he suppos'd himself to have seen the Light: but whether 'twere that he had mistaken the place, or for some other Reason, he could not find it there, though when he was return'd to his former Station, he did agen see the Light shining in the same place where it shone before. A further Account of this Light I expect from the Gentleman that gave me this, who lately sent me the news of his being landed in that Country. And though I reserve to my self a full Liberty of Believing no more / than I see cause; yet I do the less scruple to relate this, because a good part of it agrees well enough with another Story that I shall in the next place have occasion to subjoyn, in order whereunto I shall tell you, that though the Learned Authors I formerly mention'd, tell us, that no Writer has affirm'd his having himself seen a real Carbuncle, yet, considering the Light of Mr. *Claytons* Diamond, it recall'd into my mind, that some years before, when I

was Inquisitive about Stones, I had met with an old *Italian* Book highly extoll'd to me by very competent Judges, and that though the Book were very scarce, I had purchas'd it at a dear Rate, for the sake of a few considerable passages I met with in it, and particularly one, which being very remarkable in it self, and pertinent to our present Argument, I shall put it for you, though not word for word, which I fear I have forgot to do, yet as to the Sense, into *English*.

*Having promis'd (says our Author<sup>†@</sup>) to say something of that most precious sort of Jewels, Carbuncles, because they are very rarely to be met with, we shall briefly deliver what we know of them. In Clement the seventh's time, I happen'd to see one of / them at a certain Ragusian Merchants, nam'd Beigoio di Bona, This was a Carbuncle white, of that kind of Whiteness which we said was to be found in those Rubies of which we made mention a little above, (where he had said that those Rubies had a kind of Livid Whiteness or Paleness like that of a Calcidonian) but it had in it a Lustre so pleasing and so marvellous, that it shin'd in the Dark, but not as much as colour'd Carbuncles, though it be true, that in an exceeding Dark place I saw it shine in the manner of fire almost gone out. But as for colour'd Carbuncles, it has not been my Fortune to have seen any, wherefore I will onely set down what I Learn'd about them Discoursing in my Youth with a Roman Gentleman of antient Experience in matters of Jewels, who told me, That one Jacopo Cola being by Night in a Vineyard of his, and espying something in the midst of it, that shin'd like a little glowing*



Coal, at the foot of a Vine, went near towards the place where he thought himself to have seen that fire, but not finding it, he said, that being return'd to the same place, whence he had first descry'd it, and perceiving there the same splendor as before, he mark'd it so heedfully, that he came at length to it, where he took up a very little Stone, which he carry'd away with Transports and Joy. And the next / day carrying it about to show it divers of his Friends, whilst he was relating after what manner he found it, there casually interven'd a *Venetian* Embassadour, exceedingly expert in Jewels, who presently knowing it to be a Carbuncle, did craftily before he and the said *Jacopo* parted (so that there was no Body present that understood the Worth of so Precious a Gemm) purchase it for the Value of 10. Crowns, and the next day left *Rome* to shun the being necessitated to restore it, and (as he affirm'd) it was known within some while after that the said *Venetian* Gentleman did in *Constantinople* sell that Carbuncle to the then Grand Seignior, newly come to the Empire, for a hundred thousand Crowns.<sup>†\*</sup> And this is what I can say concerning Carbuncles, and this is not a little at least as to the first part of this account, where our *Cellini* affirms himself to have seen a Real Carbuncle with his own Eyes, especially since this Author appears wary in what he delivers, and is inclin'd rather to lessen, than increase the wonder of it. And

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his Testimony is the more considerable, because though he were born a Subject neither to the Pope nor the then King of *France* (that Royal *Virtuoso Francis* the first) yet both the one and the other of those Princes imploy'd him much / about making of their Noblest Jewels. What is now reported concerning a Shining Substance to be seen in one of the Islands about *Scotland*, were very improper for me to mention to Sr. *Robert Morray*, to whom the first Information was Originally brought, and from whom I expect a farther (for I scarce dare expect a convincing) account of it.<sup>†\*</sup> But I must not omit that some *Virtuoso* questioning me the other day at *White-Hall* about Mr. *Claytons* Diamond, and meeting amongst them an Ingenious *Dutch* Gentleman, whose Father was long Ambassador for the Netherlands in *England*,<sup>†\*</sup> I Learn'd of him, that, he is acquainted with a person, whose Name he told (but I do not well remember it) who was Admiral of the *Dutch* in the *East-Indies*, and who assur'd this Gentleman *Monsieur Boreel*, that at his return from thence he brought back with him into *Holland* a Stone, which though it look'd but like a Pale Dull Diamond, such as he saw Mr. *Claytons* to be, yet was it a Real Carbuncle, and did without rubbing shine so much, that when the Admiral had occasion to open a Chest which he kept under Deck in a Dark place, where 'twas forbidden to bring Candles for fear of Mischances, as soon as he open'd the Trunck, the Stone / would by its Native Light, shine so as to Illustrate a great part of it, and this Gentleman having very civilly and readily granted me the request I made him, to Write to the Admiral, who is yet alive in *Holland*, (and probably may still have the Jewel by him,) for a particular account of this Stone, I hope ere long to receive it, which will be the more welcome to me, not onely because so unlikely a thing needs a cleer evidence, but because I have had some suspicion of that (supposing the truth of the thing) what may be a shining Stone in a very hot Countrey as the *East-Indies*, may perhaps cease to be so (at least in certain seasons,) in one as cold as *Holland*. For I observ'd in the Diamond I send you an account of, that not onely rubbing but a very moderate degree of warmth, though excited by other wayes, would make it shine a little. And 'tis not impossible that there may be Stones as much more susceptible than that, of the Alterations requisite to make a Diamond shine, as that appears to be more susceptible of them, than ordinary Diamonds. And I confess to you, that this is not the only odd suspicion (for they are not so much as conjectures) that what I try'd upon this Diamond suggested to me. For not here to entertain you with the / changes I think may be effected ev'n in harder sorts of Stones, by wayes not

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vulgar, nor very promising, because I may elsewhere have occasion to speak of them, and this Letter is but too Prolix already, that which I shall now acknowledge to you is, That I began to doubt whether there may not in some Cases be some Truth, in what is said of the right *Turquois*, that it



often changes Colour as the wearer is Sick or Well, and manifestly loses its splendor at his Death.

For when I found that ev'n the warmth of an Affriction that lasted not above a quarter of a minute, Nay, that of my Body, (whose Constitution you know is none of the hottest) would make a manifest change in the solidest of Stones a Diamond, it seem'd not impossible, that certain warm and Saline steams issuing from the Body of a living man, may by their plenty of paucity, or by their peculiar Nature, or by the total absence of them, diversifie the Colour, and the splendor of so soft a Stone as the Turquois. And though I admir'd to see, that I know not how many Men otherwise Learn'd, should confidently ascribe to Jewels such Virtues as seem no way competible to Inanimate Agents, if to any Corporeal ones at all, yet as to what is affirm'd concerning the

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Turquois's / changing Colour, I know not well how to reject the Affirmation of so Learned (and which in this case is much more considerable) so Judicious a Lapidary as *Boetius de Boot*,<sup>†@</sup> who upon his own particular and repeated Experience delivers so memorable / a Narrative of the Turquois's changing Colour, that I cannot but think it worth your Perusal, especially since a much later and very Experienc'd Author, *Olaus Wormius*, where he treats of that Stone, Confirms it with this Testimony.<sup>†@</sup> *Imprimis memorandum exemplum quod Anshelmus Boëtius de seipso refert, tam mutati Coloris, quam à casu preservationis. Cui & ipse haud dissimile adferre possum, nisi ex Anshelmo petitum quis putaret.* I remember that I saw two or three years since a *Turcois* (worn in a Ring) wherein there were some small spots, which the *Virtuoso* whose it was assur'd me he had observ'd to grow sometimes greater sometimes less, and to be sometimes in one part of the Stone, sometimes in another. And I having encourag'd to make Pictures from time to time of the Stone, and of the Situation of the cloudy parts, that so their Motion may be more indisputable, and better observ'd, he came to me about the midle of this very week, and assur'd me that he had, as I wish'd, made from time to time Schemes or Pictures of the differing parts of the Stone, whereby the several Removes and motions of the above mentioned Clouds are very manifest, though the cause seem'd to him very occult: these Pictures / he has promis'd to show me, and is very ready to put the Stone it self into my hands. But the ring having been the other day casually broken upon his finger, unless it can be taken out, and set again without any considerable heat, he is loath to have it medled with, for fear its peculiarity should be thereby destroy'd. And possibly his apprehension would have been strengthen'd, if I had had opportunity to tell him what is reladed by the Learned *Wormius* of an acquaintance of his,<sup>†@</sup> that had a *Nephritick* stone, of whose eminent Virtues he had often Experience ev'n in himself, and for that cause wore it still about his Wrist, and yet going upon a time into a Bath of fair Water only, wherein certain Herbs had been boyl'd, the Stone by being wetted with this decoction, was depriv'd of all his Virtue, whence *Wormius* takes Occasion to advertise the sick, to lay by such stones whensoever they make use of a Bath. And we might expect to find *Turcois* likewise, easily to be wrought upon in point of Colour, if that were true, which the curious *Antonio Neri*, in his ingenious *Arte Vetraria* teaches of it,<sup>†@</sup> namely, That *Turcois's* discolour'd and grown white, will regain and acquire / an excellent Colour, if you but keep them two or three days at most cover'd with Oyl of sweet Almonds kept in a temperate heat by warm ashes, I say if it were true, because I doubt whether it be so, and have not as yet had opportunity to satisfie my self by Tryals, because I find by

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the confession of the most Skilfull Persons among whom I have laid out for *Turcoises*, that the true ones are great rarities, though others be not at all so. And therefore I shall now only mind you of one thing that you know as well as I, namely, that the rare Stone which is called *Oculus Mundi*, if it be good in its Kind, will have so great a change made in its Texture by being barely left a while in the Languidest of Liquors, common Waters, that from Opacous it will become Transparent, and acquire a Lustre of which it will again be depriv'd, without using any other Art or Violence, by leaving it a while in the Air. And before experience had satisfy'd us of the truth of this, it seem'd as

unlikely that common Water or Air, should work such great changes in that Gemm, as it now seems that the Effluviūms of a human Body should effect lesser changes in a *Turcois*, especially if more susceptible of them, than other Stones of the same kind. But both my Watch and my Eyes tell me that / 'tis now high time to think of going to sleep, matters of this Nature, will be better, as well as more easily, clear'd by Conference, than Writing. And therefore since I think you know me too well to make it needfull for me to disclame Credulity, notwithstanding my having entertain'd you with all these Extravagancies; for you know well, how wide a difference I am wont to put betwixt things that barely *may be*, and things that *are*, and between those Relations that are but not unworthy to be inquir'd into, and those that are not worthy to be actually believ'd; without making Apologies for my Ravings, I shall readily comply with the drowsiness that calls upon me to release You, and the rather, because Monsieur *Zulichem* being concern'd in your desire to know the few things I have observed about the shining Stone. To entertain those with Suspensions that are accustomed not to acquiesce but in Demonstrations, were a thing that cannot be look'd upon as other than very improper by,

SIR,

*Your most Affectionate*

and

*most Faithfull Servant,*

Ro. Boyle./

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## Observations

### OBSERVATIONS

Made this †@ 27th. of  
*October* 1663. about  
Mr. *Clayton's*  
Diamond. †\*

Being look'd on in the Day time, though in a Bed, †@ whose Curtains were carefully drawn, I could not discern it to Shine at all, though well Rubb'd, but about a little after Sun-set, whilst the Twilight yet lasted, Nay, this Morning a pretty while after Sun-rising, (but before I had been abroad in the more freely inlightned Air of the Chamber) I could upon a light Affrication easily perceive the Stone to Shine. /

Secondly, The Candles being removed, I could not in a Dark place discern the Stone to have any Light, when I looked on it, without having †\* Rubb'd or otherwise prepar'd it.

Thirdly, By two white Pibbles though hard Rubb'd one against another, nor by the long and vehement Affrication of Rock Crystal against a piece of Red cloath, nor yet by Rubbing two Diamonds set in Ring, as I had Rubb'd this Stone, I could †\* produce any sensible degree of Light.

Fourthly, I found this Diamond hard enough, not only to enable me to write readily with it upon Glass, but to Grave on Rock Crystal it self.

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Fifthly, I found this to have like other Diamonds, an Electrical faculty.<sup>†@</sup>

Sixthly, Being rubb'd upon my Cloaths, as is usual for the exciting of Amber, Wax, and other Electrical Bodies, it did in the Dark manifestly shine like Rotten Wood, or the Scales of Whittings, or other putrified Fish.

Seventhly, But this Conspicuousness was Fainter than that of the Scales, and Slabber (if I may so call it) of Whittings, and much Fainter than the Light of a Glow-worm, by / which I have been sometimes able to Read a short Word, whereas after an ordinary Affriction of this Diamond I was not able to discern distinctly by the Light of it any of the nearest Bodies: And this Glimmering also did very manifestly and considerably Decay presently upon the ceasing of the Affriction, though the Stone continued Visible some while after.

Eighthly, But if it were Rubb'd upon a convenient Body for a pretty while, and Briskly enough, I found the Light would be for some moments much more considerable, almost like the Light of a Glow-worm, insomuch<sup>†\*</sup> after I ceased Rubbing, I could with the Chaf'd stone exhibit a little Luminous Circle, like that, but not so bright as that which Children make by moving a stick Fir'd at the end, and in this case it would continue Visible about seven or eight times as long as I had been in Rubbing it.

Ninthly,<sup>†@</sup> I found that holding it a while near the Flame of a Candle, (from which yet I was carefull to avert my Eyes) and / being immediately remov'd into the Dark, it disclosed some faint Glimmering, but<sup>†\*</sup> inferiour to that, it was wont to acquire by Rubbing. And afterward holding it near a Fire that had but little Flame, I found the Stone to be rather less than more excited, than it had been by the Candle.

Tenthly, I likewise indeavour'd to make it Shine, by holding it a pretty while in a very Dark place, over a thick piece of Iron, that was well Heated, but not to that Degree as to be Visibly so. And though at length I found, that by this way also, the Stone acquired some Glimmering, yet it was less than by either of the other ways above mention'd.

Eleventhly, I also brought it to some kind of Glimmering Light, by taking it into Bed with me, and holding it a good while upon a warm part of my Naked Body.

Twelfthly, To satisfie my self, whether the Motion introduc'd into the Stone did generate the Light upon the account of its producing Heat there, I held it near the Flame of a Candle, till it was qualify'd to shine pretty well in the Dark, and then immediately I apply'd a slender Hair to try whether it would

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attract it, but found not that it did so; though if it were made to / shine by Rubbing, it was as I formerly noted Electrical. And for further Confirmation, though I once purposely kept it so near the hot Iron I just now mention'd, as to make it sensibly Warm, yet it shin'd more Dimly than it had done by Affriction or the Flame of a Candle, though by both those ways it had not acquir'd any warmth that was sensible.

Thirteenthly, Having purposely rubb'd it upon several Bodies differing as to Colour, and<sup>†\*</sup> as to Texture, there seem'd to be some little Disparity in the excitation (if I may so call it) of Light. Upon White and Red Cloths it seem'd to succeed best, especially in comparison of Black ones.

Fourteenthly, But to try what it would do rubb'd upon Bodies more hard,<sup>†\*</sup> and less apt to yield Heat upon a light Affriction, than Cloath, I first rubb'd it upon a white wooden Box, by which it was

excited, and afterwards upon a piece of purely<sup>†\*</sup> Glazed Earth, which seem'd during the Attrition to make it Shine better than any of the other Bodies had done, without excepting the White ones, which I add, lest the Effect should be wholly ascrib'd to the disposition White Bodies are wont to have to Reflect much Light. /

Fifteenthly,<sup>†@</sup> Having well excited the Stone, I nimbly plung'd it under Water, that I had provided for that purpose, and perceiv'd it to Shine whilst it was beneath the Surface of that Liquor, and this I did divers times. But when I endeavour'd to produce a Light by rubbing it upon the lately mentioned Cover of the Box, the Stone and it being both held beneath the Surface of the Water, I did not well satisfie my self in the Event of the Trial; But this I found, if I took the Stone out, and Rubb'd it upon a piece of Cloath, it would not as else it was wont to do, presently acquire a Luminousness, but needed to be rubb'd manifestly much longer before the desired Effect was found.

Sixteenthly,<sup>†@</sup> I also try'd several times, that by covering it with my / warm Spittle (having no warm Water at hand) it did not lose his Light.

Seventeenthly, Finding that by Rubbing the Stone with the Flat side downwards, I did by reason of the Opacity of the Ring, and the sudden

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Decay of Light upon the ceasing of the Attrition, probably lose the sight of the Stones greatest Vividness; and supposing that the Commotion made in one part of the Stone will be easily propagated all over, I sometimes held the piece of Cloath upon which I rubb'd it, so, that one side of the Stone was exposed to my Eye, whilst I was rubbing the other, whereby it appear'd more Vivid than formerly, and to make Luminous Tracts by its Motions too and fro. And sometimes holding the Stone upwards, I rubb'd its Broad side with a fine smooth piece of Transparent Horn, by which means the Light through that Diaphanous Substance, did whilst I was actually rubbing the Stone, appear so Brisk, that sometimes and in some places it seem'd to have little Sparks of Fire.

Eighteenthly, I took also a piece of flat Blew Glass, and having rubb'd the Diamond well upon a Cloath, and nimbly clapt the Glass upon it, to try whether in case the Light could peirce it, it would by / appearing Green, or of some other Colour than Blew, assist me to guess whether it self were sincere or no. But finding the Glass impervious to so faint a Light, I then thought it fit to try whether that hard Bodies would not by Attrition increase the Diamonds Light so as to become penetrable thereby, and accordingly when I rubb'd the Glass briskly upon the Stone, I found the Light to be Conspicuous enough, and somewhat Dy'd in its passage, but found it not easie to give a Name to the Colour it exhibited.

Lastly, To comply with the Suspition I had upon the whole Matter, that the chief manifest Change wrought in the Stone, was by Compression of its parts, rather than Incalescence, I took a piece of white Tile well Glaz'd, and if I press'd the Stone hard against it, it seem'd though I did not rub it to and fro, to shine at the Sides: And however it did both very manifestly and vigorously Shine, if whilst I so press'd it, I mov'd it any way upon the Surface of the Tile, though I did not make it draw a Line of above a quarter of an Inch long, or thereabouts. And though I made it not move to and fro, but only from one end of the short Line to the other, without any return or Lateral motion. Nay, after it had been / often rubb'd, and suffer'd to lose its Light again, not only it seem'd more easie to be excited than at the beginning of the Night; but if I did press hard upon it with my Finger, at the very instant that I drew it briskly off, it would disclose a very Vivid but exceeding short Liv'd Splendour, not to call it a little Coruscation.<sup>†@</sup> So that a *Cartesian* would scarce scruple to think he had found in this Stone no slight Confirmation of his Ingenious Masters *Hypothesis*, touching the Generation of Light in Sublunary Bodies, not sensibly Hot./

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Annexed some Hours after the  
Observations were Written.

*So many particulars taken notice of in one Night,<sup>†@</sup> may make this Stone appear a kind of Prodigie, and the rather, because having try'd as I formerly noted, not only a fine Artificial Crystal, and some also that is Natural, but a Ruby and two Diamonds, I did not find that any of these disclos'd the like<sup>†\*</sup> Glimmering of / Light; yet after all, perceiving by the Hardness, and the Testimony of a Skilfull Gold-smith, that this was rather a Natural than Artificial Stone; for fear lest there might be some difference in the way of Setting, or in the shape of the Diamonds I made use of, neither of which was like this, a flat Table-stone, I thought fit to make a farther Trial of my own Diamonds, by such a brisk and assiduous Affriction as might make amends for the Disadvantages above-mention'd, in case they were the cause of the unsuffessfulness of the former Attempts: And accordingly I found, that by this way I could easily bring a Diamond I wore on my Finger to disclose a Light, that was sensible enough, and continued so though I cover'd it with Spittle, and us'd some other Trials about it. And<sup>†\*</sup> this will much lessen the wonder of all the formerly mention'd Observations, by shewing that the properties that are so strange are not peculiar to one Diamond, but may be found in others also, and perhaps in divers other hard and Diaphanous Stones. Yet I hope that what this Discovery takes away from the Wonder of these Observations, it will add to the Instructiveness of them, by affording pregnant Hints, towards the Investigation of the Nature of Light.*

FINIS. /  
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## ***New Experiments and Observations Touching Cold (1665)***

NEW  
EXPERIMENTS  
AND  
OBSERVATIONS  
TOUCHING  
COLD,  
OR AN  
EXPERIMENTAL HISTORY  
OF  
COLD,  
Begun.

To which are added  
An *Examen* of Antiperistasis,  
And  
An *Examen* of Mr. Hobs's Doctrine  
about COLD.

By the Honorable Robert Boyle, Fellow of  
the ROYAL SOCIETY.  
Whereunto is annexed An Account of Freezing,  
brought in to the Royal Society, by the learned  
Dr. C. Merret, a Fellow of it.

*Non fingendum, aut excogitandum, sed inveniendum, quid natura faciat,  
aut ferat, Bacon.*

1665  
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'What Nature may do or bring forth is not to be contrived or imagined, but rather uncovered.' For the source of this quotation from Francis Bacon, see above, p. 4.

In the original edition, this title-page has the imprint: '*LONDON*, Printed for *John Crook*, at the Sign of the Ship in *St Pauls Church-yard*, MDCLXV.

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### **The Publisher to the Ingenious Reader.**

I am fully perswaded, you will much rejoyce to see that Exquisite searcher of Nature, the Illustrious *Robert Boyle*, come abroad again, as knowing he never does so, but when richly furnisht with very Instructive and Useful matter. He presents you here with a Treatise of *New Observations and Experiments, in order to an Experimental History of Cold*. This is the Body of the Book, but it comes accompanied with some *Preliminaries*, and an *Appendix*, whereof the former contains *New Thermometrical Experiments and Thoughts*, the latter an *Exercitation* about the *Doctrine of Antiperistasis*, followed with a short *Examen* of Mr. Hobs's *Doctrine, touching / Cold*.<sup>†\*</sup> From all which it will more and more become manifest, with what spirit and care this Excellent Person advanceth real Philosophy, with what exactness he pursueth his Engagement therein, and how great caution he useth, that nothing may slide into the Philosophical store, that may prove prejudicial to the Axioms and Theories hereafter perhaps to be deduc'd from thence.

Having thus shortly given you my sense of the substance of this Considerable Treatise, I am now to advertise you of one or two circumstances, necessary to be taken notice of in its perusal.

One is, that the Noble Author being at *Oxford*, when the Book was printed at *London*, he hopes the Reader will not impute to him the Errors of the Press, which yet he is perswaded will not be many, and out of which must be excepted a Blank or two, occasion'd by this. That the Authors Papers being near two years since given to be transcribed to one, whose skill in writing was much greater, than (as it afterwards appear'd) / his knowledge of what was, or was not good sense, or true English; this person suddenly going for *Africk* before the Transcript had been examin'd, and not taking care to leave all the first Copy,<sup>†\*</sup> the Author found, (besides several Blanks, that he filled up out of his Memory, or by repeating the Experiments, they belonged

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to) one or two, where he was not able to repair the Copists omissions: And besides unexpectedly met with very many Passages so miserably handled, that by putting him to the trouble of writing almost a New Book, when part of this was already in the Press, it much retarded the Publication of that, which now comes forth.

The other is, That, whereas in the Preface some passages are so penned, as to suppose the Book to be published early in the Winter, the Reader is to be advertis'd, That the former part of the Preface was sent a good while since to the Press, though the latter, however then written out, was hindred from accompanying it, by some hopes of the Authors to gain / by delay an opportunity (he missed of) to perfect an Experiment he was desirous to insert; and that, when the Frost began, which was late in the season, the Coldness did within a while arrive at that degree, that by its operation upon

the moisten'd paper, it long put a stop to the Proceedings of the Press. But the Author, that he might neither be quite defeated of his aim, nor disappoint the Curious of their Expectation, did in the first or second week of the Frost, which was about the end of the year 1664. present the *Royal Society* with divers Copies of the *History of Cold*, though the Book were not then quite printed off. And these Books being so near finish'd, that of 21. Sections, whereof the *History of Cold* consists, the Press had then reach'd to about the 19. and I had the 20. in my hands to supply it, when the weather should permit; the Author hop'd, that by seasonably communicating so much of his intended Treatise to so many of the *Virtuosi*, that were the likeliest to make use of it, he had pretty well provided against / the Prejudice, that might otherwise accrue from the slowness of the Press, and therefore allow'd himself to subjoyn to the *History*, the discourse of *Antiperistasis*, and the *Examen* of Mr. *Hobs*'s Doctrine,<sup>†@</sup> as belonging to the same subject: And finding the frosty weather to continue later, than was expected (which had he foreseen, before his *History* was printed off, it would have given him opportunity of Enlargements) he hopes the Publication may not be yet too late for diligent Readers, to make some use of the season for examining his Experiments, or trying some of the New ones, those may suggest. And therefore for the quicker dispatch of the Book, he purposely omits, and reserves for another occasion, besides the papers, that he hath not yet given me, some that I have already in my hands. And 'tis, I presume, for the same reason, that he forbears to publish, what he long since writ about the *Origine of Forms and Qualities*,<sup>†\*</sup> in a small / Tract, which he had thoughts of sending forth in the company of the ensuing *History*, as a Discourse fit to be an Introduction as well to That, as to his Historical writings about *Colours* and some other Qualities.

This is all the Advertisement I had to give you. And seeing it would be altogether impertinent, for me to take any pains, or to use any Art to procure

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a Gust for a Book, composed by Mr. *Boyle*, I have no more to say, but that the Author being so Generous as to oblige Forrain Nations as well as his Own, has already taken care of having it put into Latine,<sup>†\*</sup> Farewel,

*London, March 10.*

1664/5

*H.O.*<sup>†\*</sup> /

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## The Authors

### Preface

### Introductory.

Cold is so barren a subject, and affords so few Experiments, that are either very delightful for their surprizing prettiness, or very considerable for their immediate use, that instead of admiring, that any of my friends should wonder at my having been induc'd to write of such a Theme, I freely confess, that I have been sometimes tempted to wonder at it my self; and therefore I think my self oblig'd to give my Readers an account of these three things, *Why* I thought fit to write of Cold at all? *For what Reasons* I have treated of it after the manner to be met with in the ensuing Book? And, *Why* I venture my unfinished Collections about it, abroad so soon?

*I.* To satisfie the first of these Queries, I have several things to say. /

And first, That the subject I have chosen is very noble, and important; for since *Heat* has so general

an Interest in the Productions of Natures *Phænomena*, that (Motion excepted, of which it is a kind) there is scarce any thing in Nature, whose Efficacy is so great, and so diffus'd, it seems not likely, that its Antagonist, *Cold*, should be a despicable Quality. And certainly Cold, and Heat, especially when imploy'd by turns, are the two grand Instruments by which Nature performs so many of her Operations here below, that our great *Verulam* did not speak so inconsiderately, when he called Heat the Right hand of Nature, and Cold her Left.<sup>†\*</sup> And though in our temperate Climate the Effects of Cold seem not to be very remarkable, yet besides that, in more Northern Regions they are oftentimes stupendious, the Nature of that Quality must needs be very well worth our considering, if it were but for the Power it has to moderate and check the Operations of Heat, upon which account alone, if there were no other, it may be look'd upon as so considerable a Quality, that even lesser Discoveries about it, may both be acceptable and prove useful. /

In the next place I shall represent, that notwithstanding Cold's being so important a subject, it has hitherto been almost totally neglected. For I remember not, that any of the Classick Authors, I am acquainted with, hath said any thing of it that is considerable. They do indeed generally treat of it, as one of the four first Qualities. But that which they are wont to say,

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amounts to little more, then that 'tis a *Quality that does congregate both things of like, and things of unlike nature*. The Unsatisfactoriness of which vulgar Definition, I had some years ago an occasion to manifest (in another Treatise.<sup>†@</sup>) And having given us this inconsiderate Description of Cold, they commonly take leave of the subject, as if it deserved no further handling, then could be afforded it in a few Lines, wherein indeed they say *too much* about it, but not *enough*. And even among other Writers of Bodies of Natural Philosophy, or of the Doctrine of Meteors it self, the Reader will find, how little of true and pertinent has been contributed to the ensuing *History of Cold*. And though among the Vulgar, and the Writers that adopt their Traditions without examining them, / I find some few particulars delivered, touching Cold; yet some of them are so *untrue*, and others so *uncertain*, that they have furnished me with little else, then the necessity of Questioning, or of disproving them: so that when I considered all these things, I could not but take notice, that very little has been hitherto said of Cold, by those Schoolmen, and other Writers, (that I have yet met with) who have *professedly*, (though but perfunctorily, and, as it were, incidentally) treated of it. But yet instead of thinking it a Discouragement, that so many Learned Men, to whom that Quality could not but be obvious, and to whom it was as familiar as to me, had in so many Ages said little or nothing of it to the purpose; I found this very thing an invitation to my attempt, that I might in some measure repair the Omissions of Mankinds Curiosity towards a subject so considerable, and so diffus'd, by trying what I could do toward founding the History of a Quality, which has been hitherto so neglected, as if all men judg'd it either unworthy of being *cultivated*, or incapable to be *improved*.

Another inducement to me was, that / having six or seven years ago written some Tracts (though I have not since had opportunity to publish them) in order to the History of *Heat and Flame*;<sup>†\*</sup> it seem'd the more proper for me to treat of the contrary quality, *Cold*, since according to the known rule, *confronted Opposites give themselves a mutual Illustration*. And another inducement of almost the same Nature, was afforded me by remembering, that whereas Cold in its higher degrees, is wont to be communicated to us by the Air, (whencesoever the Air has it,) and I *have* on several occasions been oblig'd to treat of divers Properties of the Air, as of its *weight* and *spring* (in my *Physico-Mechanical Treatise* of the several *strengths* of that spring, in proportion to the degrees of the *airs Condensation*, the Experiments of which, reduc'd into Tables, were first *publish'd* (and for ought I yet know *made*) by us, (in the defence of that Book against *Franciscus Linus*.<sup>†@</sup>) and of divers *other Qualities* of the air in several passages of our other writings, which 'twere now superfluous to take notice of; all this made it appear convenient enough, that



among other *Attributes* of the Air, which we / either *have had*, or *expect to have* occasion to treat of, so eminent and diffus'd a one as its *Coldness*, should not be left untouch'd by the same Pen.

But though neither any, nor all these inducements had been sufficient to ingage me to draw together, and recruit my Observations concerning Cold, there was another, that could not miss of prevailing, *The Command of the Royal Society*, impos'd on me in such a way, that I thought, it would less misbecome me to obey it unskilfully, then not at all.†\* Especially since from so *Illustrious a Company* (where I have the Happiness not to be hated) I may in my endeavours to obey and serve them, hope to find my failings both pardoned, and made Occasions of discovering the Truths, I aim'd at.

II. After this Account of the *Motives* that induc'd me to resolve to draw together the Notes I had on several occasions set down, about the *Phænomena* of Cold, it may be now expected, that I render some reason, why I have *thus* digested them, and why I have not written the following Treatise in a more accurate way.

First then I readily acknowledge, that the Method is not exact. Nay, that it is / less so then the Scheme of heads of Inquiry, that I drew up to give my self a general Prospect of the subject I was to handle. But when I had considered, how comprehensive a Theme I had pitch'd upon, and how much more comprehensive, future discoveries and hints might make it, I thought, it altogether unadvisable for me, that had no more time, nor no more opportunity then I had, when I began to compile the following History, to engage my self to a method, according to which I was not perhaps able to treat of any one of the principal parts of the designed History. And yet on the other side, being unwilling to huddle my Experiments confusedly together, I thought it an expedient, that might in great part decline both those Inconveniences, to draw up a company of comprehensive Titles, under which might commodiously be rang'd most of the Particulars I had observ'd, reserving those few, that were not so easily referable to any of those, to be thrown at last into a Section by themselves. And this I the rather did, because I would not, by a Confinement to a strict method, discourage others from continuing the History, by adding new Titles to those 21. I have / treated of, as well as by inserting other Experiments or Observations in any of them.

That the Sections or Titles are very unequal, will not, I presume, be much blam'd by them, that consider, that my Design being to set down Matters of Fact, not write a complete and regular Treatise, the length of each Section was to be determined not by its Proportion, to that which went before it or followed after it, but by the number and condition of the Particulars that were to compose it. And I thought it much more pardonable, that any of the

Sections should be disproportionately short, then lengthened either by untruths or by impertinencies.

Some of the accounts will probably to some Readers appear too prolix; and I could very easily, as well as willingly, have prevented that objection, if I had not more consulted the scope of the Book, then the ease or Reputation of the Writer. But my design being, not only to gratifie some Readers, but to assist others to prosecute the work I had begun, as the Experiments are most of them new, and many of them tri'd by methods hitherto unpractis'd; I conceiv'd my self oblig'd to set down somewhat / circumstantially, not only the Events, but the Manner of my Trials, that I might at once, both the better satisfy the scrupulous, and be assistant to those that would examine or repeat such Experiments, and also gratifie those, who are pleas'd to think, that a somewhat assiduous Conversation with Nature, may have given me some little faculty in devising Experiments, and the ways of making them, above those that have been conversant only with Books and Notions. And in

some of the following Trials I was the more induc'd to set down all the principal circumstances, because, that being not to be made, but by the help of Glasses skilfully shap'd, and Hermetically seal'd, and other Instruments and Operations, that require more tools, and more of manual Dexterity, then every ingenious Man is Master of; 'tis very likely that most Readers will not be able, or perhaps willing, to reiterate such Trials, and therefore will be glad to find them so deliver'd, as that they may without too much danger acquiesce in them, as being made with Diligence, as well as Faithfulness. The latter of which Qualities will, I presume, be allow'd me, as well upon the account of / the plain and simple way, wherein matters of fact are delivered in the following Book, as upon the score of the Testimonies, that even Adversaries, as well as others, have thought fit to give to the Historical part of my former Treatises. And (to intimate That on this occasion) this strict Fidelity to Truth I scruple not to own, though perhaps it may be attended with an inconvenience in point of Reputation, that writers of less Veracity are less exposed to. For I have found by Experience, that *some* Men, who probably would not mention the Experiments of most others, without vouching their Authors, for fear of losing their own credit, in case the thing related should not prove true; have, without taking the least notice of me, made use of such Experiments of mine, as I have strong motives to think they never made nor saw, only because they had been related by one, after whom they thought they might without a hazard of their credit deliver any Matter of Fact. And the liberty that *some* have allow'd themselves in adopting my Communications (such as they are) is notorious enough to have been publicly complain'd of more then once, by Persons that are meer / strangers to me. But though I had not the Probability, which the Notice, that begins to be taken of it, seems to give me, of having some Justice done me: yet Veracity is a Quality that does, I think, so well become a Christian and a writer of Natural History, that I had much rather undergo any Disadvantage, I may be subject to for it, then decline the Practice of it. But to return to the following History.

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I confess the Prolixity of some passages of our History is increas'd by the transitions, excuses, and suspicions that are made use of in them; but I confess too, That if this way of writing be a fault, it was not always caus'd by inadvertency. For as to what is said to connect the parts of our History together, or excuse the not prosecuting of this or that particular Trial, the heedful Reader may oftentimes perceive, that they contain in them, though not perhaps conspicuously, either cautions, or advertisements, or hints, not impertinent to my main scope, and improvable by an attentive Peruser. And as for the suspicions and scruples to which now and then I may seem to have too long indulg'd, I had two or three inducements to invite me to what I did. For the mention / of conjectures, that every Reader was not so likely to light upon, might more conduce, then at first one would think, to the main design of my Book, which was to begin, and promote the natural History of Cold, since these suspicions about the causes and scruples about other things, relating to our Experiments, may probably produce, not only new reasonings and Inquiries, but new Trials to clear the difficulties and determine the doubts. Besides I thought it not amiss to take such occasions to make some Readers sensible, that to make indubitable inferences even from certain Experiments, is not near so easie a work, as many are pleas'd to imagine. And whereas I was not without inducements to think, that some Critical and Sagacious Readers, will not only excuse my having taken notice of so many scruples, but wish I had mov'd more on some occasions, and propos'd some in certain cases, where I have not mention'd any, I thought it might invite such Jealous Readers to think, that I foresaw divers little difficulties and scruples, that might be mov'd in several cases, where I have not expressly taken notice of them, either because I judg'd them easie enough to be / answered without my help, or because the things themselves were not considerable enough to deserve a long or solicitous discourse to clear them, especially from a Writer, that being often tir'd himself in examining such Niceties, was affraid he should too much tire the generality of his Readers, if he should too frequently insist upon them.

If it be objected, that notwithstanding some things are set down proluxly, yet other Experiments, that might properly be referred to some of the Titles I handle, are wholly omitted; I answer, that this

were indeed a fault in one, that should pretend to write full and accurate Discourses of the subjects propos'd in his Titles, but not in me, who do not at all pretend to say under each head all that may be pertinently referr'd to it, (for that may probably be a great deal more then is yet come to my Knowledge) but only those Particulars, that I my self have tri'd or observ'd, or at least have receiv'd upon credible Testimony. And perhaps some amends may be made for these Omissions, by my having frequently enough mention'd the Experiments, that, when I propos'd them, I had only design'd or attempted without / perfecting them. For the Experience of many Ages has shewn us, that hitherto, not only men do not appear to have *made* any store of Trials concerning Cold, but seem not to have so much as *design'd* it. And therefore it seem'd not unreasonable to

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presume, that it would prove an Assistance to the Generality of Readers, if probable and practicable Experiments were propos'd to them. And since 'tis the improvement of the subject that I aim at, by whomsoever it may happen to be improv'd, I thought it but reasonable to use my indeavour, that those Experiments, which for want of opportunity I my self could not try, might be tri'd by others, who may be befriended by more favourable Circumstances. Nor is that great Ornament and Guide of Philosophical Historians of Nature, the Lord *Verulam* himself, asham'd to substitute, on I know not how many occasions, his *Fiat Experimentum*, that is a *precept* or a *wish* to have an Experiment made, instead of an Account of the Experiment made already.<sup>†\*</sup> And yet in this mention of things, I could wish to have tri'd, I have been far more sparing then every Reader will take notice of. For I judg'd it not discreet to / mention all the Experiments I had thought upon, or even already set down in several Catalogues, lest they should appear extravagant to those, that are unacquainted with the several Notions, and trials, and contrivances, which made them appear to me not irrational, and which yet 'twould have been tedious, and not worth while to have particularly mention'd.

But that in what we have newly (and a little before) had occasion to say of our ways of making Experiments, our meaning may not be misconstru'd, we must here Advertise the Reader, that though in many of the following Experiments, the contrivances will not perchance be dislik'd, yet in many others they are far enough from being such as might have been propos'd by one, that had wanted no Accommodations fit for such a work as ours. But I was reduc'd to make many of those Experiments in a Village, and whilst I was writing them, was obliged to make frequent Removes, by which means I seldom had the liberty to make my Trials after such a manner, as I could contrive them, if I could have Instruments and other assistances to my wish.<sup>†\*</sup> For sometimes / I wanted conveniently shap'd Glasses, sometimes the Implements necessary to seal them up with, sometimes such ingredients as I needed to work on, oftentimes frosty Weather, for the freezing of Bodies to be expos'd to the open Air, and not seldom Ice and Snow for Artificial congelations; sometimes Weather-glasses, especially seal'd ones, two of which being unluckily broken after one another, kept me from being able to make divers considerable Experiments; sometimes tender Scales, and sometimes other Mechanical Instruments of several sorts, and more then sometimes (for it happened very frequently) I wanted time so to prosecute and finish the Experiments, as to satisfie my self about divers circumstances, which, though possibly few Readers will take notice to be wanting; I would gladly have observ'd, if I had not been hindered, not only by the haste I was often fain to make for fear of losing a frost, but the importunity both of other Avocations, and even of the distraction given me by the multitude of Experiments,

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which haste made me prosecute at once. And indeed, as in divers others of the Treatises, I have occasionally written, so particularly in a / great part of this History of Cold, my writing in places, where I wanted such Mechanical Accommodations, as I could have wished for, and devis'd, has reduced me oftentimes both to leave Experiments untri'd, that would have much illustrated my subject, or clear'd the difficulties of it, and contrive several of those I mention, not after the best manner that might be, but after the best manner, that was practicable by the accommodations I was

then able to procure: so that it need not be wondered at, or blamed, if in some passages of these Papers, Experiments to the self same purpose are more accurately tri'd, or by more Expedient ways at one time then another. For as a Physician, if he come to practise in the Country, where Apothecaries shops are but ill furnished, both as to the Number and as to the Quality of the Drugs, must accommodate his Practise to the scant *Materia Medica*, of which alone he has the command: So when I write of Experimental matters, in places where I cannot have Workmen, nor Instruments fit for my turn, I must be content to vary my Experiments accordingly, and sute them to the accommodations I am confined to, which, though / it be an unwelcome Condition, is made the less so to me, by a Hope, that the Equitable Readers will think it be all that a man is bound to do in such cases, to procure the best assistances he can, and manage those, he is able to procure, to the best Advantage.

And this I the rather take notice of on this occasion, that ingenious men might not be too much discouraged by imagining, that, because they live in the Country, or upon other scores cannot furnish themselves with the best Instruments and accommodations, nor injoy the assistance of the skilfullest Artificers; they are either Unqualifi'd for the making of Experiments and Observations, or Superseded from it. For though in some cases, where the *measures* of things must be nicely determin'd, and principally in Observations, whereon either *Theorems* or *Hypotheses* about the *Proportions* of things are to be grounded; very good Instruments are exceeding useful, and sometimes necessary: yet there are thousands of particulars, whose knowledge may be instructive to those, that Are or Would be Naturalists, where no such Nicety is requisite, and where the measuring / things by *Ounces* and *Inches* will serve the turn, without determining them to *Lines*, and to *Grains*. And even in cases, where Exact observations are (to some purposes) Requisite, those that are not so, may be oftentimes very Useful, by affording Hints, by which others may be excited and assisted to make those more accurate Trials. And here let me take notice, that a Tool or Instrument is not therefore to be despised, if it be Proper enough to the Particular use to which 'tis appli'd, because some more Mechanical head or hand, may propose or make another, that is more Artificially contriv'd, or more Neat and Portable, or that will also perform Other things, then that we are speaking of. For there is a vast multitude of Physical *Phænomena*, wherein Mathematical exactness is not necessary, and Observations about these, may be well enough made by divers Other ways, then by the most Artificial, that Can be devis'd. As though a fine

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Watch may have these Advantages, *That* it is a neater thing, and more portable then an ordinary Clock, *that* it may be improv'd by the Addition of an Alarum, and *that* it may also perhaps shew the Day of the / Month, the Age of the Moon, the Tides, and divers other things, of which the Clock shews not any; yet an Ordinary Clock may serve to measure an hour by, as well as this finer Engine; and so may a Sun-Dyal, and many other Instruments on divers occasions, though in other Cases, and other Regards, they be far less commodious, then either a Watch or a Clock. Besides, that in many cases a skilful Naturalist will by a variety and collation of Experiments, make the same discoveries, and perform the same things, for which others are wont to be beholding to Instruments, and perhaps do many things without them, that have never been done with them. And since *Necessity* is proverbially allow'd to be *the Mother of Inventions*, even in Tradesmen, and Vulgar heads, why should we doubt, but that the rich and inventive Intellect of a Philosopher, may in cases of necessity turn it self, and contrive the things it can dispose of, into so many differing forms, that it will often make its own Sagacity and Industry supply the want of exact Tools and Instruments. And these Considerations that tend to keep ingenious Men from Dispondency, I therefore think fit to Inculcate, / because the Common-wealth of Learning would lose too many useful Observations and Experiments, and the History of Nature would make too slow a Progress, if it were presum'd, that none but Geometers and Mechanicians should imploy themselves about writing any part of that History.

But to return to those Trials of our own that occasioned this (as I hope, Seasonable) Digression, I



was about to add, That as the acknowledgement I was making, that some of the Trials were for want of Accommodations less Artificial then I could have design'd or wish'd them, touches not all, nor haply the greatest part of the following Experiments; so it need not derogate from the Readers reliance on those which it does concern. For though some of them might have been more Artificially *performed* to the manner, yet they could not have been more Faithfully *registred*, as to the Events. Which though I dare promise my self, that most Readers will be induc'd to believe, upon the Considerations not long since intimated; Yet I think it requisite to give this intimation on this occasion, because, that though I have elsewhere<sup>†@</sup> largely manifested to / what contingencies divers Experiments are liable, yet I have found very few, whose events are so subject to be varied, by slight and not easily heeded circumstances, as several Experiments concerning Cold: Where oftentimes the degree of that Quality, *or* the time during which it continues appli'd, *or* the manner of Application, *or* the thickness, shape and bulk, &c. of the vessels that contained the matter expos'd to it, may have a far greater influence on the success, then those, that have not tri'd, can easily imagine. And it increases the difficulty, that these Experiments of ours being (very few excepted) the only that are yet made publick concerning Cold; we

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cannot so easily, as in other cases, free our selves from the doubts, that may be suggested by different events, by comparing together several Experiments of the same kind, though to obviate this inconvenience, as far as I may, I have divers times in cases, where the Experiments seem'd like to be thought strange, or to be distrusted, set down several Trials of the same thing, that they might mutually support and confirm one another.

Of those Contingent Experiments / about Cold, I was newly speaking of, the Reader may meet with an eminent Example in the 21. Title, where mention is made of the differing Effects of Air blown out of a pair Bellows upon a Weather-glass: and as for the suspition I there conclude with, (though I yet doubt, whether it will reach All the Cases incident to that Experiment) I have since been confirm'd in it, by finding, that by purposely varying the temper of the Bellows themselves, I could divers times considerably vary the operations, which the Winds, blown out of them in their differing states, had upon the Liquor in the Weather-glass.<sup>†@</sup> Of this I expect to have an opportunity of saying more, and therefore shall at present add but this one particular, which may sufficiently justifie me for having said, That Weather-glasses & / our Sensories may give very differing Informations about the Temperature of the Air turn'd into Wind, by being blown out of the same pair of Bellows. For having taken two Hermetically seal'd Weather-glasses furnished with highly rectified spirit of Wine, and purposely made for my Experiments, by a person eminently dexterous in making such Instruments, and having likewise provided a large pair of Bellows, I found, that by blowing 20. blasts at a time on the Ball of one of them, though the Pipe were not only slender, but of an unusual length, amounting to about 30. Inches, yet the Liquor did not sensibly subside any more then rise. And in the other Weather-glass, whose Pipe was less long, but whose Ball was purposely made far greater to be the fitter for short and nice Experiments, we found more then once, and (that as well in the cold Air, as in a close Room) that the wind that was blown in divers blasts out of the Bellows, against the lower part of the Instrument, did not only make the spirit of Wine subside, but did make it manifestly, though but very little, ascend. And 'tis not necessary, for the making good of what I taught, that such Trials should always / succeed just as these did, since it may suffice to prove what I pretended, that a good seal'd Weather-glass did divers times discover the Wind to be rather warm, then cold, when upon Trial (then purposely made) it felt not only manifestly, but considerably cold, both

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to a By-standers Hand, and to my own Hand and Face, though my hand, that was blown upon, were immediately before more then ordinarily cold.

And I shall here add, That judging it fit to make further Trial, with an unseal'd Weather-glass, I made one, that was in some regards preferable to those mentioned in the second Præliminary Discourse, by making the Bubble large, and the Cylindrical Pipe so proportion'd to it, that instead of a Drop of water, a Pillar about an Inch long of that Liquor was kept suspended, and play'd as well conspicuously as nimbly up and down in the Pipe. And having fastned this Instrument in an erected Posture, with the Sphærical part uppermost, to the inside of a Window, by blowing upon the Ball with the Bellows above mentioned, which had lain some hours not very far from the Chimney-corner, (but without seeming to / be sensibly warm'd by the neighbourhood of the fire) a very few blasts made the suspended water hastily subside, (and thereby witness the Expansion, and so the warmth of the included Air) and upon my ceasing to blow, the same water would reascend in the Pipe, and that, though I stood near it to watch it, (which shows, that the former Depression was not caused by the approach of my warm Body) and this I did more then once, both alone and before witness, notwithstanding that the Air blown at the same time out of the same Bellows upon our hand and face seem'd cool enough. But fearing to insist any longer on this matter in a *Preface*, I think it now unseasonable to add, That as some contingent Experiments in subsequent Trials may Fail oftner, so other may perchance Succeed oftner then is expected: As I have sometimes observed in the figures, that appear in the Ice made of some Liquors, that abound with Volatile, Urinous, or with with certain other Salts. But to say a word in general of Experiments, whose success is not always uniform: As a *Magnetick Needle*, though it do not always precisely respect the *Poles*, but both declines sometimes *Eastward*, and sometimes/ *Westward*, and varies that Declination uncertainly as to us, does nevertheless so far respect the North, as in spite of its Variations to be an Excellent guide to Navigators: So there are contingent Experiments, whose Events, though they sometimes vary, are seldom very exorbitant, but for the most part are regular enough to afford Philosophers very useful Informations and Directions.

If it be demanded, why in the *15*, *18*, and *19*. Sections I have inserted so many Quotations out of several Authors, and how that agrees with what I have said not far from the beginning of this Preface of the uncultivatedness of the subject I have adventured on? I answer, That what I have done crosses not what I have said. For my complaint was, That there has been very little, especially of any moment, delivered concerning Cold by Classick Authors, and that even other learned Writers, who have had occasion to say something purposely of Cold, have handled it exceeding Jejune; but this hinders not, but that if a Man will take the Pains to seek out, and enquire of Travellers, and has the curiosity and opportunity to consult Voyages and Navigations, / he may among a multitude of other things, that have nothing to do

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with Cold, meet with some few that concern that subject: And yet the Authors that deliver such particulars, can no more properly be said to have written professedly of Cold, then of Botanicks, or Zoology, or Meteors, or Civil Philosophy, because in the same Journal they mention a great frost, or a great snow, as chancing to happen on such a day, with as little particular design as they mention a Storm, or a Whale, or a Bear, or the manners of an *Indian* people. This Consideration being premis'd, 'twill not be difficult to return an Answer to the former part of the Question lately propos'd. For the unfrequency of my Quotations in most of the Sections of the following History, will, I presume, sufficiently perswade the Reader, that I would not needlessly imploy so many of them in the three Sections, that are nam'd in the Question. But the Writers of Physicks being, for ought I know, silent as to the particulars I have transcrib'd out of other Writers, and the Observations being such as I could not my self make in this Temperate Climate; I must either make / use of other mens Testimonies, or leave some of the Remarkablest *Phænomena* of Cold unmention'd. And they that shall try how much pains it will cost them, to range among Books, which many of them contain little but melancholly Accounts of storms and distresses, and Ice, and Bears, and Foxes, to cull out here and there a passage fit to make a part of such a Collection as they may here meet with, will possibly rather thank, then blame me for having, to gratifie my Readers, given my self so laborious and unpleasant an entertainment. And I was the rather content to enlarge

a little on the foremention'd occasions, not only because I was unwilling to be engag'd more then once in so troublesome an Employment, but (and that chiefly) because most of the particulars, I have collected out of Navigators, are afforded me by the Voyages of our own Country-men, who having written only in English, an Account of what their Relations contain of most material concerning Cold, will probably be welcome, as well as new to the curious of other Countries, who cannot understand their Books, divers of which having been / long out of Print, are so hard to be procur'd, even here in *England*, that I doubt not but these Extracts of them will be acceptable, even to divers of the *Virtuosi* of our own Nation, especially since I have been careful to alledge most of the Testimonies in the writers own words, though they are not always the best, wherein the things he delivers might be express'd. And this course I the rather took, that I might do what I think very useful to be done by all writers of Natural History, who would do well to distinguish more carefully, then hitherto many have done bewtixt the matters of fact, they deliver as upon their own knowledge, and those which they have but upon trust from others. I know it would be more acceptable to most Readers, if I were less punctual and scrupulous in my Quotations; it being by many accounted a more Gentile and Masterly way of writing, to cite others but seldom, and then to name only the Authors, or mention what they say in the words of Him, that Cites, not Theirs, that are Cited. And there are some Writers of such known Diligence and Veracity, as to be safely trusted, and some Cases / wherein I do not dislike, but comply with this

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Custom (after having first consulted my Author to be Master of his true and genuine sense.) but in matters Historical, and whereon Philosophical and Important Truths are to be built, I should think my self beholden to a Writer, for setting them down in such a way, as that I may satisfie my self, that the Testimony is faithfully reported.

In order to which it will be sometimes very useful to be enabled to repair to the Original Witness, and, if need be, survey there the context of the alledged passage. For I must here advertise the Reader, that in matters of any moment, 'tis not from every Writer, that I dare trust the Quotations he makes of the passages of other Authors, in his Own words, not Theirs: For upon comparing very many Quotations, I have found, that oftentimes there is no such thing, as is pretended to be really met with in the place referred to; and even when neither the Book, nor Chapter, nor Page are misquoted, I have too frequently found, that the Alledgers of Testimonies, do either through Inadvertency misapprehend, or misrecite the sense of / the Author they quote, or out of Design make him speak, that which may comply with their purpose, whether it were his own sense or no: and by their Indefinite citations make it too troublesome and difficult a work, for the Reader to find out, whether they have imposed upon him or not. But this only by the by, to return therefore to the passages we were speaking of, in the 15, 18, and 19. Sections, I shall now add, that having in the beginning of the XIX. Title of the insuing History itself rendered an Account of my not scrupling to insert some strange Relations concerning Cold, it will not be requisite to mention here, what the Reader will meet with there. And I scarce doubt but he will excuse such passages, if he consider, That as I could not omit them without leaving out some of the eminentest *Phænomena* of Cold, so being unable to examine them here in *England*, all I could do, was, to report them faithfully, and mention only such as were either affirmed by Eye witnesses (as the most, I have inserted, are) or, at least recommended by credible Testimony, whereof we shall say more by and by; To which / sort of Narratives, I know not whether I may refer That, (which yet for its strangeness may deserve a transient mention,) came awhile since to my ears, of an English man, who related to an eminent *Virtuoso* of our acquaintance, That a *Dutch* Master of a Ship, returning from the Northern Countries, very solemnly affirm'd, being therein seconded by one of his Country-men, and offered to produce his Journal for proof, That indeavouring to sail Northwards as far as he could, he came within less then a degree of the Pole itself, and found the Sea open, and the cold very tolerable.†\* But to return to what we were saying, before this odd Relation diverted us, I did not only decline the mention of divers things, with which I fear many Writers would have adorned a History of Cold, but even of those that I my self have inserted, I would have left out divers, were it not, that many of

the Relations, that may appear so wonderful, seem not to me to be repugnant to the nature of things, but only suppose a far greater degree of Cold, then we have in these parts,

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and yet the familiar effects of the Cold we have here, / would perhaps be looked on as incredible, by one that were born and bred in the Kingdom of *Congo*, where *Odoardus Lopes*,<sup>†\*</sup> who lived long there, informs us, that Ice, that is *water made solid*, is so unknown a rarity, that it would there be valued as much as so much Gold. And a Learned Physician, that lived in *Jamaica*,<sup>†\*</sup> being asked how far he found the Temperature of that Country to be like that of *Congo*, answered me, That in that Island he observed not all the Winter long, either Frost or Snow: And yet here it will not be unseasonable to say a word or two of the three Principal Authors, from whom most of our strange relations we are considering are transcribed.

The first is *Gerat de Veer*, who writ the Voyage of the *Hollanders* to *Nova Zembla*, a Book so eminent in its kind, that it may seem a wasting of time to set down a Character of it; and therefore I shall only advertise the Reader of two things, the one, That the *Dutch* did indeed make three consecutive Voyages to *Nova Zembla*, but that the / *third* being that in which they wintered, there most of the particulars are to be understood of that. The other thing is, that having lost the Translation that was made of those Voyages out of *Dutch* into *English* (published in a Book by themselves) without being able to procure another, I was obliged to have the citations transcribed, as I found them extant in that faithful Collection of Voyages compiled by *Purchas*, who seems by the Style to have (as to the Book we are speaking of) only plai'd the part of an Interpreter. And here 'twill be seasonable to add, that whereas that excellent Collection consists of several distinct Tomes or Volumes, the many Quotations to be met with in the Margent of our History under the name of *Purchas*, are to be understood, (unless the contrary be intimated) to belong to the third Part of his Pilgrim, where the *Dutch* and other Voyages into Northern Countries are to be found.<sup>†\*</sup>

The next Book I intended to mention, is *Olaus Magnus*'s History of the Northern Nations. And though this Author / is of very suspected Credit, and delivers some things upon hear-say, which, they are kinder to him then I, that are pleas'd to beleive; for which reason I do but very sparingly make use of his History, yet considering, that he was Archbishop of *Upsale* in *Sweden*, and appears to have more Learning, then many that never read his Books, imagine; I thought I might Now and Then, make use of his Testimony, in matters wherein he either professes himself to speak upon his own knowledge, or delivers but such things as being consistent with the Laws of Nature appear Improbable, only, because of the Intense Cold that they suppose; which I the rather say, because he himself somewhere speaking of the Cold,

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that by the Laws of Nature reigns in the North, subjoyns this Passage;<sup>†@</sup> *Sub quo quia natus, & versatus sum etiam circa elevationem graduum Poli Arctici 86. arbitror me posse hoc, & multis sequentibus Capitulis, nonnihil Cæteris vaga opinione scribentibus clarius demonstrare, quam vehemens & horrendum sit illic frigus.*

And, though perchance few Readers will perceive it, I have been so Severe in rejecting not only Relations, but even Authors otherwise not Obscure, that, how much soever I foresaw my scrupulousness might impoverish my History, yet there are some whole Treatises about Cold Countries, whence I have shunn'd to borrow any one Authority, because I perceiv'd the Authors had not observed the things they recount themselves, and were too easie in believing others.

The third Writer I meant to take notice of, is Captain *James*, a Person from whose Journal I have borrowed more observations, then from those of any other Sea-man, not only because his Book supplied me with them, and because it is somewhat scarce, and not to be met with in *Purchas*'s Tomes, (having been written some years after they were finished) but because this Gentleman was



much commended to me, both by some Friends of mine, who were well acquainted with him, and by the Esteem that / competent Judges appear to have made of him.<sup>†\*</sup> For having been, not only employed by the Inquisitive Merchants of *Bristol*, to discover a *Northwest passage* into the *South Sea*, but designed for so difficult a work by so judicious a Prince, as the late King, and having at his return published his Voyages by his Majesties command; as by these circumstances, though not by these only, this Gentlemans Relations may well be represented to us, as likely to deserve our consideration and Credit: So by his breeding in the University, and his acquaintance with the Mathematicks, he was enabled to make far better use then an ordinary Sea-man would have done, of the opportunity he had to observe the *Phænomena* of Cold, by being forced to Winter, in a place where he endured little (if at all) less extremity of Cold, then that of *Nova Zembla*.

I presume 'twill easily be taken notice of, That in the following History I have declined the Asserting of any particular *Hypothesis*, concerning the Adequate / cause of Cold. Not but that I may have long had Conjectures about that matter, as well as other men, but I was willing to reserve to my self an Intire Liberty of declaring what Opinion I most inclined to, till the Historical part being finished, I may have the better opportunity to Survey and Compare the *Phænomena*; and the leisure, (which I cannot promise my self in haste,) of calmly considering what Theory may best agree with them: especially since I freely acknowledge, That I found the framing of an Universal and unexceptionable *Hypothesis* of Cold, to be a work of greater difficulty,

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then every Body would imagine; especially to me, to whom some Experiments purposely made have suggested a puzzling Difficulty, which 'tis like that Philosophers have not yet thought of. And whatever Applause is wont in this Age to attend a forwardness to assert *Hypotheses*, yet though *Fame* were less to be sought then *Truth*; this will not much move me, whilst I observe, That *Hypotheses* hastily pitch'd upon, do seldom keep their Reputation long; and divers of / them that are highly Applauded at the first, come after a while to be Forsaken, even by those that devised them.

As for the Title of the following Book, I call the Experiments *new*, because indeed, if I mistake not, nine parts of ten (not to say nineteen of twenty) *are* so. But though a *150*. or *200*. Experiments of that kind, besides Collections from Travellers, and Books that do not professedly treat of Cold, may, I presume, allow me to have *begun* the natural History of Cold; yet in the very Title Page I think fit to intimate, that I look upon what I have done but as a *Beginning*. For though some very noted *Virtuosi* have been pleased to seem surprized, to see what so barren and uncultivated a subject has been brought to afford this Treatise; yet I look upon these as things, that do rather Promise then Present a Harvest, and but as some early Sheaves of that Crop, which mens future Industry will reap from a subject, that is indeed Barren, but not Unimprovable. For I see not why it should not hold in the / History of Cold, as well as in many other attempts; That the greatest Difficulties are wont to be met with at the Beginning, and those being once surmounted, the Progress becomes far more Easie. And as the Magnetick Needle, though it point directly but at the North and South, does yet discover to the Sea-man the East and West, and all the other points of the Compass: So there are divers Experiments, which though they do primarily and Directly teach us but a Notion or two, may yet assist us to discover with ease many other Truths, to which they seem'd not at first sight to afford us a Direction. So that What is here already done, such as it is, partly by Hinting various Inquiries about Cold, and partly by Suggesting ways not formerly practised of making further Experiments, may possibly make it more easie for others to Add to these a number, far exceeding that, which they will here meet with, then it would have been without such assistances, (which I had not) to contribute to the *History of Cold*, even such a stock as I have / begun it with. And this I the rather incline to think, because I find, that when once a Man is in the right way of making Inquiries into such subjects, Experiments and Notions will reciprocally direct to one another, and suggest so many things to him, that if I were now to begin this work again, and had Cold, and fitly shap'd Glasses, and Instruments, with other Accommodations at command, there are divers parts, on which my Inlargements would not perchance be much Inferiour to what is Already extant there, if they did not

much Exceed it. But besides That, I have other work enough, and that of a quite other Nature upon my hands; the Truth is, that I am plainly Tired with writing on this subject, having never handled any part of Natural Philosophy, that was so Troublesome, and full of Hardships, as this has proved; especially because, that not

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only the Experiments being *New*, and many of them subject to miscarriages, required to be almost constantly Watched, but being unable to *produce* or *intend* Cold as we can do Heat, nor *command* / the Experiments that concern Congelation, with as little difficulty, as we can do those, that belong to divers other subjects; I was fain to Wait for, and make Use of a Fit of frosty weather (which has very long been a rarity) as sollicitously as Pilots watch for, and improve a Wind.

*III.* It remains now, that I give some account, why I suffer so Unfinished a piece, as I acknowledge this to be, to come forth at this time. And I confess, that if I had not preferred the gratifying the Curious, before the advantages of my own Reputation, I should have kept this Book in my hands some Winters longer, that It might come forth, both more rich, and less unpolished. But how great a power my Friends have with me in such cases, the Reader may easily guess by the Preamble he will find prefixed to the first Title of the ensuing History. For by the Date of that, he will see, how early my Papers about Cold were to have been / communicated; nor was I any thing near so much befriended, as I expected, by those interposing Accidents, that have for above a year and a half, kept those Papers lying by me. For the *then next*, and *now last* Winter proved so strangely Mild, as to be altogether Unfavourable to such a work as I had design'd. Wherefore finding, that Delays had done me no more service, and press'd by the sollicitations of divers *Virtuosi* from several parts, I resolved, that I would suspend till another opportunity, the drawing together of what I had Observed or Collected, touching the Regions of the Air, and some of the chief *Hypotheses*, that are controverted about Cold, with what other loose Papers, touching that Quality, I not could so readily dispatch to the Press; and would not with-hold from the Curious what assistance my Collections could afford them, to make use of this Winter to prosecute Experiments of Cold. And remembring how favorable an entertainment my former Endeavours to gratifie Ingenious Men, had found among them, I took a Course, / wherein I was more likely to obtain Thanks than Praises, and chose rather to adventure on the Equity and Favour of the Reader, for the Pardon of those faults and Imperfections, that are imputable to Hast, then to deny him the opportunity of this Cold season, wherein to Examine the Truth, and Supply the Deficiencies of what I had delivered. And this I the rather did, both because I was desirous to Quit this subject, for another from which it had diverted me, and for which I have more Value and Kindness; and because, that as a tender Constitution of Body kept me, whilst I was writing the following History, from adventuring upon some Trials, that might (probably) have enrich'd it; so the Continuance of the same disadvantages, together with other inopportune Distempers superadded to them, do not permit me to Know, *whether*, and *how far* I shall be able to Prosecute the work I have begun: and do oftentimes reduce me to be more concern'd to Shun the Effects of Cold, then Observe the *Phænomena* of It. And indeed, whether those prove true Prophets or no, that assure me I / shall lose no reputation by this History (as incompleat as it comes forth) I think, if

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Ingenious Men knew, how much Trouble and Exercise of my patience it has cost me, they would, peradventure, vouchsafe me some of their thanks, if not for what I have *done*, yet for what I have *suffer'd* for their sakes, (and would scarce have undergone upon any Inferior account whatsoever;) it being, though a less Noble, yet no less Troublesome an Employment, to Dig in Mines of Copper, then in those of Gold: and Men being oftentimes obliged to Suffer as much Wet and Cold, and Dive as deep, to fetch up Sponges, as to fetch up Pearls. /

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Page 5. line 17 [in fact 27] read, *that in not nice*, for, *that even in nice*, p. 46. l. 8. r. *effected* [for *affected*], p. 48. l. 16. dele *and*, together with ( [after *water*,], p. 82. l. 28. r. 28. *chapter* [for 27. *Chapter*], p.178. l. 7. dele *which* [after *water*,], p. 266. l. 22. r. *it did rise four inches* [for *it did rise 4 inches and 2/9*], p.292. l. 6. r. *that stood on the ice* [i.e., *on* inserted], p. 302. l. 9. r. *three*, for, *thee*, p. 380. l. 10. r. *cemented by intercepted and then frozen water*, instead of *congealed by cold water* [in fact, the 1st edition has *cemented together by cold water*: see below, p. 351], p. 488. l. 11. r. 52. degr. 52. min. [for 51. 52.]

*In the Appendix of Dr. Merret, pag. 35. lin. 36 [in fact 26]. read, upon these mixtures, not in.*

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Figure 1. *Page* 9, 10, 11 & 98.

A the Ball or Egg.

B C the Stem.

D the little Aqueous Cylinder.

Figure 2. the open Weather-glass mentioned *pag.* 24, & 43.

Figure 3. the seal'd Weather-glass or Thermoscope mentioned *pag.* 24, 55, 56.

Figure 4. the Barometer or Mercurial Standard placed in a Frame BB mentioned *pag.* 25.

Figure 5. an Instrument mentioned *pag.* 93.

A the Bolt-head.

B the small Stem.

B C the Cylinder of water inclos'd.

## New Thermometrical Experiments And Thoughts.

### The I. Discourse, Proposing the I. Paradox,

Viz.

*That not only our Senses, but common Weather-glasses,  
may mis-inform us about Cold.*

It may to most men appear a work of needless Curiosity, or superfluous diligence, to examine sollicitously, by what Criterion or way of estimate the Coldness of Bodies, and the degrees of it are to be judg'd, Since Coldness / being a Tactile Quality, it seems impertinent to seek for any other judges of It then the Organs of that sense, whose proper object it is. And accordingly, those great Philosophers, *Democritus*, *Epicurus*, *Aristotle*, (and till of late) all others both Ancient and Modern seem to have contented themselves in the matter with the Reports of their Sensories.<sup>†\*</sup>

But this notwithstanding, since we can scarce imploy too much care and diligence in the Examining of those Touchstones, which we are to Examine many other things by, perhaps it will be neither unseasonable nor useless to premise something touching this Subject.

For though it be true, that Cold in its primary and most Obvious Notion be a thing relative to our Organs of Feeling, yet since it has also notable Operations on divers other Bodies besides ours; And since some of them seem more sensible of its changes, and others are less uncertainly affected by them, it would be Expedient to take in the Effects of Cold upon other Bodies, in the Estimates / we make of the degrees of it.

And to make this appear the more reasonable, I shall not scruple to propose the following *Paradox*, namely, That our Sensories either alone, or assisted by Common Weather-glasses are not too confidently to be relied on in the judging of the degrees of Cold.

To make this *Paradox* Plausible (which is almost as much as I here pretend

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to) I shall represent in the first place, that the account, upon which we are wont to Judge a Body to be Cold, seems to be, that we feel its particles less vehemently agitated then those of our Fingers of other parts of the Organ of Touching. And consequently, if the temper of that Organ be chang'd, the Object will appear more or less Cold to us, though it self continue of one and the same Temper.

This may be exemplified by what has been observ'd by those that frequent Baths, where the milder degrees of heat, that are us'd to prepare those that come in for the higher, seem very great to them that coming / out of the cold Air dispose themselves to go into the Hot Baths, but are thought cold and chilling to the same persons when they return thither out of much warmer places; which need not be wondred at, since those, that come out of the cold Air, find that of the moderately warm Room more agitated, then the cold Ambient would suffer the External Parts of their Bodies to be, whereas the same warm Air, having yet a less agitation then that in which the hotter parts of the Bath had put the sensitive parts of the Bathers Bodies, must seem cold and chilling to Them.

But 'tis not only in such cases as this, wherein Men can scarce avoid taking notice of a manifest change in themselves, that these mistaken Reports of our senses may have place. For oft-times we

are impos'd upon by more secret changes in the disposition of our Sensories, when there needs something of attention and of Reasoning, if not of Philosophy to make us aware of them. For being apt to take it for granted that our Temper is the same, when there is no very manifest / cause why it should be chang'd, we often impute that to Objects, whereof the Cause is in our selves; and if this change in our selves be wrought by unsuspected Agents, or by insensible degrees, we do not easily take notice of it. Thus though in Summer divers Cellars, that are not deep, are perhaps no colder then the External Air was, (when it was judg'd but Temperate) in Winter or the Spring; yet it will seem very Cold to us that bring into it Bodies heated by the Summer Sun, and accustomed to a warmer Air; nay cold does so much depend upon the degree of Agitation in the parts of the Object in reference to the Sentient, that even when we may think the Sensory unalter'd, it may judge an Object to have a degree of Coldness which indeed it hath not; as I remember, that to satisfie some Friends, that 'tis not every Wind which feels cold to us, that is really more Cold, then the still Air, I have sometimes shewen, that in not nice Weather-glasses Air blown out of a pair of Bellows does not appear to have acquired / any Coldness by being turn'd into Wind, though if it were blown against the hands or face, it would produce a new and manifest sense of Cold; of which the reason seems to be, That though the Organ in general seems not to be alter'd, yet the Wind by reason of its Motion, is able not only to drive away the parts of the Air contiguous to the hand, or face, and the warm steams of the Body which temper'd its Coldness; but to pierce deeper then the calm Air is wont to do into the pores of the skin, where by comparison to the more inward and hotter parts of the Sensory it must needs appear less agitated and consequently colder.

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Besides, that sometimes we may meet with certain Steams in the Air that have in reference to the Blood and Spirits of humane Bodies (though not perhaps to divers other Liquors) a certain hidden power of chilling, as *Opium*, ev'n in outward applications (for in such ways I have known a great Chirurgeon much use it and highly extol it) strikes a Coldness into the Body by the subtile *Effluvium's* / that insinuate themselves at the pores of the Skin; and perhaps too, that Coldness is ascrib'd to External Bodies, which is produc'd in us by some Frigorifick Vapour, or other distemper, which being too slight to be taken notice of as a disease, may yet be of Kin to those Agents, that produce what Physicians call horrors and Rigors at the beginning of Feavers, and some other distempers; or produce that strange and universal Coldness of the external parts, which is frequently enough observ'd among other Symptomes in Hysterical Women. Moreover, Bodies may often appear colder to us then to a Weather-glass, because our Sensories are more affected by the density and Penetrancy of the parts. This may seem somewhat strange, but being sutable enough to some of my Conjectures about Cold, I have often made Tryals with very nice Weather-glasses, that have assur'd me, that (at least oft-times) when water seems to be cold enough to our touch, it appears not to be colder to the Weather-glass then the Ambient Air. /

These Trials I have sometimes made with seal'd Weather-glasses, but the most with another sort of Weather-glasses (whose structure and use are by and by to be mentioned) which though they seldom prove durable, nor of any great use in any other then such nice and short Experiments, yet they discover slighter changes of the Temper of the Air then would be *notable* (not to say *sensible*) in ordinary Thermometers. But of multitudes of Trials that I sometimes made with these Glasses, I can at present find among my loose Papers but a very few; and though I remember, that in one or two (made about the same time with some of those that follow) I observ'd Things that make me now wish I had had Opportunity to make those further Trials of Them, which some of their *Phænomena* seem to direct the making of: yet I shall annex these that follow as I find them entred, because they are not perhaps destitute of hints improvable by further prosecution.

*June* 26. between two and four in the afternoon (the Weather / moderate for the season) I took a thin white glass-Egge blown at a Lamp about the bigness of a Walnut, with a stem coming out of it about the bigness of a large Pigeons Quill four or five Inches long, and open at the Top;†\* this slender pipe being dipp'd in water, admitted into its Cavity a little Cylinder of Water, of half an Inch

long or somewhat more, which (the Glass being erected) subsided by its own weight, or the Temper of the Air in the Egge (in reference to the outward Air) till it fell to the lower part of the Pipe, where it comes out of the Egge, and thereabout it would rest. Now if taking this Glass by the Top betwixt my Thumb and forefinger, I deprest the Egge under the surface

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of a Bason of fair water (cold enough to the touch) the little Aqueous Cylinder, that parted betwixt the Air in the Egge, and the external, would, instead of being made to subside by the Eggs immersion into the Cold water, presently rise up from the lower part of the Pipe, till it reach'd about the middle of it, though the Glass / were, in this and the following Trials, held erected; and as soon as it was taken out of the Water into the Air, the water would again subside, whether I held the Glass, or let it rest upon the Boards, or a Linen Carpet, that cover'd the Table, on which the Trials were made. And this I did several times as well with as without witness. I tried also that if, instead of water, I made the use of Quicksilver, though not big enough to cover the Egge much above half way, and in the rest proceeded as above; the cold Quicksilver would presently make the Aqueous Cylinder hastily ascend near three Inches, sometimes almost, and sometimes quite to the Top of the slender pipe, whence the water would again quickly subside, when the Glass was taken out into the free Air, or set to rest upright as before.

Besides, having set the vessel of Quicksilver and the Bason of water very near one another, I did at least upon three or four several Trials find, as I expected, that when by immersing the Egge in water, the / pendulous Cylinder was rais'd so high, that it did no longer sensibly ascend, by nimbly taking the Egge out of the water and depressing it in the Quicksilver, it would rise far higher: and I also tri'd, that nimbly removing the Egge out of the Quicksilver into the water, the pendulous Cylinder would subside, after plunging the Egge under water, though not so fast, nor near so low as it would do, in case the Glass were remov'd from the Quicksilver into the Air. Upon another Trial made much about this time, though not the self same day; the pendulous water in the same Glass, (the day being for the most part windy and rainy) did subside upon the immersion of the Glass into water, not only a while before noon, but an hour or two after dinner, and at distant hours afterwards, though the Descent of the Pendulous water was neither so quick, nor so considerable as it had been formerly in the Mornings.

*June 27.* In the morning a small Cylinder of Water pendulous in the / above mentioned Glass, upon the immersion of the Egge in a Bason of water, would immediately and very considerably subside, whereas the same glass, being immersed in the Vessel of Quicksilver formerly mentioned, would presently ascend. Both parts of this Experiment we several times tried, and the Reason was suspected to be, that the Quicksilver had stay'd all night in my Chamber, which was somewhat warm, whereas the water was brought up that morning, and to the touch seem'd colder then the Quicksilver, and a while after dinner, the same water having been still kept in the room, we divers times found, that as well That, as the Quicksilver, did immediately upon immersion impel up the pendulous water in the slender pipe. Another time in frosty weather (and about the beginning of *January*) we did with such a glass (as has been already several times mention'd) take some drops of water out of a vessel, wherein that Liquor had for a good while been kept; that it might be reduc'd as near as we could to the Temperature / of the Ambient

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Air; then suffering the suspended water to continue a convenient while in the long and slender stem of the Weather-glass, that the internal Air might be reduc'd to the temper of the external, we took up the Glass by the open end; and immersing the obtuse part of It into a shallow Vessel containing some of the above mentioned Water, we found the suspended drop suddenly impell'd upwards about half an Inch or more, and the Ball of the Thermometer being taken out of the Water into the Air, the pendulous drop did again (though far more slowly then it ascended) subside. This was repeated



three or four times with some intervals between (and that in a Room where there was no Chimney) and still with the like success, save that in the two last Trials we took the Weather-glass out of the shallow water, and plunging it into a deep vessel of the same water (that stood very near the other) we found (for further confirmation of the Experiment) that the pendulous water was upon these / new immersions, impell'd up, near (if not full) as high again, as when we had immers'd it only in the shallow vessel: and taking it out of this deep Glass, we found the Cold of the external Air to reduce It to its former humble station. Thus far the notes, I have yet been able to recover: and though, as I said, I dare not build very much upon them, yet by small seal'd Weather-glasses I find enough to invite me to suspect, that of the degrees of heat and cold in the Air we may receive differing informations, when we imploy only our Organs of Touching, and when we make use of fit Instruments.

I shall add on this occasion, that not only water it self, but moist vapours abounding in the Air, may make Us think it colder then the Weather-glass discovers it to be. For though it be generally taken for granted, that the Thermometer does only more exactly measure or determine the Effects, which cold hath both upon it and upon our Sensories, yet I have long suspected that there is somewhat else in the case. And I / have observ'd, that sometimes the weather seem'd more or less cold to me, then that which preceded, when the contrary appear'd in the Weather-glass; and that, when upon consideration of the whole matter, that difference did not appear to depend upon those circumstances of Exercise or Rest, or the Temperature of the Air I came out of, or any of those other things, to which a considerate man, that goes upon no better then the common opinions about Weather-glasses, would be apt to impute to that *Phænomenon*. And I was the less dispos'd to think my self mistaken, because having purposely enquir'd of others in the same house, who were not told, what Information the Weather-glass gave, they agreed with me in the sense I had of the Temperature of the Weather. And having since, as occasion serv'd, communicated my Observations and suspicions to divers Ingenious Men, I have been by their recenter Observations confirm'd that what I have taken notice of, was not the Effect of any *Ἰδιοσυγκρασία*.<sup>†\*</sup> / From which, and other particulars, that we may have elsewhere opportunity to mention, we may plausibly enough infer, that it were not amiss, not only to take notice, when we have opportunity, of the sense, that is Express'd of

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the degrees of Cold by Birds and other animals, whose diet is more simple and regular then ours, and whose perceptions are commonly more delicate and less diverted; but, especially, to examine the coldness of the Air and other Bodies as well by Experiments and instruments, as by the touch. And on this Occasion I must not premit that memorable Account, that is given us by *Martinius* in that Noble piece of Geography which he calls *Atlas Chinensis*, where speaking of the Air of that populous Countrey he has this singular passage:<sup>†@</sup> *Ad Cæli* (says he) *solique temperiem quod attinet, majus in hac provincia frigus est, quàm illius poscat poli altitudo: vix enim illa excedit gradum secundum supra quadragesimum; & tamen per integros quatuor sæpe menses flumina omnia adeò durè concrescunt gelu, ut currus equosque / ac gravissima etiam onera glacies ferat, innoxie ac securissimè transeant: ex iis ingentia etiam glaciei frusta exscinduntur, quæ in futuram æstatem ad delicias servant. His mensibus omnes naves ita in ipsâ glacie defixæ sunt, ut progredi nequeant ubicunque illas frigus occupat (quod certo certius circa medium Novembris ingruere solet) per quatuor illos menses immotæ ibi perstare coguntur, neque enim resolvitur glacies ante Martii initium; hæc plerumque glaciei concretio uno fit die, cum non nisi pluribus fiat liquefactio;* to which he adds what makes most to our present purpose, *omnino illud mirum, tantum non videri aut sentiri illud frigus ut Europeos ad hypocausta subeunda videatur posse cogere, aut in Europâ ad glaciem producendam sufficere unde ad subterraneas illic exhalationes pro harum rerum causis indagandis omnino recurrendum est, &c.*

But all that I have been implying of the Necessity and Usefulness of the Weather-glass, is no way inconsistent with the truth of the latter part of our formerly propos'd paradox, namely that we are

not rashly to rely upon the Informations even of common / Weather-glasses themselves. For though they be an excellent Invention, and their Informations in many Cases preferable to those of our senses, because those Dead Engins are not in such cases obnoxious to the same Causes of uncertainty with our Living Bodies, yet I fear they have too much ascribed to them, when they are look'd upon as such exact Instruments to measure heat and cold by, that we neither can have nor need desire any better. For, not yet to mention some inconveniences in the contrivance of them, which makes them unapplicable to some purposes, and less proper in others, then Thermoscopes *might* be made, even in divers cases, wherein they are presum'd to be unexceptionable, their Reports are not to me, I confess, quite exempt from suspicion. For in ordinary Weather-glasses

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some part of the Liquor being contiguous to the External Air, it is subject to be impell'd more or less upwards, not only according as heat or cold affects the included Air, but according as the incumbent Air happens to be heavier or / lighter. And though this be a thing not taken Notice of by those that have treated of Weather-glasses, yet after what we have elsewhere manifested concerning the weight and spring of the Air, and what we have probably conjectur'd concerning the varying height of the *Mercurial* Cylinder in the Torrecellian Experiment;<sup>†@</sup> I see not why It should not much call in Question the Informations we receive from common Weather-glasses in those cases, where the height or weight of the Atmosphærical pillar, that presses upon the Water in the Weather-glass, is considerably longer or shorter, lighter or heavier then is usual.

For besides the reason of the thing, we have Experience on our side. I might mention on this Occasion an Experiment I thought on, and also attempted last winter to show ev'n upon a Ballance the varying gravity of the Atmosphære in one and the same place, by hanging a small Metalline weight at one End of a pair of Scales so strangely exact, that they would turn with far less then the 500. / part of a grain; and counterpoising it at the other end with a Hermetically seal'd Glass Bubble, which being blown as large and as thin as could possibly be procur'd of so small a weight, might by its great disproportion in Bulk to the Metalline Body lose more of its weight then That would upon the Ambient Airs growing more heavy. But the particular Account of this Attempt belonging to another place, the trial ought not to be more then hinted here, especially since it may suffice for our present purpose to alledge that having found (as we have already in other papers noted<sup>†@</sup>) that in a Weather-glass, where the Water is not fenc'd from the External Air, the weight of the Atmosphære may make it alter considerably between the Top and Bottom even of a Church or Steeple, though it appear'd by more certain Thermoscopes, that 'twas not the differing Temperature of the Air as to Cold and Heat, but the differing gravity of the Atmosphære, which being shorter and lighter at the Top press'd less forcibly upon the subjacent / Water and the included Air, as is more fully made out in the Treatise above related to. And having by the intervention of a Learned Acquaintance desir'd to have some Experiments made of the Effect of the Air upon Weather-glasses in deep Pits or Mines, where the Atmosphærical Cylinder is longer and heavier, I reciev'd Information that an Ingenious Physician,<sup>†@</sup> who had the Opportunity of trying what I desir'd, had found, that in the Bottom of one of those very deep Pits, the water in a common Weather glass rose near three Inches higher then at the top, in a shank or pipe of about thirty Inches long. And this notwithstanding the warmth, that is usual in such deep places, which seems

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not any thing near so plausibly referable to any other cause, as to the increas'd gravity of the Atmosphærical Pillar incumbent on the Water, that Pillar being heavier at the Bottom then at the Mouth of the Pit, by the weight of an aerial Pillar equal in length to the pits perpendicular height or depth. /

But these are not the only Cases wherein the differing gravities of the Atmosphære may, as well as Heat and Cold, have an interest in the rising and falling of the Liquor in Common Weather-glasses.

For though you should not remove them out of one place, and though consequently it may seem that the Atmospherical Pillar, that presses upon the water, must be still of the same length, yet (not to urge, that That may alter, unknown to us) if retaining its length it retain not its gravity, we may be easily impos'd upon, and take that Ascension or Subsidence of the Liquor for the Effect of a higher or remiss degree of Cold, which may either totally or at least in part (and in what part, we are left to guess) be the Effect of the increas'd or lessened weight of the Atmospherical Pillar, happening either by the copious dispersion of Vapours and other heavy Steams through the Air, *or* upon other Occasions not necessary to be here discours'd of, *or* by the Præcipitation of such vapours by rain or into dew, *or* else by the Removal / of the Occasions of the Augmented Gravity or Pressure of the Air. For we have often observ'd great Variations to happen in the height of the Mercurial Cylinder in the Torricellian Experiment upon great rains and fogs, and other sudden and considerable mutations of the Incumbent Air. But since I my self thought fit, notwithstanding the plausible ratiocination, that led me to this Conjecture, to examine it by Experience; I can scarce doubt but that others may have the like Curiosity that I had. And therefore, because it may seem a paradox, it will not be amiss, of many to annex three or four Trials I made to examine the propos'd doctrine, especially ours having been the first observations of this kind, that, for ought we know, have been made by any. And indeed others could scarce have well made such, though they had lighted on the same thoughts, for want of such seal'd Weather-glasses to make them with. To omit then those that I made with a seal'd Weather-glass, and an ordinary one (in which the / water remains suspended beneath the included air) I shall briefly relate, that in a Room unfurnished with a Chimney, I kept two Weather-glasses, which for more exactness sake, I caus'd to be made of a length far greater then ordinary; so that the divisions of the one were half inches, and those of the other not much less, and yet were Numerous. The one of these which was furnished with good spirits of Wine, was seal'd, the other not, but this last I caus'd to be so made of the shape represented by the Scheme,<sup>†\*</sup> that the Air being shut up in the lower part of the Instrument (not as in common Weather-glasses at the Top) the Liquor might as well in this as in the seal'd Weather-glass rise with heat and fall with Cold. In these Thermoscopes (where the Ascension and relapse of the Liquors were, by reason of the length of the Pipes, far more conspicuous then in Vulgar Weather-glasses)

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I observ'd with pleasure, that the Hermetical Thermoscope (if I may for distinction sake so call It, by reason of its being Hermetically / seal'd) did regularly enough descend in cold weather, and ascend in warm: But the other, which was not seal'd, but had a little hole left open at the Top of the Pipe, though, when the Atmosphære continued of the same weight, it would like the other rise with Heat and fall with Cold, yet when the Atmosphæres gravity was alter'd, they would not uniformly move together, but when (as we gather'd from other observations) the Atmosphære grew heavier, the Liquor in the Pipe did not ascend, as high as it would have done, if the Atmosphære had continued in its former degree of gravitation. And on the contrary, when the incumbent Air came to be lighter, the Liquor would rise in the open Weather-glass in a proportion greater then the single increase of heat would have exacted; so that by comparing the two Weather-glasses together, I did usually foretel, whether the Mercury in the Torricellian Tube (which I keep purposely by me in a frame) were risen or fallen, and consequently whether the external Air were / heavier or lighter then before. As on the other side, by looking on the height of the Mercurial Cylinder, I could easily tell before hand, whether the Liquor in the open Weather-glass were higher or lower then that in the Hermetical; the rising or falling of the Mercurial Cylinder one quarter of an Inch (the Temperature of the Air continuing as to heat and cold) usually signifying a great disparity betwixt the Ascension or the falling of the Liquors in the two Instruments.

Among the several notes, I find among my loose papers, and in a Diary I kept for a while of these observations, I shall content my self to transcribe the following two, because, though divers others were made by my Amanuensis, whose care is not to be distrusted, yet by reason of my absence I could not take notice of them my self. The first of these *Memorandums* runs thus:

Last night I took notice, that there was but one or two Divisions difference betwixt the two Thermometers, but upon such a change of / Weather, that happened this day, as made me imagine, that the Atmosphære would be lighter then before, consulting the *Barometer* (if to avoid Circumlocutions I may so call the whole Instrument wherein a Mercurial Cylinder of 29. or 30. Inches is kept suspended after the manner of the Torricellian Experiment.) I found the Quicksilver lower then it had been a great while, and thereupon concluding, there would be a notable disparity, between the seal'd and open Weather-glass, I hastned to them, and found that the latter being much alleviated from the weight of the Incumbent Air, was no less then 17. Divisions higher then the others, and comparing the height the two Instruments were this day at, with an observation I my self made about a week ago, when the Quicksilver was much higher then now it is; I found, that although this afternoon the seal'd Glass being at 41, the other was at 58; yet Then, when the seal'd Weather-glass, was five divisions higher, namely, at 46, the unseal'd Weather-glass / was but at 27. So that betwixt that time and this, the Liquor in the seal'd Weather-glass, has descended five Divisions, but that in the open Weather-glass has ascended 31.

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Thus far the first of the above mentioned Notes; the second is as follows.

The Mercurial Cylinder being higher, then it has been a good while, and yet the Weather warm and Sunshiny, when the Liquor in the seal'd Glass stood very near the 50th division, that in the unseal'd was fallen down as low as the 32.

So that it is very possible, that the unheeded change in the weight of the external Air may have a greater power to compress the included Air in an unseal'd Weather-glass, then a not inconsiderable degree of warmth may have to dilate it, and consequently in an ordinary Weather-glass, where the Air is included at the Top, it may often fall out, that contrary to what men suppose must needs happen, the pendulous Water may rise in warmer weather, and fall in colder. /

And ev'n since the writing of the immediately foregoing part of this page, within a few days that interven'd, I have my self made observations, that do yet more clearly manifest this truth, as may appear by the following notes. The first of which speaks thus.

*Memorandum*, that Yesterday night the Quicksilver being at 29 Inches, the Liquors in the seal'd and unseal'd Weather-glasses, were near about the same Division, the former being at 40, and the other being but half a Division short of that Number. But this night the Quicksilver being risen about  $\frac{1}{4}$  of an Inch; the Liquor in the seal'd is ascended to 45, and the other descended beneath 35 about half a Division, so that there is now 10 Divisions between them.

This is the first Note, to which the following night enabled me to add this other.

The Quicksilver being risen almost  $\frac{3}{4}$  of an Inch above the station it rested at the night before last night, the Hermetical Weather-glass being as it was then above the 40 Divison; the / Liquor in the other, which was open, in two days and nights is fallen to the 17, and consequently is subsided about 23 Divisions, whilst the other is about the same height at which it was at the beginning of that time.

Two or three days after, being returned to the place wherein I had made this last observation, and from which some urgent Occasions had for that time exacted my absence; I found the Disparity, betwixt the two Thermometers that is express'd in the following Memorial.

This day the Quicksilver being risen to 30 Inches, when the Liquor in the seal'd Weather-glass was at about 41 Divisions, that in the other was depress'd a pretty deal below the Ninth Division, so that the difference between the two Thermometers was increas'd since the last Observation from 23 to near 33 Divisions, all which the Liquor in the open Weather-glass had sunk down, whilst that in



the seal'd continued almost at a stand. And the day after this Memorial, I had occasion to / register another, which being the last, I shall here think requisite to take notice of in this Discourse, I shall subjoyn it with that, which immediately preceded in order of Time.

This day the Quicksilver continuing at the same height, at which I

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observ'd it yesterday, but the Weather being grown much colder, the Liquor appears in both the Glasses to have uniformly enough subsided; that in the seal'd Weather-glass, being about the 33, and the other being sunk quite below the lowest mark of all, which was more then I apprehended it would have done, when there was no frost, especially since by my Diary it appears, that one of the last times I observ'd the Hermetical Weather-glass to stand at near about the same height, namely, the 34; the Liquor in the other Glass was no lower then the 41: nor probably would there be now so great a difference, if the Atmosphære had not been this day very heavy; whereas, when this freshly recited observation was made, I find / by the Diary, the Quicksilver to have ascended but to 29 Inches, and a pretty deal less then a half.

Since that time, being forced by several Avocations to be often absent from the place where my Thermoscopes were kept, I was not careful to prosecute such Observations, those already set down (not to mention those that are not here transcrib'd) being judg'd abundantly sufficient to evince the Paradox propos'd to be prov'd by them: Only, to manifest that after I desisted from registering my Observations, the *Phænomena* may probably have been as remarkable as before; I shall add, That one of the last times I chanc'd to take notice of the Difference to be gather'd by comparing the two Weather-glasses, I found (the weather happening to be warmer then ordinary) the difference between them to exceed any that I remembred my self to have then observ'd, amounting to forty four, if not to forty five Divisions. /

And ev'n since the writing of the Last Line, we have had opportunity to observe a *Phænomenon*, which if it had occur'd to us in the place where we might have compar'd the Barascope with the Exact Weather-glasses hitherto mention'd (and whereby we had been invited to rely upon it) would perhaps appear more Considerable then any of the Observations yet recorded. For not very many hours ago, finding in the Morning the Quicksilver to be risen in a good Barascope of mine (though another from that, all this while referred to, and elsewhere kept) above  $\frac{3}{4}$  of an Inch higher then the place it rested at the Night foregoing, and a somewhat Nice Weather-glass (where the included Air is kept in the lower part of the Instrument, which is shaped like that already describ'd in this Discourse) being consulted to show what Effect so great and sudden a change of the Atmosphæres gravity would have upon it; I saw the tinted Liquor in the shank depress'd a full Inch or more beneath the Surface of the Ambient Liquor in the Viol, / which strange depression of the Liquor in a pipe above 20 Inches long, and where the alterations of the Air as to Heat and Cold are not wont to produce any thing near so great an Effect, I could not but take much notice of. Since the season of the year makes it no way likely that the night, though Cold, could have had so powerful an Operation on it, especially since an Amanuensis that watch'd it much longer then I, affirms that he saw the Liquor driven down quite to the very Bottom of the pipe, and a Bubble of the outward Air to make its passage through the water, and to joyn with the Air contain'd in the cavity of the Viol. /

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## The II. Discourse

The II. Discourse,  
Containing some  
*New Observations about the Deficiencies of Weather-glasses,*

And since I had occasion to speak of the Deficiencies of Weather-glasses, and the mistakes whereto men are liable in the Judgement they make of Cold and Heat upon Their Informations, it will not perhaps appear impertinent to add three or four Considerations more to excite men to the greater Wariness and Industry, both in the making and using Weather-glasses, and in their Judging by them.

I. And first, I consider, that we are very much to seek for a Standard / or certain Measure of Cold, as we have settled Standards for weight, and magnitude, and time, so that when a man mentions an Aker, or an Ounce, or an Hour, they that hear him, know what he means, and can easily exhibit the same measure: but as for the degrees of Cold (as we have elsewhere noted concerning those of Heat) we have as yet no certain and practicable way of determining them; for, though, if I use a Weather glass long, 'tis easie for me to find, when the Weather is colder, or when warmer, then it was at the time when the Weather-glass was first finished, yet that is a way of estimating, whereby I may in some degrees satisfie my self, but cannot so well instruct others, since I have no certain way to know determinately, so as to be able to communicate my knowledge to a remote Correspondent, what degree of Coldness or Heat there was in the Air, when I first finished my Thermoscope; For besides that, we want distinct Names for the several gradual differences of Coldness, we have already / declar'd, that our sense of feeling cannot safely be relied upon to measure them; and as for the Weather-glass, that is a thing, which in this case is suppos'd to be no fit Standard to tell us what was precisely the temper of the Air, when it self was first finished, since that does but inform us of the recessions from it, or else that the Air continues in the Temper it was in at the making of the Instrument, but does not determine for us that Temper, and enable us, to express it; as indeed it is so mutable a thing, ev'n in the same place, and oft-times in the same day, if not the same hour, that it seems little else then a Moral impossibility, to settle such an universal & procurable Standard of Cold, as we have of several other things. And indeed there is scarce any Quality, for whose differences we have fewer distinct Names, having scarce any for the many degrees of Coldness that may be

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conceiv'd to be intermediate, betwixt Lukewarmness and the Freezing degree of Cold, and even these are undefin'd enough; for that, which to some mens senses / will feel Lukewarm, by others will be judg'd Hot, and by others perhaps cold; nor is even the glaciating degree of Coldness well determin'd, since not only differing Liquors, as oyl, wine, and water, will manifestly freez much more easily one then another, but even Liquors of the same denomination; and of waters themselves some are more easily turn'd into Ice then others, and I see no great cause to doubt but that there may be sufficiently differing degrees of Cold, whereof the mildest may suffice for the congelation of some waters. I must not forget to add, that the same person, that has made many observations with a Weather-glass, is so confin'd by that numerical Instrument, that if by the spilling of the Liquor, or the cracking of the Glass, or the casual intrusion of some Bubbles of Air, or by any of divers other Accidents that may happen, the Instrument should be spoil'd, he would, though he should imploy again the same Instrument, be reduc'd to seek out a new Standard, wherewith to measure the varying / temperature of the Air. And though it be not difficult to include in the Cavity of a Weather-glass some other fluid Body instead of Air, yet it will be very difficult, if not impossible, to include a Body, fit to resent and show the Alterations of the Ambient Air, without being also liable to receive impressions from it at the time of its being first shut up.

Yet I will not here omit that I have sometimes consider'd whether the essential oyl of Aniseeds (which is that that is distill'd by the intervention of water in a Limbeck) might not, during a good part of the year, be of some use to us, in making and judging of Weather-glasses. For this Liquor, as we elsewhere also note, having the peculiarity of loosing its fluidity during almost all the Winter, and a good part of the Spring, and Autumn too, when the Weather or the time of the day is

colder;†@ this Liquor, I say, being such, in / case you very gently thaw it, and then putting into it, the Ball of a Weather-glass furnish'd with spirit of Wine that will burn all away, you suffer the oyl to re-congeal leisurely of it self, you may by observing the station of the spirit of Wine in the Thermoscope, when the Oyl begins manifestly to curdle about it, be in some measure assisted, to make another Weather-glass like it. For if you put such rectified spirit of Wine into a Glass, the Cavity of whose Sphærical, and that of its Cylindrical part, are as near, as may be, equal to the correspondent Cavities in the former Glass, you may by some heedful Trials, made with thaw'd and recongeal'd oyl of Aniseeds, bring the second Weather-glass to be somewhat like the first; and if you know the Quantity of your spirit of Wine, you may easily enough make an estimate, by the place it

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reaches to in the Neck of the Instrument, whose capacity you also know, whether it expands or contracts it self to the 40, the 30, or the 20 part, &c. of the bulk it was of, when the / Weather-glass was made. By the help of the same Oyl you may make some kind of estimate, though a more uncertain one, of the difference of two Weather-glasses of unequal bigness: And though I know how much may be alledg'd to show the uncertainty of this way of making a Standard for Weather-glasses; yet as what I have formerly represented, may manifest me to be far enough from looking on it as an exact Standard of Cold; so perhaps the way propos'd may not be altogether useless in the making and comparing Weather-glasses, since in such cases, where we are not to expect to hit the mark it self, it is of some advantage to be able to shoot less wide of it then otherwise we should.

II. But not to insist any further on a difficulty, which is so hardly evitable as that, which occurs about settling a perfect Standard of Cold, there *are* unaccuratenesses in the measuring of Cold by Weather-glasses, which *may* be avoided, but are not; For, Men are not wont to take care, that the Stems be even and Cylindrical / enough, but are wont to make use of such, as are much wider at the upper part near the bubble, then elsewhere; nor do they observe, as they might, a proportion betwixt the Diameter of the Bore of the Cylinder, and that of the Cavity of the Sphærical Bubble, and divers other circumstances are commonly neglected, which if well order'd would make much towards the Certainty and instructiveness of the Informations, afforded us by Weather-glasses. To which may be added, that even in those, where some part of the Liquor is expos'd to the external Air, there may be made Contrivances much more convenient, in order, at least, to some particular purposes, then that of the Vulgar Weather-glass, some of which we have imploy'd and others have been either skilfully devis'd, or also happily attempted by some eminently ingenious Members of the Royal Society.†@ And though that, which we have already describ'd in another Treatise,†\* be very simple, yet it is much more commodious for several of the following / Experiments of Cold, then that, which is commonly in use. For in this, where the included Air is as it were pendulous at the Top of the Glass, 'tis very troublesome and difficult so to apply Cold Bodies, and especially Liquid ones to it, as therewith to measure their Temper, whereas the Thermometers, I speak of, being made by the insertion of a Cylindrical pipe of Glass (open at both ends) into a Viol or Bottle, and by exactly stopping with sealing wax, or very close Cement the Mouth of the Viol, that the included Air may

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have no communication with the External, but by the newly mentioned Pipe:†\* In this kind of Instrument, I say, by chusing a Viol as large, as you please, and fitting it with a Cylynder, slender enough, the proportion between the part of the Viol possest by included Air, and the Cavity of the Cylinder, in which the Liquor is to play up and down, may be easily made so great, as to make the Liquor in this Instrument, with the same degree of Heat or Cold, rise or fall four or five, or more times as / much as the pendulous Liquor is wont to do in an Ordinary Weather-glass, where the cavity that lodges the Air, is wont to be much too small, considering the Bigness of the pipe, whereinto the Air must, when 'tis rarifi'd, expand it self. But 'tis not the greater sensibility (if I may

so speak) of this very kind of Weather-glasses, nor their not needing frames, that makes me take notice of them in this place (where I purposely pass by contrivances that I know to be more curious) but this other Quality, which makes them fit for divers of the following Experiments, wherein we shall have occasion to mention them, namely, that with little or no trouble and inconvenience we may imploy Liquors or other Bodies to refrigerate the included Air, by immersing the Viol, if need be (by a weight) into the Liquor to be examin'd, and letting it stand there as long as we please. And so we may also measure the Coldness of Earth, Snow, powder'd Ice, and other consistent Bodies, which may be heap'd about the Viol, or in which it may be buri'd. /

*III.* I consider too, that though men are wont confidently enough to conclude, that in case (for instance) the Coldness of the weather make the Liquor in a Thermoscope yesterday an Inch higher then 'twas the day before, and this day an Inch higher then 'twas yesterday, the Air must be this day as cold again as it was yesterday, or at least that the increase of Cold must be double to what it was yesterday, and so in other proportions, yet the Validity of this Collection may very justly be Question'd; For, though we should grant, that Cold is that which of it self, or by its own power contracts the Air, yet how does it appear, that a double degree of Cold must produce a double degree of condensation in the Air, and not either more or less. Since besides that 'tis taken for granted, but not prov'd, that the differing Quantities of included Air in several Instruments, and the differing bignesses of the Pipes, and the differing degrees of Expansion, wherein the included Air may happen to be, when the Ascension of the Water begins to / be reckon'd, may render this *Hypothesis* very suspicious; besides all this, (I say) I am not inclin'd to grant (what Philosophers have hitherto suppos'd) that the Condensation of the Air, and the ascension of the Water is *only*, or so much as *principally*, effected by the proper Virtue of the Cold, but by the pressure of the Ambient Air, as we shall ere long more fully declare: And if this be made out, then the computation, we are considering, will be found to be very fallacious, for we have elsewhere shown,<sup>†@</sup> *That the strengths requir'd to compress Air, are in reciprocal*

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*proportion, or thereabouts, to the spaces comprehending the same portion of Air;* so that if a Cylinder (for instance) of four Inches of Air, be just able to resist a strength or pressure equivalent to 10. pound weight, when it comes to be compress'd into two Inches; in this case, I say, an equal force superadded to the former, (which makes that a double force, or equivalent to 20 pound weight,) will drive up that already compress'd Air into half the space; that is, into one Inch or / thereabouts; whence it follows, that in estimating the condensation of the Air in a Weather-glass, we must not only consider, how much space it is made to desert, but also, what proportion that deserted space bears to the whole space it formerly possess, and to what degree of density it was reduc'd, before the application of the then force; and we must remember, that the resistance of the included Air is not to be look'd upon, as that of a weight, which may remain always the same, but that of a spring forcibly bent, and which is increas'd more and more, as it is crowded into less and less Room. But these Nicer speculations it would here be somewhat improper to pursue.

*IV.* Wherefore I shall proceed to what may seem a Paradox, that even the particular Nature of the Liquors, imploy'd in Weather-glasses, is not altogether to be neglected, till we have a better and more determinate Theory of the causes of Cold, then I fear we have: For, though usually it matters not much, what Liquor you imploy, yet 'tis not impossible, / that in some cases men may slip into mistakes about them, for it will not follow, that if of two Liquors, the one be much the more obnoxious to the higher degree of Cold, that of Glaciation, the other must be less easily susceptible of the lower degrees of Cold; since those, that make seal'd Weather-glasses, some with water, and some with spirit of wine, have confessed to me, that they find these (last nam'd) much more apt to receive notable impressions from faint degrees of Cold, then those that are furnished but with water, which yet is easily turn'd into Ice by the cold of our Climate, which will by no means produce the like effect upon pure spirit of Wine.

Besides we cannot always safely conclude (as Philosophers and Chymists generally do) that the



more subtile and spirituous Liquors must be the least capable of being congealed (that is, made to lose its fluidity, as oyl and some other substances are wont to be reduc'd to do by the Action of Cold) for the Chymical Oyl of Aniseeds distill'd by a Limbeck is / so hot and strong a Liquor, that a few drops of it conveniently dissolv'd will make a whole Cup of Beer taste as strong, and perhaps heat the Body as much as so much Wine, and yet this hot and subtile Liquor I have found upon Trial, purposely made, to be more easily congealable (in the sense freshly explain'd) by cold, then even common water; and to continue so several days, after a Thaw had resolv'd the common Ice into fluid water again. And I know some distill'd Liquors, whose component particles are so piercing and so vehemently agitated, that the tongue cannot suffer them, and they are not perhaps inferior to most Chymical Oyls, nor to *Aquafortis* it self, and yet these may be congeal'd by far less degrees of Cold, then such, as would yet prove ineffectual to

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freez either the generality of Chymical Oyls, or the generality of saline spirits.

And indeed till we attain to some more determinate Theory of Cold, and come to know more touching its causes, then we yet do, I see not, / why it should be absurd to suspect, that though there be some kind of Bodies, which seem fitted to produce Cold indiscriminately in the Bodies they invade or touch, yet if the refrigeration of a Body be but the lessening of the wonted or former agitation of its parts (from what cause soever that remisness proceeds) it seems not impossible, but that besides those Bodies or Corpuscles, that may be look'd upon as the Catholick Efficients of Cold, there may be particular Agents, which in reference to this or that particular Body may be call'd frigorifick, though they would not so much refrigerate another Body, which perhaps would be more easily affected, then the former, by other efficients of Cold. For we may observe, that Quicksilver may be congeal'd by the Steams of Lead, which have not been taken notice of to have any such Effect upon any other fluid Body, and yet Quicksilver is not to be depriv'd of its fluidity by such a degree of Cold, as would freez not only water but wine. And by what we have formerly / related upon the credit of that great Traveller, the Jesuit *Martinius*,<sup>†\*</sup> it seems, that water it self may in some Regions be so dispos'd by the constitution of the Soyl, that 'tis susceptible of strange impressions of Cold in proportion to the Effect, which that degree of Cold produces there in humane Bodies. Besides, *Opium* also, of which three or four grains have too oft destroyed the heat of the whole mass of Blood in a mans Body, though that be a very hot, subtile, and spirituous Liquor, does not sensibly refrigerate water, as far as I could observe with a good seal'd Weather-glass, which I put sometimes in a glass of ordinary water of the same Temper, and (as we guess'd) of the same Quantity, wherein *Opium*, enough to kill very many men, was put in thin slices, and suffered to dissolve; which seems to argue, that as differing Liquors have each their peculiar Texture, so there may be certain Bodies, whose minute particles by their peculiar seize, shape and motion, may be qualified / to hinder, or at least lessen the agitation of the particles of the appropriated Liquor, into whose pores they insinuate themselves; And thereby, according to the lately mention'd supposition, they may refrigerate that particular Liquor without having the like Effect on other Liquors, whose Textures are differing. And I might countenance this by adding, that as fiery and agitated a spirit as that of wine, when well esteem'd, is justly thought to be; yet I know more liquors then one, that being mingled with it, will in a trice deprive it of its Fluidity; and the like change I have sometimes made in some other liquors also. But I must not insist on such matters, having mention'd them but only to awaken mens curiosity and circumspection, and not to build much upon them, which will be easily credited, if it be remembred, that a little above I my self sufficiently intimated, that this Conjecture supposes something about the Theory of

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Cold, which is not yet sufficiently clear'd. Only, because the former Experiments show, That / the various Agitation of the minute parts of a Liquor, whereon its Fluidity depends, may be hindred or suppressed by the intervention of adventitious Corpuscles: But do not *clearly* show, That the Liquor

by being depriv'd of that Kind of Agitation does actually acquire a Coldness: I might subjoyn thus much, that by the Addition of a certain substance (which for just reasons I must forbear to describe) that would scarce sensibly refrigerate common Water; I can make a certain (and for ought I know, one only) Liquor, that is wont to the touch to be much of the Temper of Water, to conceive a considerable degree of Coldness: This, I say, (as strange as it may seem) I might here subjoyn to countenance the Conjectures, I have been delivering, and afford some new Corrolaries; but for the Reason newly intimated I forbear, and the rather because I think it high time to return thither, whence the Considerations, I have offer'd about Weather-glasses, have made me digress. /

I was going then to take notice, upon the Occasion offer'd by what I related of the Influence of the Atmosphæres gravity upon common Weather-glasses, of the difference between them and those that are Hermetically seal'd. And indeed, *these* are in some things so much more convenient then the others, that (if I be not mistaken) it has already prov'd somewhat serviceable to the Inquisitive, that I have directed the making of the first of them, that have been blown in *England*; At the Beginning indeed I had difficulty to bring men to believe, there would be a rarefaction and condensation of a liquor Hermetically seal'd up, because of the School Doctrine touching the impossibility of a *vacuum*, and especially, because I had never seen any Experiment of this kind, nor met with any that had, but after some Trials, which my Conjectures led me to make succesfully enough, that in Hermetically seal'd Glasses, both Air and Water might be alternately rarifi'd and condens'd; I found my work much facilitated by the sight of / a small seal'd Weather-glass, newly brought by an Ingenious Traveller from *Florence*, where it seems some of the Eminent *Virtuosi*, that enobled that fair City, had got the start of us in reducing seal'd Glasses into a convenient shape for Thermoscopes. But since that, the Invention has in *England* by a dexterous hand, that uses to make them for me, been improv'd, and the Glasses we now use are more conveniently shap'd, and more Exact then the Pattern, I caused the first to be made by.†\* But the filling of these long ones that we now use, is a work of more niceness and difficulty, then they that have not tried will be apt to imagine, and therefore may elsewhere deserve either from our Pen, or his, that is most vers'd in making them, a more particular account of the way of Performing it: The advantages of these Weather-glasses being at no hand inconsiderable. For, the weight or pressure of the Atmosphære (which, as we have noted, may work very much upon others,) their being seal'd defends

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them from: And by / this Advantage they may be us'd in the highest and in the deepest places, with as much certainty as any where else. Next, whereas in other Thermometers the Liquor is very subject to be spilt, in case they be removed from place to place, and which is worse, though they be not remov'd, is subject to be prey'd upon and wasted by the Air, whereby informations of such Weather-glasses are rendred in Tract of time somewhat uncertain: In seal'd Weather-glasses, there is no danger, that Liquor should either spill or evaporate. And upon the same Account, *these* have this Advantage, that you may safely let them down into the Sea, and immerse them in any Liquor, you please, without excepting the most corrosive to examine their Coldness: Not to mention, that instead of the courser Liquors used in common Weather-glasses, which are some of them not unapt to freez, and others unapt enough to comply with the slighter alterations of the Air, and instead of the colourless Liquor, whether water or no (I know / not) us'd in the *Florentine* Weather-glass I saw, We imploy highly rectif'd spirit of Wine, whose being brought to a lovely red with Cochinele, open'd by the most subtile volatile spirit of Urine, by which means the included Liquor is not only very conspicuous and secur'd from freezing, but so susceptible of even the slighter impressions of external Bodies (which would work but faintly on water) that 'tis pleasant to see, how many Inches a mild degree of heat will make the Tincture ascend in the very slender Cylindrical stem of one of these useful Instruments; of which we have spoken the more particularly in this place, because we shall have frequent occasions to mention them in the following Papers; and no body as yet, that we know, has written any Account of them.

But though these Weather-glasses be much more to be relied on, then those that are commonly in

use, yet we would have a Philosopher look upon both these and our Sensories, but as Instruments to be employed by his Reason, when he makes his Estimates / of the Coldness of Bodies: And though perhaps it will signifie nothing in the Event, yet I see not, why it should misbecome a Naturalists Diligence and circumspection to try, whether ev'n such weather-glasses ought to be so far allow'd of, as to hinder men from looking after any other kind of ways of estimating Cold.

For, though the sealing of these Weather-glasses protect the included Liquor from the pressure of the Air, and keep it from evaporating, yet it will not follow from hence, that they must be exempt from all the other imperfections, which we formerly mention'd to be imputable to Weather-glasses.

I know not whether you will allow me to add on this occasion, that the tinted spirit of Wine (and the like may (for ought we know) be said of any such Liquor) being a particular mixture, in case it be allow'd possible, that the subtile steams of such Bodies (as we formerly noted to be frigorifick in respect to some Liquors) may insinuate themselves through the / pores of Glass; as 'tis granted, that the *Effluviums* of the Loadstone do readily per-meate It: in this Case, I say, though I willingly allow it not to be *likely*, yet it is not absolutely *impossible*, that some Steams, that wander through the Air, may be more or less Cold, or may more promote or hinder an agitation among the

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minute parts in reference to It, then in reference to other Liquors: as we formerly noted, that a grain or two of *Opium* will exceedingly allay the warmth and motion of the whole mass of Blood in a mans Body, though ten times that Quantity will not sensibly refrigerate the tenth part of so much water. And that this may appear the less extravagant, I shall here add some mention of an odd *Phaenomenon*, that, as it were, by some Fate has occur'd to me, since I began the Discourse I am now upon; for whilst I was yesterday writing It, I had occasion to Examine by such a Seal'd Weather-glass (as I have been speaking of) the Temper of a certain strange kind of mixture, that towards the close of this Treatise, / I shall have Occasion to take special Notice of: and though to the touch it appear'd but Likewarm, yet having put into it the Ball, and part of the stem of the seal'd Weather-glass, I found the Included Liquor slowly enough impell'd up so high, that at length, to my wonder, it rose eight or nine Inches in a Stem, which was not much above a foot long; but that which I relate, as the surprizing Circumstance, is, that when I had taken out the Thermoscope, and remov'd it again into a deep Glass full of Cold water, whence I had just before taken it out, to put it into the Anomalous mixture, I had a mind to examine; the Tincture in the Weather-glass did not (as it was wont, and as any one would have expected) begin to subside again towards its former station, but continued within about half an Inch or less of the very Top of the Instrument, though neither my own busie Eyes, nor those of a person very well Vers'd in making and using Thermoscopes, could perceive, that the expanded Tincture was any / where discontinued by any Air or Bubbles, which at first we suspected might possibly (though it were very unlikely) have been generated by the Tepor of the mixture. But that which continued our wonder, if not increased it, was, that during four or five hours, that the Instrument continued in the Cold water, and during some hours also, that it was expos'd to the Air, the Tincture did not subside above half an Inch; and which is yet more strange, having left the Glass all night, in the window of a Room, where there was no Chimney, I found in the morning, that its descent was scarce sensibly greater, for it continued about eight Inches higher, then the mark it stood at, when I first put it into the Lukewarm mixture, and how long it will yet retain this strange expansion, is more then I can tell. But by this and what I may have occasion hereafter to relate concerning this mixture, it may appear somewhat the more reasonable to suspect, that even seal'd Weather-glasses furnished with high rectifi'd spirit of Wine, may in some (though / very rare) conjunctures of Circumstances, and from some peculiar Agents, either by their insinuating themselves through the Pores of the Glass, or on some other Account, receive impressions, that, as far as can easily be discern'd, are not purely the genuine and wonted Operations of Heat and Cold.

The Chymist *Orthelius* tells us,<sup>†@</sup> that the Liquor distill'd from the Oar of



*Magnesia* or *Bismute* (which seems to be the same Mineral, that we in English call Tin-glass) will swell in the Glass 'tis kept in, not only manifestly, but very considerably at the full Moon; and shrink at the new Moon; and if all my endeavours to procure that Oar had not prov'd fruitless, I should be able by my own Experience to disprove or confirm so admirable a *Phænomenon*; but being as yet unfurnish'd to make the Trial my self, lest it might appear a Vanity, so much as to mention (without rejecting it) a thing so very unlikely: I shall add, that since I find the Thing for the main, which was delivered by the Chymist, imploy'd / as an Argument by a famous Mathematician (the Jesuite *Casatus*) whose expressions are such, as if he himself had observ'd, that even in stopt Glasses, the foremention'd Mineral spirit increased very sensibly in Bulk about the time of the full Moon, which wonder being admitted, may not only countenance what we were saying, but hint some other very strange things in Nature.<sup>†@</sup> This brings into my mind (what I have elsewhere mention'd) that a Tincture of Amber, I had made with high rectifi'd spirit of Wine, did for many Moneths in a well stopt Glass discover it self to be affected with certain changes, which were thought to proceed from some secret mutations of the Air, that did / sensibly so work, as I had not observed it to do upon other Liquors, wherein the spirit of Wine abounded. And perhaps upon long and diligent observation, one might find a Disparity betwixt Weather-glasses kept in the same place, but furnished with differing Liquors, a Disparity, I say, that could not be so well ascrib'd to any thing as to the peculiar Nature of the Respective Liquors, which, though of divers kinds, may (to add that towards the facilitation of Trials) be made of a very conspicuous colour, by the self-same Metal, Copper, which not only gives the Known colour in *Aqua fortis*, but affords a fair solution in *Aqua Regis*, and it makes a Liquor of a most deep and lovely blew in spirit of Urine, or of *Sal Armoniack*, and the like; nay, I have found, that in good Chymical Oyl of Turpentine (for express'd oyls are too easily congeal'd) the bare filings of it will yield a sufficient Tincture. But because it is yet but a bare suspicion, that Seal'd Weather-glasses made of differing Liquors, but in other points alike may be / otherwise then uniformly affected by the Temperature of the External Air; I shall now add an observation already made, to show, that even the Seal'd Weather-glasses furnish'd with spirit of Wine are not so perfectly secluded from all commerce with external Bodies, and liableness to their

operations, but that they may be wrought upon otherwise then we think. For I have more then once observ'd, that even in seal'd Thermoscopes (made purposely at home for me, and with great care by the expertest maker of Them) after a good while, and when no such matter was expected, there have emerg'd Bubbles, which, whether they proceeded from some undiscernable Particles of Air, harbour'd in the Pores of the Water, which in process of time, by their Union came to make conspicuous Bubbles, or from some dispos'd particles of the spirit of Wine it self by successive alterations brought to a state of Elasticity, I now examine not; but only affirm, that sometimes I have had of these Bubbles great enough to possess / the space of many Inches in the shank of a long seal'd Weather-glass, and I have been troubled with them in more Weather-glasses then one or two: which I therefore take Notice of, not only, because it serves to prove what I was saying, but because it is very fit, an Advertisement should be given of it to prevent mistakes. For when these Bubbles are small, and are generated or happen to stay at or about the Place, where the Sphærical and Cylindrical parts of the Glass meet, they may easily (as I have observ'd) lurk unheeded, and reaching from side to side, so divide the spirit of Wine in the Ball from That in the Stem, that the latter shall not be able to rise and fall according to the changes of the weather; the Bubble notwithstanding its aerial nature, being more indispos'd to be mov'd up and down in the slender Stem of a small Weather-glass, then the spirit of Wine it self, as we have elsewhere shown, that when Air is not forc'd, a Bubble of it will not in several cases so readily pass through a very narrow passage, as would that grosser fluid, Water. /

But all these difficulties (not to call them extravagances) which I have been mentioning about seal'd Weather-glasses, I represent not to show, that it is (at least as yet) worth while to suspect ours so far, as to imploy all the Diligence and Inventions, that were requisite to prevent or silence the



suspensions of a Sceptick, or that might be thought upon, in case the matter did require or deserve such extraordinary Nicety, but only to give men a rise to consider, whether it would be amiss to take in (when Occasion presents it self) as many collateral Experiments and Observations as conveniently we can, to be made use of as well as our Sensories and Weather-glasses in the Dijudications of Cold. And perhaps an Attentive Enquiry purposely made, would discover to us several other Bodies, Natural or Factitious, which we might make some use of in estimating the degrees of Cold. For though (to give an instance) water be thought the Liquor, that is most susceptible of such an Intensity of Cold, as will destroy or suspend its / Fluidity, yet not here to repeat, what we formerly deliver'd of the easie congealableness of Oyl of Aniseeds, we have (as we elsewhere note to another purpose) distill'd a substance from Benzoin, which becomes of a fluid, a consistent Body, and may be reduc'd to the state of fluidity again by very much lesser alterations of the Ambient Air, as to Heat and Cold, then would have produc'd Ice or Thaw'd it. I could also here take notice of, what I have sometimes observ'd in Amber-greese, dissolv'd in high rectifi'd spirit of Wine, or in other Sulphurous or Resinous concretions

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dissolv'd in the same Liquor; for now and then, though it seem'd a mere Liquor in warm Weather, it would in Cold weather let go part of what it swallow'd up, and afterwards redissolve it upon the return of warm weather; some of these concretions, as I have seen in Excellent Amber-greese, shooting into fine figur'd masses, others being more rudely congeal'd. And I might also add, what I have observ'd in Chymical Liquors, (not unskilfully prepar'd / out of Urine, Harts-horn, &c.) which would sometimes seem to be totally clear Spirits, and at other times would suffer a greater or lesser proportion of Salt to Chrystallize at the Bottom, according to the Mutations of the Weather, in point of Cold and Heat. Such kind of instances (I say) I could mention, but I shall rather chuse to prosecute my Examples in that obvousest of Liquors, Water, and add, that even That may afford us other Testimonies of the increased or lessen'd cold of the Air, then that which it gives us in Common Weather-glasses. For in some parts of *France* the Watermen observe, that the Rivers will bear Boats heavier loaden in Winter, then in Summer;<sup>†@</sup> and I have upon inquiry been credibly inform'd, that Seamen have observ'd their ships to draw less water upon the Coasts of frozen Regions (where yet the Sea is wont to be less brackish) then they do on our British Seas: which argues, that water is thicker and heavier in Winter then in Summer. Nay, I shall add, that not only in / differing Seasons of the Year, but even at several times of the same day I have often observed the Coldness of the Air to be (regularly enough) so much greater at one time of the day then at another, that a Glass bubble Hermetically seal'd and pois'd so as to be exactly of the same weight with its equal Bulk of Water, as that Liquor was constituted at one time of the Day, would about Noon, when the warmth, that the Summers Sun produc'd in the Air, had somewhat rarifi'd the water, and thereby made it bulk for bulk somewhat lighter then before, the Bubble would sink to the Bottom of the water, which (for the better marking the Experiment) I kept in a Glass-Tube; but when at night the coolness of the Air had recondens'd the water, and thereby made it heavier, it began by little and little to buoy up the Bubble, which usually by morning regain'd the Top of the Water; and at other times of the day it not unfrequently happen'd, that the Bubble continued swimming up and down betwixt the Top and the Bottom, / without reaching either of them, sometimes staying so long in the same part of the Tube, that it much surpriz'd divers of the *Virtuosi* themselves, who thought the poising of a weight so nicely, not only a *very great* difficulty (as indeed it is) but an insuperable one. But of this Experiment I elsewhere say more;<sup>†\*</sup> and because about other Weather-glasses I have said so much already, I think it may not be improper to Sum up my thoughts concerning the *Criteria* of Cold, by representing the following particulars.

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1. That by reason of the various and unheeded predispositions of our Bodies, the single and immediate informations of our senses are not always to be trusted.

2. That though *Common Weather-glasses* are useful Instruments, and the informations they give us are in most cases preferable to those of our sense of touching, in regard of their not being so subject to unheeded mutations: yet ev'n these Instruments being subject to be wrought upon by the differing weights of the Atmosphære, / as well as by Heat and Cold, may (upon that, and perhaps some other accounts) easily mis-inform us in several cases, unless in such Cases we observe by other Instruments the present weight of the Atmosphære.

3. That the *seal'd Weather-glasses*, we have been mentioning, are so far preferable to the *Common* ones, as (especially they not being obnoxious to the various pressure of the external Air) that there seems no need in most cases to decline their reports, or postpone Them to those of any other Instruments: But yet in some nice Cases it may be prudent (where it may conveniently be done) to make use also of other ways of examining the Coldness of Bodies, that the concurrence or variance to be met with in such ways of Examination, may either confirm the Testimony of the Weather-glass, or excite or assist us to a further and severer inquiry.

4. That I would not have Men too easily deterr'd from devising and trying various Experiments (if otherwise not unlikely or irrational) about / the estimating of Cold, by their appearing disagreeable to the vulgar Notions about that Quality. For I doubt, our Theory of Cold is not only very imperfect, but, in great part ill grounded. And I should never have ventur'd at trying to make seal'd Weather-glasses, if I could have been withheld either by the grand Peripatetick Opinion, that (to shun a void) water must remain suspended in Glasses, where if it fall, the Air cannot succeed it; or the general opinion ev'n of Philosophers as well new as old, That Air must be far easier then any visible Liquor condens'd by Cold. /

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The III. Discourse

The III. Discourse,  
Containing  
The II. Paradox,  
Viz.

*Touching the Cause of the Condensation of Air, and Ascent of Water  
by Cold in Common Weather-glasses.*

Though I thought here to end the Præliminary Discourse, as doubting it may be thought prolix enough already, yet for confirmation of what I was lately noting, about the incompleteness of the Theory of Cold (and because the evincement thereof may give rise to many Trials that may enrich the History of Cold) I will here subjoyn a Discourse formerly written on another Occasion. For though upon that Account I am / fain to leave out the beginning of It, as not suted to the present Occasion, yet the main Body of the Discourse may be (I think not improperly) annex'd to what has been already said about Weather-glasses, since it examines the causes of the principal *Phænomenon* of them, and will perhaps help to discover the incompleteness of mens Notions about Cold, by showing that the true cause, ev'n of the most obvious *Phænomenon* of Common Weather-glasses (though almost every man thinks he understands It) has not yet been sufficiently inquir'd into.

The discourse then (that first part of It as forreign to our present purpose) being omitted, is as follows.

---- To prosecute our Disquisition satisfactorily, it will concern us to consider, upon what Account the water rises in Cold Weather and falls in Hot, in common Weather-glasses, whose Construction being so well known, that we need not spend time to set it down, we may forthwith proceed to take notice, That concerning the reason, why in these / Weather-glasses the water, or other Liquor in the shank or pipe, ascends with Cold, and descends with Heat: there are three opinions, that will deserve our Consideration.

The first is the common opinion of the Schools and Peripateticks, and indeed of the generality of learned Men of differing Sects, who teach, that the Cold of the External Air, contracting the Air included in the Weather-glass, and thereby reducing it into a narrower Room then formerly it possest, the water must necessarily ascend to fill the place deserted by the retired Air, lest that space should become a *vacuum*, which Nature abhors.

But against this Explication we have several things to object.

For first, I am not satisfi'd, that any of the Schoolmen or Peripateticks (at least of those I have met with) have solidly evinc'd that Nature cannot be

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brought to admit a *vacuum*. Nor do I much expect to see that assertion well prov'd, by these, or by any other, that forbear to make use of the Argument of the / Cartesians drawn from the Nature of a Body, whose very essence they place in its having extension: which I say, because about this Argument I neither have yet published, nor do now intend to deliver my thoughts.

Next, it seems a way of Explicating, that little becomes a Naturalist, to attribute to the senseless and inanimate Body of water an Aim at the good of the Universe, strong enough to make it act, as if it were a free Agent contrary to the tendency of its own private Nature to prevent a *Vacuum*, that, as is presum'd, would be hurtful to the Universe.

But these Arguments we have elsewhere urg'd, and therefore need not insist longer on them here.

Thirdly, if you take a Bolthead, with a large Ball and long stem; and do, with that and Quicksilver make the Torricellian Experiment, there will be an Instrument prepar'd like a Common Weather-glass, save that the stem is longer, and that the Liquor is *Mercury* instead of Water, and yet in this case we see not, that the *Mercury*, which remains pendulous / in the pipe at the height of about 30. Inches, offers to ascend into the cavity of the Bolthead, to fill up the space, whence the Air was expell'd by the *Mercury*, and which the Quicksilver also by its subsiding deserted. And the outward application of Cold Bodies to the forsaken part of the head will not perhaps Occasion the rising of the Quicksilver a  $\frac{1}{4}$  of an Inch, if half so much, though the like degree of Cold would make the water ascend in a Vulgar Thermometer, though shorter, to the height of several Inches. But this Argument I also on another Occasion further display and vindicate.

Wherefore I shall add one more, taken from the Consideration of these seal'd Weather-glasses, that are describ'd in this present History of Cold. For, in these the Air does not shrink, but rather seems to be expanded, when the weather grows Colder. If it be said, that water being contracted by the Cold, the Air follows it to prevent a *Vacuum*: I answer, that those, that say this, should explain, *why*, whereas in Common Weather-glasses / the water ascends to follow the Air, in these the Air must descend to follow the water: And *why*, since to avoid a *Vacuum* the one in common Weather-glasses, and the other in seal'd ones resists contraction, Nature does not rather make the Air in Common Thermometers, retain the extension, they conceive due to its nature, then put her self to the double Labour of suffering the Air to be preternaturally condens'd, and compelling the water to ascend contrary to its nature. But these Arguments I will not urge so much, as this other, that in our present case, the above propos'd Answer will by no means salve the difficulty. For if the water be really condens'd into less, and the Air expanded into more space then they respectively possest before; I see not, how a *Vacuum* or a worse Inconvenience will be avoided; for I demand, since Glass is granted to be impervious to Air and water (as indeed else Nature would not need to make water ascend contrary to its own tendency in a Common Weather-glass) / what becomes of the Body, that was harbour'd in the space

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deserted by the water upon its Condensation? Which Question, those that do not say, any thing escaped away through the Glass, or that any thing was annihilated, will not easily answer. But this is not all, for I further demand, when the Air expands it self to follow the water, how by that expansion of the Air, a *Vacuum* both *coacervatum* (as the old *Epicureans* spoke) and *interspersum*, is avoided. For the aerial Corpuscles cannot advance into this space deserted by the water, without leaving either in whole or in part the spaces they fill'd before, so that by this remove an aerial Corpuscle only *changes place*, but does not adequately *fill* any *more place* then it did before. But if it be said, that the same Air without any substantial Accession, may adequately fill more space at one time then at another: If this, I say, be pretended, I shall not urge that it appears not, why it were not more easie for Nature in common Weather-glasses, as well as in seal'd ones, to rarifie the / Air, which they teach to be so very easily rarifi'd and condens'd, then to make the heavy Body of water to ascend. For I may very well reply, that I scarce know any Opinion in Natural Philosophy, that to me seems more unintelligible, and more worthy to be confidently rejected, then This harsh *Hypothesis* of Rarefaction. Of which I should think it injurious to so judicious a Philosopher, as my Lord *Brouncker*, to indeavour here to manifest the absurdity, though I had not in another place shewn it already.<sup>†@</sup>

The next Opinion, we are to consider touching the cause of the ascension of Water by cold in Weather-glasses, is that of Mr. *Hobs*, who, in the last Chapter of his Book *de Corpore*, Sect. the 12. having premis'd a delineation of a common Weather-glass, subjoyns this Explication:<sup>†\*</sup>

In the sixth and seventh Articles of the 27. Chap. (where I consider the cause of Cold) I have shewn, that fluid Bodies are made colder by the pressure of the Air, that is to say, by a constant wind that presseth them. / For the same cause it is, that the superficies of the water is press'd at F, and having no place, to which it may retire from this pressure, besides the Cavity of the Cylinder between H and E, it is therefore necessarily forced thither by the Cold, and consequently it ascendeth more or less according as the Cold is more or less increas'd. And again, as the Heat is more intense, or the Cold more remiss, the same water will be depress'd more or less by its own gravity, that is to say, by the cause of gravity above explicated.

But however the Author of this Explication, to prepare us to receive it, tell us, that however the above mention'd *Phænomenon* be certainly known to be true by experience, the cause nevertheless has not yet been discover'd: yet I confess, I think, this newly recited assertion might as well have been plac'd after his explication, as just before it.

For first, whereas he remits us to the sixth and seventh Articles of the 28.

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Chapter (for the reference is misprinted) as containing the / grounds of this Explication, I must profess my self far from being satisfi'd with the general Theory of Cold deliver'd in that Chapter, as being partly precarious, partly insufficient, and partly scarce intelligible, as I shall elsewhere have Occasion to shew: and as for what he particularly alledges in the sixth and seventh Articles of a constant wind, that presses fluid Bodies, and makes them Cold, besides that that is prooflessly affirm'd; we shall anon have Occasion to mention an Experiment, where water was not only much refrigerated, but turn'd into Ice, though it were seal'd up in Glass Vessels, and those suspended too in other Glasses, wherein some of them had Air about them, and some others were totally immers'd in unfreezing Liquors, so that the water that was seal'd up was sufficiently protected from being *raked* by the wind, as Mr. *Hob's* concept of the Cause of freezing requires.

Secondly, I see no necessity, that the Cold should press up the superficies of the Water into the shank of / the Weather-glass, especially since 'tis manifest, that the Water will rise with Cold in a Weather-glass kept in a still place, and free from any sensible wind. Besides that, it should be prov'd, and not barely affirm'd, that an insensible Motion deserves the name of wind, and that such



a one is the cause of the refrigeration of water, and it should be also shewn, how this wind comes to be able to raise the water, and that to the height of many Inches more in one part of the superficies then in another. Besides all this, I say, we find by Experience, that Water powred into a Bolthead, till it have fill'd the Ball, and reach'd a good way into the Stem, will upon a powerful refrigeration, short of freezing (which is the case of water in Weather-glasses, when the Air grows colder) manifestly shrink into a narrower room, instead of being impell'd up higher in the Pipe. And if in an ordinary Weather-glass, with a long shank, you apply a mixture of Ice or Snow, and Salt to the Bolthead, the water will readily ascend in the shank to the / height of divers Inches, which how it will be explain'd by Mr. *Hob's Hypothesis*, I do not well see.

Thirdly, I wonder he should tell us, that the reason why the press'd water ascends into the shank of the Weather-glass, is, because it hath no other place into which it may retire from the pressure of the wind, since he, rejecting a *Vacuum*, and affirming the world to be every where perfectly full, should not, methinks, have so soon forgotten, that in the very Paragraph or Section immediately preceding this, himself had told us, that he *cannot imagine, how the same place can be always full, and nevertheless contain sometimes a greater, sometimes a less Quantity of matter; that is to say, that it can be fuller then full.*<sup>†@</sup> So that I see not, why the water should find more room to entertain it, in the Cylindrical cavity of the Weather-glass already adequately fill'd with Air, then elsewhere. And in the seal'd Weather-glasses, we have above been mentioning, and wherein the water descends with Cold, it will be very hard for Mr. *Hobs* to / make out the *Phænomenon* according to his doctrine. Besides that his Explication gives us no account of the Condensation of the Air by cold in

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such Weather-glasses, as those, wherein the water descends with cold and rises with Heat.

Fourthly and lastly, whereas Mr. *Hobs* takes notice of no other cause of the depression of water in Weather-glasses by Heat, but its own gravity, he seems to have but slightly consider'd the matter. For though in some cases the gravity of the water may suffice to depress it, yet in other cases that gravity alone, will by no means serve the turn, but we must have recourse to the expansive Motion or spring of the Air included in the Cavity of the Glass. For if you place a Thermometer with a large Ball, wherein the water ascends but a little way into the shank, in a window expos'd to the warm Sun, you will often perceive the surface of the water in the Pipe to be a good deal lower, then that of the water on the outside of the Pipe, which shews, that this depression / proceeds not from the bare sinking of the water, but from its being thrust down by the pressure of the incumbent Air; since the waters own weight, would make the internal water fall but to a level with the surface of the external water, and not so much beneath it. And for further proof, you may, by keeping such a Weather-glass long enough in the hot Sun, bring the Air so far to expand it self, as to drive the water out of the shank, and break through the external water in divers conspicuous Bubbles, after whose eruption the remaining Air being again refrigerated by the removal of the Weather-glass into a cooler place, the loss of that part of the Air, that escap'd away in Bubbles, will make the water ascend higher in the shank, then in the like degree of Cold, it would formerly have been impell'd. And thus much may suffice to shew the unsatisfactoriness of Mr. *Hob's* concept.

The third and last opinion we shall mention, is, that of some ingenious modern Naturalists, who acknowledging / that the Air has a weight (which Mr. *Hobs* also does in effect admit, though he make not so good use of it as they) do by that explicate the ascension of water in Weather-glasses, teaching that the Cold of the Ambient Air making the included Air shrink into far less room then it possest before, the water in the subjacent Vessel is, by the weight of the incumbent Air, which presses on it more forcibly in all the other parts of its surface, then it is press'd upon in that included in the shank, impell'd up into that part of the shank, which was newly deserted by the self-contracting Air.<sup>†\*</sup>

But though this Account be preferable by far to those which we mention'd before it, and though it

be not only ingenious, but, as far as it reaches, *true*, yet to me I confess it seems not *sufficient*, and therefore I would supply what is defective, by taking in the *pressure*, (and in some cases the *spring*) of the external Air, not only against the surface of water (for That the newly mention'd explication likewise does) but also against the internal / or Included Air. For the recited *Hypothesis* gives indeed a rational account, why the water is impell'd into the place deserted by the Air; but then supposes, that the Air is made to contract it self by cold alone, when it makes room for the water that

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succeeds in its place, whereas I am apt to think, that both the effects may proceed, at least in great part, from the same cause, and that the pressure of the contiguous and neighbouring Air, does according to my Conjecture eminently concur to reduce the cool'd Air, shut up in the Weather-glass, into a narrower space. This it does in common Weather-glasses, because the Ambient Air retains the whole pressure, it has upon the Account of its weight, whereas the internal Air by its refrigeration, even when but equal to that of the External Air, looses part of the pressure, it had upon the account of its now weakned spring.

But this, as I newly intimated, is not the sole account, upon which the Air may in some sorts of Weather-glasses impel up the water, and contribute / to the condensation of the Air incumbent on the water. For in some circumstances (one or two of which we shall produce by and by) it may so happen, that the rest of the Air, that bears upon the water to be rais'd, will not be so much refrigerated, as the included Air, that is to be condens'd, and consequently the other Air will have a stronger spring, then this last mention'd Air will retain, and therefore the former will have a greater pressure, then the latter will be able to resist.

We shall not now examine, *whether* the spring of the Air depend upon the springy structure of each aerial Corpuscle, as the spring of wool does upon the Texture of the particular hairs it consists of, *or* upon the agitation of some interfluent subtile matter, that in its passage through the aerial particles whirls each of them about, *or* upon both these causes together, *or* upon some other differing from either of them; but this seems probable enough, that as, when Air, being seal'd up in a Glass, / is afterwards well heated, though it acquire not any greater dimensions, as to sense, then it had before, yet it has its spring much increased by the Heat, as may appear, if the seal'd Tip be broken under water, by the eruption of Bubbles by the indeavour of the imprison'd Air to expand it self; so upon the refrigeration of the Air, so seal'd up, though the additional spring (if I may so speak) which the Heat gave it, will be lost upon the recess of that Heat, or as soon as the effect of that heat is destroy'd, yet there will remain in the included Air a considerable spring, and sufficient to make it as well fill (at least as to sense) the cavity of the seal'd Glass, as it did, when its spring was stronger. And proportionably we may conceive, that though Cold, at least such as we meet with in this climate of ours, do make the spring of an included parcel of Air weaker, then it was before the refrigeration of that Air, yet it may not make it so much weaker, but that the aerial Corpuscles may be kept so far extended as not at all (or, scarce sensibly) / to quit the room they possess before, in case there be not contiguous to them any other Body, which by its pressure indeavours to thrust them inwards, and so make them desert part of that space: which clause I therefore add, because, that if the case propos'd *do* happen, 'tis obvious to conceive, that the weakned spring of the Air cannot retain so much force to resist an external pressure, as it would have, if the Cold had not debilitated it, and consequently this cooled Air must yield and suffer it self to

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be condens'd, if it come to be expos'd to a pressure, to which it was but equal before its being weakned. And such in common Weather-glasses is the pressure, that is constantly upon the surface of the water without the Pipe, upon the account of the gravity of as much of the Air or Atmosphære, as comes to bear upon it.

Having thus explain'd our conjecture, we will now proceed to the Experiments we made to countenance it, as we find them entred in our loose notes.

In one of which I find what follows. /

We took a Viol capable of containing five or six ounces of water,<sup>†\*</sup> and having fill'd it almost half full with that Liquor, we inverted into it a Glass-pipe of about 10. Inches long, and much bigger then a large Swans Quill, seal'd at one end, and at the other fill'd top full with water, so that the open Orifice being immers'd under the Vessell'd water (of the Viol) there remain'd no Air at the Top of the Pipe: Then, as much of the Orifice of the Viols neck, as was not fill'd by the pipe, being carefully clos'd with Cement, that no Air could get in or out, the Viol was plac'd in snow and salt, till the vessell'd water began to freez at the Top and Bottom: And according to our expectation we found, that notwithstanding this great degree of infrigation of the Air in the Viol, the water in the Pipe did not at all descend. So that either the Air did not shrink by so great a Cold, or the water, whether to avoid a *vacuum*, or otherwise, did not remove out of the Pipe to possess the place deserted by the refrigerated Air. /

Afterwards we endeavoured to repeat the Experiment with the same Glasses, but having had occasion to be absent a little too long (though not very long) we found at our return the upper and seal'd part of the pipe beaten out, which we suppos'd to have been done by the intumescence of the water in the Viol upon its glaciation.

Wherefore we fastned into the same Viol another Pipe some Inches longer then the former, and drawn very slender at the seal'd end, that it might easily be broken there, and having set the viol to freez as before, without finding the water to descend in the Pipe, we did with a *forceps* break off the slender seal'd end, that the outward Air might come to press upon the suspended water, and, by it, upon the cool'd Air in the viol, whereupon, as we expected, the water was swiftly depress'd, by our estimate, eight or ten Inches, but not so low by a pretty deal, as the surface of the water in the viol.

After this, by rarifying the Air in the Viol, and by blowing into it / through the pipe, the water was rais'd within about half an Inch of the Top of the Pipe, whose slender end being seal'd, the viol was again plac'd in snow and salt, but the spring of the Air at the Top, which was rarifi'd before, was by refrigeration so weakned, that it was unable sensibly to depress the water; wherefore breaking off the Apex, as before, the upper Air immediately drove it down divers Inches.

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Our *last* Tryal therefore, was to leave in the same Pipe about 3½ Inches of Air rarifi'd, as little as we could, and placing the viol in salt and snow, as before, we observ'd, that the Air in the Pipe did, upon the refrigeration of the Air in the viol, expand it self very little, though the water in the Viol were in part turned into Ice; but upon breaking off the slender seal'd end, the outward Air presently depress'd the water above two Inches beneath the last level, and by removing the Glass into a warmer room, we found, that the water ascended a pretty deal above an Inch higher, then the same / uppermost level, whereby we probably concluded our Weather-glass to be stanch.

Thus much I find together in one place among my promiscuos collections: but after this coming to have the conveniency of Glasses so shap'd as to be easily seal'd, I judg'd it fit to make use of some of them to keep ev'n the most suspicious from objecting, that I should also have made some Trials with Glasses, which being Hermetically seal'd would be sure most accurately to hinder all immediate Intercourse betwixt the internal and external Air. And I remember, that once we took a Glass, like the Bolthead of a common Weather-glass, save that the small End was drawn very slender, for the more easie breaking of the Apex:<sup>†\*</sup> And into this Glass a convenient Quantity of water was powr'd, and then the Glass being seal'd up at the sharp end and inverted, the water fell down to that end, and possest its due space in the Pipe: Then the round end of the Glass, having a

mixture of snow and salt appli'd, / about it, though the internal air must needs have been thereby much refrigerated (as will be readily granted, and may be gather'd from divers of the Experiments mention'd in these papers) yet we observ'd not the water manifestly to rise. And though an attentive Eye should in such a Trial discern some sensible intumescence in the water; yet that may well enough proceed from some little expansion of the Aerial particles, which we have elsewhere shewn to be usually latitant in Common water, upon the diminution of the pressure of the Air above the water, caused by weakning that air's spring by the Cold. But when we had, to complete the Experiment, broken the slender end of the Glass under water, the included air, becoming then contiguous to water, that had obtain'd immediate Intercourse with that water, whose surface was every where prest by a pillar of the External air that leaned upon it, the water was by the gravity of that outward air hastily impell'd into the Cavity of the Pipe (the spring of whose air / was, as we said, weakned by the Cold) to the height, if I misremember not, of several Inches.

Another sort of Trials I remember we made after the following manner. We took Glass Bubbles (blown with a Lamp) some of about the bigness of a Nutmeg, and some much greater; each of these Bubbles we furnished with a very slender stem (often no bigger then a Ravens Quill) which was usually divers, and sometimes many Inches long.<sup>†\*</sup> Into this stem a drop or two of water being convey'd, might easily enough, by reason of the Lightness of so

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little Liquor, together with the slenderness of the Cavity (which permitted not the included air to penetrate the water at the sides, but rather impel up the intire Body of it) be kept suspended, and so betray very small changes, (and much smaller then to be taken notice of by common Weather-glasses) as to rarefaction and condensation in the air it lean'd upon.<sup>†@</sup> Now when in one of these Instruments, if watching when the pendulous water was somewhat near the / Top of the stem, we nimblely applied to the Orifice of that stem the flame of a Candle, we could by that Heat almost in a moment seal it up, by reason of the thinness of the Glass, and the slenderness of the stem. And if then we plac'd the thus seal'd Glass in a mixture of snow and salt, how much soever the air within the cavity of the Ball must be, in all probability, refrigerated by this operation, yet it would scarce sensibly, and not at all considerably shrink, as we gather'd from the pendulous waters remaining in the same place, or its falling at most but inconsiderably lower. But if then, with a pair of Scissars or otherwise, we dexterously broke off the seal'd end of the stem, and thereby expos'd the internal refrigerated, to the pressure of the external air, the water immediately would be hastily thrust down, sometimes divers Inches below its former station, and sometimes quite into the cavity of the round end of the Glass. To which we shall add, that not only, when these Thermometers were seal'd, neither the usual degrees / of Cold, nor those of the Heat in the Ambient Air would at all considerably depress or raise the pendulous water, which if the Glass were not seal'd, would, as we formerly noted, shew it self wonderfully sensible of the mutations of the Air as to those two Qualities: But we sometimes purposely tri'd, that though upon the refrigeration of the formerly rarified air in the Glass, the pendulous water were descending fast enough, yet if ev'n then we nimblely seal'd up the open Orifice of the stem (which may easily be done in a trice) the descent of the water would be presently stopt, and it would stay either just in, or very near the same part of the shank, wherein it chanc'd to be, when by sealing of the Glass it came to be fenced from the pressure of the Atmosphære, and in that place it would continue till the seal'd end were broken off. For then in case the ambient air were as cool as it was, when the Glass was seal'd, the water would for the reason already given be further deprest, according as the weakned spring of the inward rarifi'd air / was more or less remote from an equality to the pressure of the ambient air.

Besides, for further Trial, we took a large Glass-egg with a long stem, which stem was purposely so bent, that it represented a glass-Syphon, in whose shorter leg the glass was drawn very small, that it might be the more easily first seal'd, and then broken.

This done, we got in a convenient Quantity of water, which ascended to a pretty height in both the



legs of the bent glass, after which the shorter leg being nimbly seal'd, after the manner hereafter to be mention'd, there remained a pretty Quantity of air above the water in that shorter leg, which

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was purposely left there, that it might by its spring impel up the water in the longer leg upon the refrigeration of the Air included in that longer leg.<sup>†@</sup> All this being done, the whole glass was so plac'd in a convenient frame, that the oval part of it was supported by the frame, beneath which the bended shank of the Weather-glass did hang so, that a / mixture of Ice and Salt might be conveniently laid upon this frame to surround and refrigerate the air included in the Egg, without much cooling the air in the Cylindrical part of the Glass. The account that I find of this Trial in one of my notes, is this.

In the greater bent Egg, that was seal'd up with water, in both legs, upon the application of Ice and Salt to the Ellipsis at a convenient time, the water in the longer leg ascended a little, but not by our guess above a barley Corns length, if near so much, and about four Inches of air (as I remember) that were left in the shorter leg, expanded it self (to sense) as much; but, as soon as I broke off the slender wire, wherein the shorter leg ended, the external air rushing in, made the water rise about two inches and a quarter in the longer leg, and then, there not being water enough, broke through it in many bubbles.

Thus far the note, to which I shall only add, that in this case the ascension of the water in the longer leg cannot / be attributed to the weight of the air in the shorter leg, that being, I know not how much, too small to lift up so much water, but to the spring of that air. And also that we need not marvel, the Expansion of that air should be so small, since some of the Experiments, hereafter to be related, will shew us, that the refrigeration of the air in such Trials (as that newly recited) does not weaken the spring of it any thing near so considerably as one would expect. So that the air in the longer leg could yield but a very little to that in the shorter leg, especially since the smallness of this last nam'd portion of air made its spring to be more easily and considerably weakned by a small Expansion.

Thus far our Paradoxical Discourse, which contains divers particulars, that, being added to the considerations, whereunto we have (by way of Appendix) subjoyned It, might afford us several Reflections: But having dwelt too long on one subject already, we shall now conclude with This, upon the whole matter; /

That there is somewhat or other in the Business of Weather-glasses, which (I fear) we do not *yet* sufficiently *understand*, and which yet, I hope, that by other Trials and more heedful Observations we *shall discover*. /

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### The Paper that was Prefixt

The Paper that was prefixt (by way of a short Prefatory Address) to the ensuing History of Cold, when being to be brought in, and presented to the *Royal Society*, it was put into the hands of (its most worthy President) the Lord Viscount *Brounker*, was as followeth.<sup>†\*</sup>

*Little-Chelsey, Feb. 14. 1662. S.A.*

*My Lord,*

The time Your Lordship and the Society appoint me for the bringing in of my Papers, concerning Cold, is so very short, that to give You the fruits of my Obedience as early as You are pleased to require them, / I must present them You very immature, and I should say very unfit for your Perusal, if you were not aswell qualified to supply Deficiencies and Imperfections as to discern them. For of all the Old Observations, I made divers years ago in order to the History of Cold, I have not yet found enough to fill up one Sheet of Paper: And as for those, I made the last Frosty season, besides that I was several times diverted by Avocations distracting enough, the same sharpness of the weather, which gave me the Opportunity of making some Experiments, brought me an Indisposition, which by forbidding me to be often, and stay long in the cold Air, hindred me from making divers others; and (which is worst of all) whilst I was confin'd to a place where I wanted divers Glasses, and other Instruments I would have employ'd, the ways both by land and water, were so obstructed by the snow and ice, that I could not seasonably procure them from *London*, and was thereby reduc'd to leave several trials, I should have made, either unattempted, or unprosecuted. But lest You should think, that, what I intend only to excuse my unaccurateness, is meant to excuse my Pains, I shall without / further Apology apply my self to do what the shortness of the time will allow me, which is little more then to transcribe into this Historical Collection, most of the Particulars, which Your Lordships Commands exact, though haste will make me do it in the very words, for the most part, that I find them, in a kind of Note-book, wherein

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I had thrown them for my own private use, which I the less scruple now to do, not only because the haste, that exacts from me this way of writing, may serve to excuse it in me, but that it may the better appear, how little I had design'd to wrest or byass them to any preconceiv'd *Hypothesis*. /

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## **The Experimental History of Cold Begun.**

### THE EXPERIMENTAL HISTORY OF COLD Begun.

#### **Title I.**

#### ***Experiments touching Bodies capable of Freezing others.***

To go Methodically to work, we should perhaps begin with considering, what *subjects* are capable, or not capable of harbouring the Quality we are to treat of; And to invite us to this, it seems probable enough, that among the Bodies, we are conversant with here below, there is scarce any except / Fire, that is not, at some time or other, susceptible of actual Cold, (at least as to sense:) And ev'n concerning Fire it self, till that difficulty be clearly determin'd, which we have elsewhere started; namely, whether Fire be nor, as Wind (at least like such as is made by Air blown out of a pair of Bellows) rather a *state of Matter*, or Matter consider'd whilst it is in such a kind of Motion, then a *distinct and particular species of natural Bodies*? there may remain some Doubt, since we see, that Bodies, which may be either *in a Moment*, as Gunpowder, or (as far as sense can judge) *totally*, as high rectifi'd spirit of Wine, turn'd into fire, may yet *immediately before* their Accension, be actually Cold: And as to Gunpowder, *presently after* Accension, its scatter'd Parts caught in clos'd Vessels, will also appear cold to the Touch. But such things nevertheless we must not now insist on, *partly* because it requires the resolving of a somewhat difficult Question, which more

properly belongs to the Considerations about Heat, where we have / already handled it; *partly* because our Design in the following Collections, was not so much to gather and set down *Observations*, that were obvious to any that was furnish'd with a Mediocrity of Attention, as *Experiments* purposely made in order to the History of Cold; and *partly* too, because in this Collection, though we do, as occasion serves, take notice of some Experiments and *Phænomena*, that relate to Cold in General, or indefinitely; yet our chief work has been to find out, and deliver, the *Phænomena* of Congelation, or of that intense Degree of Cold, which either *does* freez the Bodies it works upon, or at least *were capable* of turning common water fitly expos'd to it, into Ice. And this may serve

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for a general Advertisement about the ensuing Papers; and consequently having premis'd it, we shall without any further Preamble proceed to the setting down such things, as we have tri'd and observ'd concerning those Matters: beginning with those that belong to the Title prefix'd to the first Part, or Section, of our History. /

1. The Bodies that are cold enough to freez others, are in this climate of ours but very few, and among the most remarkable, is a Mixture of Snow and Salt, which though little known, and less us'd here in *England*, is in *Italy* and some other Regions much employ'd, especially to cool Drinks and Fruits, which men may easily do, by burying, in this mixture, Glasses, or other convenient vessels, fill'd either solely with Wine, or other Drinks, or else with water, that hath immersed in it the fruits to be refrigerated.

2. The Circumstances we are wont to observe in making and employing this mixture, we shall hereafter in due place deliver, and therefore here we shall only take notice; that we could not find upon some trials, that such Glasses filled with water, as would be frozen easily enough by this mixture of Snow and Salt, would be in like manner frozen, in case we employ'd Snow alone, without mingling any Salt with it. I deny not, that 'tis very possible, that in very cold Countries, as well Snow / as beaten Ice may freez water powred into the Intervals of its Parts. But there is great odds betwixt water so intermingled with Ice or Snow, and only surrounded with it in a vessel where the water is, as it were, in one entire Body, and of a comparatively considerable thickness: And there is also a great Difference betwixt the degrees of coldness in the Air of Frigid Regions, and of *England*. And perhaps too there may be some Disparity betwixt the Degrees of Coldness of Ice and Snow in those Climates, and in ours. And we must have a care, that in case a Vial full of water buri'd all night should freez, we ascribe not the Effect to the bare Operation of the Snow, which may be (entirely, or in great Part) due to the coldness of the Air, which would perhaps have perform'd the Effect without the Snow.

3. But though Snow and Salt mixt together will freez water *better* then *Snow alone*, yet we must not think, that there is any such peculiar vertue in Sea-salt, to enable Snow to freez, but that there are divers other / Salts, each of which concurring with Snow, is capable of producing the like Effect. For we found upon trial, that we could freez water without the help of Sea salt, by substituting in its place, either *Nitre*, or *Alume*, or *Vitriol* or *Sal Armoniack*, or even *Sugar*; for either of those being mingled with a due proportion of Snow, would serve the turn, though they did not seem equally to advance the congealing power of the Snow; nor scarce any of them did do it so well as Sea salt. But of this elsewhere more.

4. When we had made the newly mentioned trials, some particular conjectures we have long had, about the nature of Salts, invited us to try, whether, notwithstanding the comminution and consequent change produced in Salts by Distillation, the Saline Corpuscles, that abound in the distill'd liquors of those concretes, as well as in their solutions, would not

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likewise, by being mixt with it, enable Snow to freez water, at least in small and slender Glasses? This we first went about to try with good / spirit of Salt, but we found, as we fear'd, that though it made a sufficiently quick dissolution of the Snow it wrought upon, yet its fluidity hindered it from being retain'd long enough by the Snow, to the bottom of which it would fall, before they had stay'd so long together, as was requisite to freez so much as a little *Essence*-bottle full of common water.

5. Wherefore we bethought our selves of an expedient, whereby to try the operation, not only of those spirits, but of divers other bodies, which were unapt for a Due commixture of Snow after the way newly mention'd, or of which we had too little, or valued them too much, to be willing to spend quantities of them upon these trials. And this way (that remains to be mention'd) we somewhat the better lik'd, because the Experiments made according to it would also prove Experiments of the transmission of Cold through the extremely close body of Glass.

And even in this way of trying, we did at first meet with a discouragement, which least it should happen / to others, we shall here take notice of, namely, that having put a convenient quantity of Snow into a somewhat thick green glass Vial, though we copiously enough mixt with it a somewhat weak spirit of salt, (being loath to imploy the best we had) and having well stopt the vessel, did carefully shake together, and thereby agitate the mixture in it, yet the Glass appeared only bedew'd upon the outside, without having there any thing frozen. But suspecting, that the thickness of the Glass might be that, which hindred the operation of the included mixture, we put snow and a convenient proportion of the self same spirit of salt into a couple of thin Vials, one of which we clos'd exactly, and the other negligently, and having long shaken them, we found that what adhered to them on the outside, was (though but somewhat faintly and thinly) frozen.

6. And, as to this sort of Experiments we shall here observe once for all, that the Snow or Ice included, together with the Saline Ingredient / (whatever that were) was always thaw'd within the Glass, and that consequently, 'twas the condens'd vapor of the Air, or other liquor that adhered to the outside of the glass, which was turn'd into Ice, which is the Reason, why in mentioning these Experiments we often use the word *freez* in a transitive sense, to signifie the operation of the frigorifick mixture upon other bodies.

7. This premised, let us proceed to relate, that we afterwards took Oyl of *Vitriol*, and mixing it with Snow in such an other vial as that last mentioned, we found its freezing power far greater then that of spirit of salt. And least it should be pretended, that in these Experiments, the cold was not transmitted through the sides of the glass, but that the Air within the vial, highly refrigerated by the mixture, Did upon the account of their free intercourse enable the Air contiguous to the outside of the vial to freez the Dew it met with sticking on it; we prosecuted the Experiments with the addition of this circumstance, that on several occasions / we seal'd up the vial, that contained

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the snow and the other frigorifick body it was mixt with, and afterwards by the help of this mixture froze the externally adhering moisture.

8. Having then according to this way substituted spirit of Nitre for oyl of *Vitriol*, or spirit of Salt, we found, that it froze yet more powerfully then either of those two liquors, and continued to do so in those parts of the outsides of the glass, that were adjacent to the included snow, till that snow was almost totally resolv'd into a liquor. This we tri'd both in a thin seal'd glass, and in a pretty thick glass stopp'd only with a Cork.

9. Afterwards we succesfully enough tri'd the Experiment with spirits less acid, as not only with spirit of Vinegre, but with spirit of Sugar, I mean the Red Emphyreumatical spirit forc'd over in a Retort, which mixt with snow, according to the manner of the Experiment, did at length freez the externally adhering moisture. But the filmes of ice were very thin, and very apt quickly to disappear. /



10. Having thus made a number of trials with acid spirits, we thought fit to make some with Urinous spirits that abound in volatile salt, and accordingly having mixt spirit of Urine and Snow in an open vial, and agitated them, we found that the external moisture did discernably, though not very strongly, freez.

But with spirit of *Sal Armoniack* drawn from Quick Lime (according to the way I have delivered in another Treatise) the operation was quick and powerful enough.

11. Having tri'd to freez water with acid, and with volatile spirits apart, we thought it not amiss to try what they would do both together, and accordingly pouring upon snow both some spirit of Urine, and a little oyl of Vitriol, and shaking them into the snow in an open Vial, we found that the mixture did freez, though the glaciation, in this case produced, were very languid.

12. Having thus tri'd salts disingag'd from their grosser parts, or shattered into Corpuscles by distillation, we made some trial likewise / with grosser salts, as with *Sal Gem*, with a sublimate made with common Sublimate and *Sal Armoniack*, nay, and with both Loaf and Kitchen Sugar, with all which among the like bodies, that I can now Remember, the Experiment succeeded well enough: also a very strong solution of Pot-ashes, mixt with snow in a open single Vial, did freez, but that very faintly. And both a very strong solution of very pure salt of Tartar, and (at another time) a strong solution of Pot-ashes, being the one as well as the other, mixt and agitated with snow in a single vial, produced filmes of ice (though thin ones) on the outside of the glass.

13. After this, we thought fit to make a trial of another kind, of which I find this account among my Notes. We filled a single vial with snow, and then powred into it a convenient proportion of a strongly sweet solution of *minium* in spirit of Vinegre, and having shak'd the mixture together, we found, that this sweet Sugar of Lead, did as well as acid and alcalizate salts, excite the / cold of the snow so much, as to produce filmes of ice on the outside of the glass: but a parcel of the same solution, being for divers hours kept in snow and salt, was not thereby frozen.

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In order to the discovery of some hints of the account, upon which the above mentioned mixtures were more intently frigefactive then snow alone, we sealed up a single vial full of snow unmingled with any other ingredient, and found it to thaw much more slowly then any of those parcels of snow which we had mixt with salts or spirits.

In prosecution of this conjecture, we shall add, that for ought we could find by divers trials, no salt, that helps not the snow to dissolve faster then else it would, did inable it to produce ice, though usually it did produce dew on the outside of the vial, that contained the mixture; and accordingly, neither Chrystals of Tartar, nor Borax, both beaten to powder, nor, which is more (considering what we lately noted of the effects of another sort of Sublimate) / would Sublimate inable the snow to freez; as well the powder of Sublimate, as that of Borax, and that of Tartar, lying for a great while in the snow undissolv'd.

14. Belonging to this matter, I find among my papers also this Note.

[Water of Quick Lime (made, by quenching store of unslak'd Lime in common water) twice tri'd would not make snow freez, perhaps because though the water were kept stopt, yet the liquor having been kept in the glass a twelve-moneth, and more; probably the spirits may have flown away, which I find by inquiring of one that Drinks much Lime-water, that it abounds with, when fresh, and grows destitute of a while after; and possibly also the badness of the Lime was the cause, why being mingled with snow it would not freez, though all the vials, that did not freez, did yet gather store of dew on the outsides (perhaps because of the snow, whose melting alone may suffice to produce that effect.)<sup>†\*</sup>

15. It may seem somewhat more / strange, that distilled oyl of Turpentine, which is so hot and fiery a liquor, should not enable snow to freez, but this agrees not ill with the conjecture lately mentioned, for it will hereafter appear, that in oyl of Turpentine Ice dissolves slower then in Divers other liquors, without excepting common water it self.

16. And yet notwithstanding the bad success of this trial, we were not Discouraged from making another with spirit of Wine; for, though according to the common opinion of Chymists and Physicians, it be a mere vegetable Sulphur, yet we, that have elsewhere ventured to ascribe some such operations to it as Chymists would have belong to Saline Liquors, did not scruple to seal up in a single vial almost filled with snow, a convenient quantity of pure spirit of Wine, (drawn off from quick Lime the better to dephlegm it) and of this mixture we found the operation more powerful then any of those we have formerly mentioned: for the freezing vertue of this did not only last long, both in the seal'd single vial, / and in another that was open, but the inclosed mixture presently crusted the outside of the glass (or of the neck, if it were made to fill that) with ice, which might be taken off in flakes of good breadth, or in pieces of good thickness. Nay, it presently froze Urine into Figured ice, which might be taken off in scales.

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17. This last circumstance puts me in mind of another Experiment, whereby we tried by a vigorous mixture of Snow, and some choice spirit of Nitre, we had met with to freez liquors of more difficult congelation then fair water.

We took then some snow, and mingled with it some of the newly mentioned spirit of Nitre in so luckily a proportion, that it froze very vigorously and very suddenly, insomuch that once almost as soon as it was set to the ground, it froze the vial to the floor it was set on, and the outside of the glass, that contained this mixture, we wetted with spirit of Vinegre, which was frozen into pretty thick ice. But yet (not quite to forget that circumstance) retaining the / salt taste of spirit of Vinegre, and though this mixture would not discernably freez spirit of Nitre on the outside, yet it transmitted cold enough to freez weak spirit of Salt, and to give Us the pleasure of seeing some Saline liquors presently turned into figur'd Ice, as not only the last mentioned spirit exhibited some little (as it were) Saline Iceikles crossing each other, and quickly vanishing, but (which was far prettier) having often observed, that *Sal Armoniack* being dissolved in water, and the solution being put very slowly to evaporate in part, but not too much, away, the remaining liquor would in the cold shoot into parcels of salt very prettily figur'd, some of them resembling combs with teeth on both sides, and others resembling feathers; having observ'd this, I say, and being desirous to try, whether the spirit of *Sal Armoniack*, distilled by the help of quick Lime, being put to congeal on the outside of a glass, would not afford a Resemblingly figured Ice; we found upon trial, both that the mixture was able to / freez that subtile spirit, and also, that it shot into Branches almost like those, exhibited by such salts undistilled. And it was not unpleasant to behold, how upon the inclining the glass so, that the freezing mixture rested a little, near any part of the spirit, this liquor would shoot into such branches as we have been speaking of, so nimbly, that the eye could plainly discern them, as it were, to grow, and hastily overspread the surface of the glass, but those Branches were wont quickly to vanish.

I had almost forgot to mention, that I tried the freezing with snow, and divers fermented Liquors undistilled instead of spirit of Wine, and though the Experiments succeeded not with small Beer, much less with water, yet there was a glaciation, though but slight, produc'd not only by the addition of Wine, but even by that of moderately strong Ale.

18. Having observed, that the Liquors and other bodies, that assisted the snow to freez, were generally such as hastned its dissolution, we / thought it not altogether unworthy the trial, to examine, what would be the Event of procuring a speedy dissolution of the snow, by substituting bodies actually warm, instead of potential hot ones: Of this sort of trials, I find among my Notes these two registred.

[1. Into a single vial almost filled with snow, there was poured a pretty quantity of well heated sand, that it might dissolve the snow in many places at once, without heating the ambient Air, or the outside of the glass; but though

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the solution of the snow seemed to succeed well enough upon the shaking of the vessel, yet the outside of the glass was only bedewed, not frozen.

2. Into another single vial almost filled with snow, we poured some water, which we judg'd of a convenient warmth, and we poured it in by a funnel, that had but a slender orifice beneath, that the warm water might fall into the middle of the snow, without Running to the sides, and taking a convenient time to shake the glass, we did by this way produce / a very considerable degree of cold, and much dew on the outside, but were not satisfied, that any of that dew was frozen, though the success would have invited us to have made further trials in greater glasses, if we had had any more snow at hand.]†\*

Wherefore This Experiment is to be further and more artificially tri'd.

19. It is a common tradition, not only among the vulgar, but (I presume, upon their account) among learned men, that the oftentimes variously, and sometimes prettily enough figur'd hoar frost, which is wont to appear upon glass windows in mornings, preceded by frosty nights, are exsudations, as it were, that penetrating the glass-windows, are, upon their coming forth to the cold external Air, frozen thereby into variously figured ice: How groundless this conceit is, may be easily discovered, if men had not so lazy a curiosity, as not to try (which they may do in a moment, and without trouble) whether the Ice be, according / to the tradition on the outside of the window, and not contrary to it on the In-side, where indeed it is generated of the aqueous Corpuscles, that swimming up and down in the Air within the Room, are by the various motion that belongs to the parts of fluid bodies as such, brought to pass along the window, and there by the vehement cold of the neighbouring external Air, communicated through the glass, condens'd into dew, and frozen into Ice.

20. And because divers modern Naturalists have taught (I think erroneously) the glass is easily enough pervious, not only to Air, but to divers subtile liquors,†\* lest the favourers of this Doctrine should object, that we have ill assigned the natural cause of the ice, appearing on the outside of the glass in the former Experiments, which according to them may rather proceed from the subtler (but yet visible) parts of the excessively cold mixture of the snow and saline bodies penetrating the pores of the glass, and settling on the outside of it: To obviate this objection, I say, and to / confirm what we have taught in another Treatise about the wandring of store of aqueous vapours through the Air, we will add the following Experiments, purposely made to evince these truths.

21. At one time four ounces and a quarter, of a mixture of Ice and Salt, being inclosed in a vial, and thereby enabled to condense the vapours of the ambient Air, was by the accession increas'd 12. grains.

Another time a vial, wherein snow (weighing two ounces six drachms and an half) was suffered to condense the vapid Air, the dew, that partly adher'd to it, and partly fell from it, made the whole weigh four grains more then the vial did, when it was first put into the scale, in which scale we found some water flowing from the dew, which gave that increase of weight. And here let

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me add by the way, that the tip of This seal'd vial, being broken under water, suck'd in a considerable quantity of it, whether, because of some little rarefaction of the Air included in the

sealing, or because of the in-frigidation / of that Air by the snow, or for both these Reasons, or any other, I shall not Now dispute.

22. But other Experiments to the same purpose we made, wherein the increase of weight was more considerable; and that the way, we used, may be the better understood, and the conclusion built upon it the more undiscuss'd, we will add a couple of trials, that we find among our notes concerning this matter.

[In a single vial we seal'd up as much snow and salt, as afterwards, when melted, we found to weigh between five and six ounces, after a while the salt beginning to melt the snow, the Dew on the outside began to congeal, and being rubb'd off, the hoar frost would quickly begin to come again. This vial for further trial being put into a pair of scales with a counterpoise, after a while, as the vapours, that wandred through the Air in the warm room, hapned to be detain'd more and more upon the outside of the glass, and to be there frozen, the scale, wherein the glass was, began to be deprest, and / to shrink lower and lower, after which, by adding a little to the counterpoise, we reduced them again to an *Equilibrium*: And yet after a while, the scale, that held the vial, subsided again more and more, till the Included snow was melted; so that to reduce the scales to their first *Equilibrium*, we were fain to add in all to the Counterpoise a weight, which we Estimated to be about eight or ten grains, (for we had then no great weights by us.) The vial being taken out, there appeared near half a small spoonful of Liquor in the scale it stood in, which proceded from the thaw of the Ice, that was generated about it. But in that part of the scale, which was covered with the convex part of the bottom of the glass, there appeared no wet.

A like or smaller quantity of snow and spirit of Wine being seal'd up in a single vial, the outside quickly appear'd cas'd with ice as high as the mixture reacht within, and this vial also being counterpois'd in a pair of scales, did by degrees depress the scale that held it, till it had sunk it / very low, and about seven grains did but reduce the scales to an *Equilibrium*, but the scales being somewhat rusty, we could not make the Trials with that Exactness we desired.]

23. But at other times, when the Experiment was more luckily, though not more carefully tri'd, with better scales, the increase of weight from the condens'd vapours of the Air, was somewhat more considerable; for I find in a short note,

[That at one time a mixture of spirit of Wine and Snow, weighing three ounces and three quarters, afforded of condens'd vapours about 18. grains.

And at another time a mixture of Snow and *Sal Gem*, weighing three ounces and seventy grains, procured us an accession of water weighing about 20. grains.]<sup>†\*</sup>/

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## Title II.

### *Experiments and Observations touching Bodies Disposed to be Frozen.*

1. 'Twere almost endless to try particularly, which bodies *are*, or *are not* capable of congelation, and the degree of cold would also in such Experiments be (as near as men can) determin'd; because many bodies will freez in one degree of cold, that will not in another; wherefore we are willing to leave these trials to those, that have more leisure and opportunity to prosecute them, and shall only set down *some*, and those, somewhat various, that we may not leave this part of the History of Cold quite unfurnish'd. And we must mention the fewer, because, being in the Countrey, we were not provided of divers of the bodies / which we should have expos'd.

2. In very cold snowy weather, we tri'd, that (besides common water) Urine, Beer, Ale, Milk, Vinegre, and French and Rhenish Wine (though these two Last but slowly) were turned into ice,



either totally, or in part! But such instances will possibly be thought too obvious to be insisted on; therefore I shall add, That not only we froze a strong solution of *Gum Arabick*, and another of white Sugar in common water, but that We took Alume, Vitriol, Salt-Petre, and Seal salt, and made of each of them in a single vial as strong a solution as we could, we also made a strong solution of Verdegrease in fair water (which was thereby deeply coloured) all these we exposed to the cold Air. The solution of Alume, Nitre and Verdegrease froze without affording any notable *Phænomena*, either in the figuration of the Ice, or otherwise: Of the solution of Vitriol there remain'd at the bottom of the glass, a pretty quantity unfrozen, and of a clear substance, whose colour was very high of the Vitriol, / whereas the upper part of the same solution differed very little in colour from common Ice.

3. But because it seems not so strange, that these gross sorts of Saline bodies should be turned into Ice, we thought fit to try, whether or no also divers salts, freed from the grosser parts of their concretes by the fire, were not likewise capable of congelation. We exposed therefore spirit of Vinegre in one small glass, and spirit of Urine in another, to an intense cold, and found, that not only the former, but the latter also froze.

4. We took likewise some of the fiery lixivate salt of Pot-ashes, and a single vial, in which we put, to two ounces of water, a drachm of the Alcali, and exposing it to a very sharp Air, we did, when we came to see the success of the trial, find Ice lying on the top in little sticks (something crossing one another) almost like the Crystals of rocked Petre, and besides these that lay levell'd, there were others that shot downwards in very great numbers.

5. We also found that Oyl of Tartar / *per deliquium*, or at least a strong solution of the fixt salt of Tartar, though it seemed much to resist the cold, yet it was once by snow and salt brought to Congelation.

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### *Appendix to the II. Title.*

Since I wrote the present Book concerning Cold (excepting some of the Appendices) having once had the Opportunity of an Hours Discourse with an Ingenious Man, that not only liv'd some years in *Muscovy*, but was, and is still Physician to the great Monarch of that Empire,<sup>†\*</sup> and having likewise at other times conversed with Navigators, and some other credible persons, that had travelled either to *Greenland*, *Terra Nova*, or other gelid Climates, I propos'd them divers Questions, by their Answers to which, I learned some particulars, which together with others, that I have met with in Voyages and other Books, I think it not / amiss to annex by way of Appendices to the foregoing, and some of the following Sections, or Titles.

About the freezing of common express'd Oyls, I know not well what to determine; For that they may by a very intense Cold be depriv'd of their Fluidity, and be made capable of being cut into portions, that will retain the figure given them, my own Trials invite me to believe; but whether such oyls will be turned into *true* (by which I mean) hard and brittle Ice, is a Question scarce to be determin'd by any Experiments we can make here in *England*, where we could not reduce oyl Olive into Ice: And for the Relations of those that have liv'd in colder Countries, I find them to disagree: For when I asked the lately mention'd Doctor the Question, how far he had known oyl Congeal'd in *Muscovy*? He answered me, That it did there freez much harder then in our Climate, but would not, that *he* had observed, be turn'd into true & perfect Ice. On the other hand I find the Testimony of that Ingenious Navigator Captain / *T. James*, who relating the effects of cold he met with in the Island where he and his men were forc'd to winter, does in one place reckon Oyl among the Liquors, such as Vinegre, and Sack, that ev'n in their house was firmly frozen, and more expressly elsewhere. *All our Sack* (says he) *Vinegre, Oyl and every thing else that was liquid, was now frozen*

as hard as a piece of wood, and we must cut it with a Hatchet.<sup>†@</sup> And Olaus Magnus speaking of the sights, wont to be made upon the Ice in the Northern Regions, *Glacialis Congressus* (says he) *fit in Laneis Calcibus, non pelligibus, aut Coriis unctis: vis enim frigoris, quodcunque sit unctuosum convertit in Lubricitatem glaciale.*<sup>†@</sup>

There being a great Similitude in point of Inflammability, and disposition to mix with many subtle Oleous Bodies, betwixt spirit of Wine and Oyl, and as great an affinity in divers other regards, betwixt that spirit and both aqueous and saline Liquors, with which it will readily mix; I had a great Curiosity to know, what kind of change would be produc'd in vinous spirits, in case they were exposed / to a cold great enough to work a visible change in their Texture; I therefore solicitously inquir'd of the *Russian* Emperors lately

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mention'd Physician, whether or no he had observ'd in *Muscovy* any manifest change produc'd by cold in Hot Waters, and spirit of Wine? To which he returned me this answer; That common Aniseed-water, and the like weak spirits would be turn'd into an imperfect kind of Ice, and that ev'n the very strong spirits, though they would not be turn'd into Ice, would be turn'd into a kind of substance like Oyl. /

### Title III.

#### *Experiments touching Bodies Indispos'd to be Frozen.*

1. We found many liquors, whose subtle parts being by Distillation brought over, and united into very spirituous liquors, and so either totally, or in great measure freed from those phlegmatick or aqueous parts, that dispose Bodies to congelation, could not be brought to freeze, either by the cold of the external Air, to which in frosty nights we exposed them, or by such an Application of snow and salt, as served to freez other Bodies.

2. Of this sort were, among acid *menstruum's*, *Aqua fortis*, spirit of Nitre, of Salt; also oyl of Turpentine, and *almost* all, (I add the word *almost*, because the Essential oyl of Aniseeds, and the Empireumatical / oyl of common oyl will lose their fluidity in a less degree of Cold, then that of our mildest frosts,) I say *almost all* the Chymical oyls we had by us, as likewise spirit of Wine, and other strong spirits of fermented Liquors, and even Sack it self, if it were good, would very hardly be brought to afford us any Ice at all: But among the many liquors, that would not freez, there were a few, whose trials afforded us some circumstances not altogether unworthy their being mention'd.

As 1. I being desirous to satisfie some friends, that twas the brisk spirit of the Grapes, whether resulting from, or extricated and exalted by fermentation, that kept (all) the rest of the Sack from freezing: I took a parcel of that liquor, that would afford us no Ice at all, and by the help of a lighted candle, or some other actually flaming body, kindled it, and letting the inflammable part burn away, the remaining part of the Liquor (which was by vast odds the greatest part) was easily brought to freez. /

Next when the formerly mentioned trial was made with water and Pot-ashes, we likewise, in another glass, exposed a solution, wherein the proportion of salt of Pot-ashes, in reference to the water was four times greater, there being in this ʒij [2 drams] of the salt to ʒi [1 ounce] only of water, and this solution, though the glass were covered with hoar frost and Ice on the outside, froze not at all within. And likewise, when another time we made a very strong solution of salt of Tartar, that was very pure and fiery, it did not freez, though a considerably strong solution of salt of Pot-ashes, that was exposed with it, did. So that these Experiments about the glaciation of Lixivate Liquors must be repeated to be reduc'd to a certainty.

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3. That the common express'd oyls of Vegetables will, after their manner, freez, that is, lose their fluidity, and become, as it were, curdl'd in very cold weather, is a matter of common observation; but I had a mind to try, whether or no Train oyl, that is made of the fat of / Animals, (commonly that of Whales) though not by distillation, properly so called, yet by the help of fire, would not be more capable of resisting the violence of the cold, and accordingly I found, that Train oyl, exposed to the Air in a convenient vial, continued fluid; notwithstanding a more then ordinary sharpness of weather, and this I tried two or three several times, but at length one night proved so very cold, that the next morning I found the oyl unfluid; which differing Events seem a little to Countenance, but more to disfavour the Report of *Olaus Magnus*, who writes,<sup>†@</sup> That whereas in Northern Regions 'tis usual for strong places to lose in winter the protection afforded them in Summer, by their Ditches, though never so wide and deep, because the frost makes them easily passable to the Enemy: This inconveniency is wont to be prevented by pouring into the Ditches, the Ice, if there be need, being first broken, great store of this Train oyl, which swimming upon the surface of the water, and being incongealable / by the cold, protects the subjacent water from the freezing violence of the cold, and keeps the moats unpassable. But because our Author mentions this as a known and vulgar Practice in those Icy Regions, it may perhaps deserve a little Enquiry, whether the Whale Oyl, used by the *Swedes*, *Laplanders*, *Muscovites*, and other Inhabitants of those parts be not differing, either as to the Fishes, 'tis made of, or as to the way of making it, or as to the way of keeping it from such Train Oyl as we Employed, unless perhaps it do already appear by the Relation of writers belonging to those Countries, or of Travellers, that have been in them, that *Olaus Magnus* has in that particular, as I fear, he has in some others, misinformed his Readers.

4. We took notice, that a strong solution of *common Sugar* was easily enough turned into Ice; but on a strong solution of *Sugar of Lead* we could not with salt and snow work the like change, and this, though the trial were not negligently made; which I therefore think not unworthy / to be mention'd, because that the two only Ingredients of this Sugar were *Lead*, which is esteemed a very cold Body, and *spirit of Vinegre*, from which, as I noted above, we did by the like degree of cold to that we here employed, obtain Ice: And though in this *metalline Sugar* we may well suppose the Saline parts of the spirit of Vinegre to be much more concentrated or united, then they were in the spirit; yet the *solution* must abound with aqueous parts: and this Sugar seeming but a kind of Vitriol of Lead, 'tis worth our Notice, that its solution would not freez, as well as that of common Vitriol, though in this latter concrete the metal be corroded by a spirit, which, as far as can be judged by the Liquors afforded in distillation, is very much sharper and stronger then spirit of Vinegre.

5. We likewise tried to freez *Quicksilver*, and for that purpose provided a bubble, that being blown with a Lamp, was but thin, and so flat, that the sides

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almost touched, and it held but a little *Mercury*, and / that by the figure of the Glass, being reduced to a large surface, with but very little depth or thickness, it was far more exposed, then if it had been in a ordinary round Bubble, to the action of the cold; but we could not at all freez this extravagant liquor, though we tried it more then once, and though the last time, we exposed it in the same vessel to the same degree of Cold, wherewith we made one of the following Experiments, that required a very Intense degree of that Quality. And in another thin glass-Bubble we long exposed Quicksilver to an extraordinary sharp air, but though the cold had some operation upon it, not here necessary to be mention'd; yet we could not find, that it did at all bring it to freez: wherefore I could wish that trial were made in *Muscovy*, *Greenland*, *Charles-Island*, or some other of the most Icy Regions, where the Effects of cold (which here are upon Quicksilver but languid) are the most considerable, and sometimes stupendious.

6. It is very remarkable, that / though not only the solutions of other gross salts, but, as we have seen, divers more saline and spirituous liquors, were brought by snow and salt to Congelation; yet a brine made very strong of Common salt, could not be brought to freez at all, though we kept it



exposed with the other saline solutions, that *did* freez, during a whole night, that was exceeding sharp. Which Experiment I also tried many years since, to draw thence an Argument in favour of the *Cartesian Hypothesis* about cold, which I shall not now consider;<sup>†\*</sup> but rather add, that being desirous to try, with what proportions of Sea salt and water, the congelation of them might be effected, I found, I could freez some Sea water, that had been brought up in a Barrel to that Monarch of the *Virtuosi*, the King,<sup>†\*</sup> for the making of trials with it; and that having in a single vial exposed to the Air in a very bitter night, a solution consisting of twenty parts of water, and one of salt, which is double the proportion of salt to be commonly found in our Sea-water, the next day we found a / good part of the Liquor frozen, the Ice swimming at the top in figures almost like Broom, spreading from the surface of the water downwards. And to add, That upon the by, we suffered the Ice of salt-water to thaw, to try, whether it would yield fresh water, but it seemed not devoid of some Brackishness, which whether or no it proceeded from some parts of the contiguous brine, that adhered to the Ice, I leave to further and exacter observations, since I am credibly informed, that in *Amsterdam* there are divers, that use the thaw'd Ice of the Sea-water to brew their Beer with, instead of common fresh water.

3. And since I made that Experiment, I find in the industrious *Bartholinus*'s newly publish'd Book, *De Nivis Usu*,<sup>†@</sup> a Confirmation of the probability of

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the Report I just now mention'd, his words being these, *De Glacie ex marinâ aquâ certum est, si resolvatur, salsum saporem deposuisse, quod etiam non ita pridem expertus est Cl. Jacobus Finckius Academiae nostræ senior, & Physices Professor, benè meritus, in glaciei frustis è portu nostro allatis.* /

### Title III.

#### *Experiments and Observations touching the Degrees of Cold in several Bodies.*

1. After having treated of the Bodies that are the most capable of producing Cold, and of those that are most dispos'd, or indispos'd to receive it, it would be Methodical to take notice of the *Degrees of Cold*, to be met with in differing Bodies: But though a work of this nature might somewhat conduce to the Discovery of Cold in general, yet it is so laborious a Task, and, to be well perform'd, requires so much more of Leisure, and Conveniency, than I am Master of, that I must resign it to those that are better furnish'd with them; which I the freelier do, because the Experiments, which at this / Time make the principal part of our History, being chiefly of the highest Degrees of Cold, we may seem to have done something of what more properly concerns our present Design, by having made the Experiments, anon to be subjoin'd within this present Section or Title. And yet thus much we elsewhere do towards the framing of a Table of the Degrees of Cold, that we do on other occasions set down those hitherto unpractis'd ways that we have imploy'd, to estimate the greater or lesser Coldness of Bodies, by several kinds of Weather-glasses, differing from the common ones, and far more fit than they, for such a Purpose. For by Hermetically seal'd Thermoscopes furnish'd with high rectifi'd spirit of Wine, we can estimate the differing degrees of Coldness in Liquors, of which we shall presently mention an Example. And by using such Weather-glasses, as have their Air included not at the top, but at the bottom of the Instrument, we can within some reasonable Latitude, measure the Coldness both of intire / solid Bodies, or minuter Bodies, as Salts, &c. by beating them *alike*, and *very* small, and placing the Instruments at equal Depths in the powder of each of them. And besides that the shape of these Thermoscopes does, as we have elsewhere shewn, make them proper for these uses, for which the vulgar ones, where the included Air is at the top of the Instrument, are not fit: besides this, I say, 'tis easie in these we make use of, to make the Pipe so slender in proportion to the Cavity of the Vial, whereinto 'tis inserted, that very much minuter Differences of Cold will be *manifest* in these, than are wont to be *sensible* in common Weather-glasses. And besides these two sorts we have elsewhere propos'd, and describ'd a third and new kind of



in a very slender Pipe of Glass, betwixt the outward and the inward Air, makes it far more fit for those Experiments, wherein we either despair, or care not, to *measure* the Difference of Cold betwixt two Bodies, but are / only desirous to try, *whether* or no they differ in Coldness, and in case they do, which of them has most: For these Weather-glasses, are so exceeding sensible even of the minute Differences of Heat and Cold, as manifestly to discover Disparities, which other Thermoscopes are not nice enough to give us any Notice of. Only this Advertisement we must add about them, that when we use them to examine the Coldness, not of liquid, but of consistent Bodies, we alter a little the figure of the wide end of the Glass; and instead of making it a round bubble, as we have elsewhere describ'd, we make it with a flat or flattish bottom, that the whole Instrument might thereon, as on a Basis, stand of it self upright, and so, being still taken up by the open and slender end, for fear of rarifying the included Air, (which Caution is here given once for all) may be transferr'd with a pendulous drop in the Pipe, and plac'd sometimes on one, and sometimes on another of the solid Bodies to be examined by it. For if the Body, 'tis removed / to, be more of less cold then that it rested on before, that coldness communicated through the Glass to the Air, by which the pendulous drop is supported, that Airs Expansion or Contraction will manifestly appear by the rising or the falling of the drop. And thus we have taken pleasure to remove it from one kind of wood to another, from woods to metals, and from metals to stones, &c. But the Expedients, that may be propos'd to improve these little Instruments to the purposes we have been treating of, and the Cautions, that may be added to prevent mens drawing mistaking Inferences from the Informations they seem to give them, will take up more time, then we are willing to spend upon an occasion, that will not perhaps be thought to deserve it, nor much to require any others, then those we shall by and by subjoyn. And therefore I shall proceed to the Experiment promis'd at the beginning of this Title or Section.

2. To make so much as a tolerable Estimate of the Difference betwixt / such great Degrees as are not any of them too weak to congeal water, is a thing, which, as we have not yet known to be attempted, so it seem'd not easie to be perform'd. For, *Freezing* having been commonly reputed the ultimate Effect or Production of Cold, men have not been solicitous to look beyond it. And though the Disparity we find betwixt several Fits of weather, all of them frosty, seem to be too manifest and frequent to be probably ascrib'd to nothing, but the differing Dispositions of our Bodies, yet how to estimate that Difference, it is not so obvious. For though we should have recourse to common Weather-glasses, yet they might easily deceive us, since not only by estimating by them, the coldest day of one Winter, with the coldest day of another, but in judging of the Coldness of any two days in the same fit of frosty weather, there intervenes time enough to make it doubtful, whether the vari'd Gravitation of the Atmosphere, produce not the change observ'd in the Weather-glass. Besides that, admitting / vulgar Thermometers could not, as they easily may, misinform us, they are imploy'd only to

give us an Account of those degrees of Cold, which Nature of her own accord produces in the Air; but not to *discover*, whether or no Nature assisted by Art, may not produce greater: And, 'twill easily be granted, that they are yet less made use of to help us to an *Estimate* of this Disparity. And though some guess may be made by the operations of Cold upon Liquors expos'd to it, yet some, as water, and very aqueous Liquors, will freez too soon, and others, as Vinous spirits, will not at all, (that we have found) here in *England*. And though French-Wine will sometimes be brought to begin to freez, yet that happens but very seldom, and in many Winters not at all, and leaves too great an Interval, betwixt the degrees necessary to congeal Wine, and sufficient to congeal Water, not to mention the uncertainty proceeding from the differing strengths of the Wines.

3. Upon these and other considerations / we thought it requisite to make use of an Expedient, whose Nature and use will be easily gathered out of the following Experiments: And though by a mischance, that broke my Weather-glass, I have been hindred from measuring exactly in what Proportion to the whole bulk the spirit of Wine was contracted, by the surplusage of Cold, that was more then necessary to make water freez, yet I doubt not but something of use to our present Theme, may be thence collected, and especially the main thing design'd will manifestly appear, which is the Intensity of Cold produc'd by Art, beyond that which Nature needs to employ upon the glaciating of water.

[4. A small seal'd Weather-glass furnished with spirit of Wine, the ball being about the bigness of a large Nutmeg, and the Cylindrical stem being very slender, and about ten Inches long, the Ball and part of the stem being immers'd in a vessel of water, half buri'd in snow and salt, when the water began to freez at the top, the bottom and the sides (but / before the Ice had reach'd the Ball, for fear it should break it) the tinted liquor was found subsided to  $5\frac{2}{3}$  Divisions, being half Inches, and being taken out thence, and Ice and Salt being immediately appli'd to the Ball, the Liquor fell lower to about  $1\frac{1}{2}$  Division.]

And that it may not be doubted, but that the water, though in part congeal'd, remain'd warm in comparison of the spirit of Wine, though uncongeal'd, that had been refrigerated by the snow and salt, we will add this other Experiment, which we find in another of our Notes thus set down.

[5.†@ The seal'd Weather-glass being kept in the water till it began to freez, descended to  $5\frac{1}{2}$ : Being immediately remov'd into the same snow and salt, that made the water begin to freez, it descended at the beginning very fast, and afterwards more slowly, till it came to the very bottom of the stem, where it expands it self into the Ball; then being remov'd into the same glass of water, whence it was taken, and which was well stor'd / with loose Pieces of Ice, it did nevertheless hastily ascend at the beginning, and was soon after impell'd to the former Height of five Divisions and an half, or thereabouts.]

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6. But perhaps some amends may be made for the disaster of the Weather-glass, by adding, that I found by another Trial, that the Condensation of Liquors by such Colds, as we are wont to have, or can easily produce here, is nothing near so great as one would imagine. And though for want of a Glass-ball, furnish'd with a neck slender enough, I could not make the Experiment so much to my satisfaction, as perhaps else I might have done; yet the goodness of the scales, I made use of, and some greater care, then possibly every Experimenter would have imploy'd, may make the following Observation Luciferous.

7. We took then (on a cold, but not frosty day) oyl of Turpentine, as a Liquor, whose being free from phlegm or water, we would easily be more certain of, then if we had imploy'd spirit of Wine; and this / oyl it self we rectifi'd in a gentle heat, to make it the more pure and subtle. Then we took a small round vessel of clear glass furnish'd with a conveniently long stem or pipe, and having first weighed the glass alone in a pair of very good scales, we found it to weigh [!ounce!]i, [!drachm!]i,  $56\frac{1}{2}$  gr. [1 ounce, 1 dram,  $56\frac{1}{2}$  grains] then putting in oyl of Turpentine, till it fill'd the round part of the Glass, and ascended a little way into the stem, we carefully mark'd with a Diamond on the outside of the Glass, how high it reach'd, and then weigh'd the Glass and the Oyl together, which weigh'd [!ounce!]ij, [!drachm!]vii, and  $34\frac{1}{2}$  gr. [2 ounces, 7 drams, and  $34\frac{1}{2}$  grains] then we put in by degrees a quarter of a Drachm, and with a Diamond carefully mark'd, how high it reach'd in the pipe, and so we continued putting in several Quantities of oyl, still carefully weighing each parcel in the scale, and marking its height on the outside of the Glass (which we did in order to a certain design, and found it a work tedious and troublesome enough) till the Liquor and the Glass together weighed [!ounce!]iij, [!drachm!]j [3 ounces, 1 dram],  $4\frac{1}{2}$  grains; then we put fair / water into an open-mouth'd Glass, in which we also plac'd the little Bolt-head with oyl of Turpentine, and by such

a circumposition of salt and snow, as is hereafter to be often mention'd,†@ we made the water, which was contain'd in the wide mouth'd Glasses, and by which the Sphærical part of the Bolt-head, containing the Oyl, was surrounded, we made this water, I say, begin to freez, and when we perceiv'd a little Ice to be produc'd in it, we carefully mark'd with a Diamond to what part of the stem the oyl of Turpentine was subsided, and then transferring the Bolt-head into a mixture of snow and salt, where we kept it for an hour or two, till we could perceive it to fall no lower, and marking with a Diamond this station also of the Liquor, we afterwards remov'd the Glass into a warmer Air, till the Oyl by expanding it self had regain'd the highest mark, whence it had begun to sink. Then into a very little Glass, carefully counterpois'd in a pair of exacter scales then the former, we gently poured out of the Oyl, till / what remain'd rested against that mark on the outside of the stem, to which it fell, when the water began to freez: and this we found to amount to somewhat above 9½ grains, so that for conveniency of reckoning, we may

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safely enough take the Intire number of 10. grains. After this we poured out of the remaining oyl into the same little Glass, till, what rested in the Pipe, was even with that mark, to which the snow and salt had made it fall; and this parcel of oyl hapned to be almost precisely of the same weight with the other; so that in this Trial (for perhaps in others, which it were therefore worth while to make, the degree of Cold may much vary the Events) the Artificial way of freezing, we imploy'd, made the oyl subside as much after it had been refrigerated and condens'd by a cold capable of freezing water, as that degree of Cold had been able to condense it at first. And lastly, having deducted the weight of the Glass from the weight of the whole Oyl and Glass, to obtain the weight of the oyl alone; and having divided / the weight of the whole Oyl, *first*, by that of the former parcel, we have mentioned to be ten grains, and *then* by the superadded weight of the second parcel, we took out, (both which parcels together we estimated at twenty grains,) we found that rectifi'd oyl of Turpentine of a moderate temper, being expos'd to such a degree of Cold, as would freez common water, did by shrinking lose but *about* a ninty fourth part of its Bulk, and being reduc'd to as great a degree of Cold as we could bring it to by snow and salt, ev'n then it lost but *about* a forty seventh part of its Bulk; I say *about*, because I thought it needless, as well as tedious to mind fractions and little odd numbers, especially since, as we formerly intimated, it was scarce possible to arrive at a great exactness in such a Neck, as that of our Bolt-head, though it were proportionable enough to the Ball, and chosen among several, that were purposely procur'd for the trying of Experiments.

8. There are some other Trials / about the Degrees of Cold, which for want of Ice and other Accommodations we could not make, as we would have done, *often*; nor shall scarce be able to do it, till more friendly Circumstances afford us an opportunity: And yet because our Trials, though not prosecuted as far as we thought, may possibly prove not unwelcome, we will subjoyn something about two of the chiefest of them.

9. The one was design'd to measure in what proportion water of a moderate degree of Coldness, would be made to *shrink* by the circumposition of snow and salt before it begin by Congelation to *expand* it self: of this, what we shall here take notice, is only, That by a Trial purposely made with common water, in a round Glass furnish'd with a long stem, we found the water in that stem to subside so very little, that, whether or no it were insensible, it was inconsiderable. But probably a greater Quantity of water, and a slenderer stem, would have made the shrinking of the Liquor more Notable; / and upon that Account 'tis, that I here mention It.

10. The other Thing was, to measure by the differing weight and Density of the same portion of water, what change was produc'd in it, betwixt the hottest time in Summer, and first a glaciating Degree of Cold, and then the highest we could produce by Art. And in order to this, we weigh'd with a pair of exact scales, a glass bubble heavier then water, in that liquor, when it seemed to be at a moderate Temper, as to Coldness, and by the Diminution,



which we found of the glasses weight in the water, we easily collected, according to the Rules of the Hydrostaticks, the weight of as much water, as is equal in bulk to the glass Bubble, and thereby the Proportion betwixt the glass and an equal bulk of such water, as we first weighed it in; then by the application of snow and salt, we made that water begin to freez, and weighing in it again the same bubble, 'twas easie to collect by the Decrement of its weight in this refrigerated water, what Proportion an equal Bulk of the / liquor did *then* bear to the Glass; and by comparing these two differing Proportions together, we were assisted to make an Estimate, how much the water was made more heavy, and dense by the Action of a freezing degree of Cold: Afterwards taking our time in Summer, we thought fit in the same parcel of water (that had been purposely reserved in a glass) to weigh the same bubble, that by the difference of its weight in the water, when made much lighter by the heat of the ambient Air, we might obtain the Information we desir'd: to which we shall add, That we also recommended to some *Virtuosi*, that were likely to have the opportunity of gratifying Us, that such an Experiment might be procured to be made in the midst of Summer in some part of *Italy*, by the help of the there not unfrequent Conveniency of a Conservatory of snow, wherein the water might be reduc'd to freez before the end of the same hour, at whose beginning the there warmer Air had given it its greatest Expansion, and so the Difference / betwixt the Density of the same parcel of water might be the more conspicuous.<sup>†\*</sup> But as I have not received any Account of my Desires from abroad, so coming now at home to review the Memorial, I caused to be written of the newly mention'd Observation, I find, that through the Negligence or Mistake of an *Amanuensis*, there must needs be a manifest oversight committed in the setting down the Numbers, which my Memory does not now enable me to repair. And the season being now improper to repeat the Experiment, as well as the numerical parcel of water I had kept, and I employed both times, being thrown away, I think it may be sufficient, if not too much, to have thus particularly intimated the way we took, without ading the Cautions, wherewith we proceeded, nor what Trials we made to the same purpose with high rectifi'd spirit of Wine, since unlucky accidents frustrated our Attempts.

11. Whether the making of these kind of Trials, with the waters of the particular Rivers or Seas, men / are to sail on, may afford any useful estimate, if, and how much, Ships and other Vessels, may on those waters be safely loaden more in Winter then in Summer, may be an Enquiry, of which I shall not in this place take any further notice, then to intimate thus much, That the difference betwixt water highly refrigerated, and that which is but of an usual degree of coldness, is not so great as some Learned Moderns seem to have thought. For on a Day, which (though made cold by snow intermingled with the rain that then fell) was not a frost, we took common water, and weighed in it a glass Bubble, whose weight in the Air was 150. grains, and this Bubble

weigh'd in that water, lost so much of its former weight, as to weigh about  $285/8$  grains: and then by snow and salt, reducing that water to such a degree of Coldness, that it began to be turned into Ice about the inside of a small open glass that contain'd it, we found the same Bubble not to weigh at all above one eighth part of a grain less then it did before: So that, / if we may judge of the shrinking and condensation of the water by the Increment of weight, it shrunk but about a 230. part of its former Bulk, and this according to a pair of scales, that would turn with about the 32. part of a grain: which may keep us from wondring at what we lately delivered concerning the very inconsiderable subsidence of the water, we exposed to snow and salt in a small Bolthead. And it may also make that the more probable, which we not long since related about the oyl of Turpentine not losing much above a 100. part of its Bulk, by being expos'd to such a degree of cold, as made water begin to freez. Whether we may from this, and from the formerly recited Experiment, of the great subsidence of spirit of Wine in a seal'd Weather-glass, safely conclude, these subtile distill'd Liquors to be much more sensible then water of Cold, as well as of Heat, further Trials will best resolve; and these I have not now so much opportunity, as I could wish, to pursue.



12. But they that have a mind to / prosecute Experiments of this kind, and others, that relate to the *Degrees of Cold*, may perchance be somewhat assisted even by these Relations, and especially by those Passages that mention the use of the seal'd Weather-glass, furnished with spirit of Wine, and of those wherein a drop of liquor is kept pendulous. For the former of these being not subject to the Alterations of the Atmospheres Gravitation, nor (as may be probably suppos'd, by reason of the strength of the high rectifi'd spirit of Wine) to be frozen, by sending the same Weather-glass (which may be made portable enough, as I have tried by transporting one of them in a Case that might be easily carri'd even in a Pocket) from one Countrey to another, one may make far better Discoveries of the differing Degrees of Coldness in differing Regions, and know (somewhat near) how much the Air even of *Muscovy*, or *Norway*, or *Greenland* it self, is colder then that of *England*, or any other Countrey, whence the Weather-glass shall be / sent: The Instrument being accompanied with a memorial of the Degree, it stood at, when expos'd to such a Cold, as made water begin to freez.

13. The other Thermometer, where a drop of liquor is kept pendulous, may not only be imploy'd in such cases, where the Pipe and Bubble can be erected upon the Horizon, but by reason, that the outward Air will indifferently impel the Bubble laterally or upwards, upon the Refrigeration of the inward, and that the bubble will not barely by its weight drop out of the inverted Instrument, because of the resistance of the subjacent outward Air; for these causes, I say, such a Thermoscope may, as we have tri'd, be also us'd, where the Pipe shall be held Horizontal, or inclin'd, or even Perpendicularly downwards, so that the flat Part of the Bubble may be appli'd to discover the Coldness, either of the Wall, or of the Ceiling of a room, or other Bodies however scituated. And if the Pipe be made / long and even, (as sometimes

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we imploy one above a foot long) not only sensible, but great Effects of very little Disparities in the Coldness of Bodies, to which the Instrument is appli'd, may with pleasure be observed. And the same drop of liquor may be long enough preserv'd useful in the Pipe. But this Advertisement I shall give, that as sensible as this Instrument appears to be of the nicer Differences of Coldness, as of Heat, yet they that shall have the Curiosity to examine with it, as I have done, the Temperature, I say not, of more resembling Bodies, but of Liquors, that may be thought to have their parts so differinglly agitated, as common Water, high rectifi'd spirit of Wine, and even rectifi'd oyl of Turpentine, (I add not *Dephlegm'd oyl of Vitriol*, because of some odd *Phænomena* not here to be insisted on) will perhaps find the Event so little, in many cases, answer the Expectation he would have had of uniformly finding great Disparities in their actual Coldness, if he had not met / with this Advertisement, that he will not much wonder, that a Person, who wants not other Employments for his Time, was willing to decline so tedious and nice a Task. /

## Title V.

### *Experiments touching the Tendency of Cold Upwards or Downwards.*

1. Though, after the consideration of the sphere of Activity of Cold,<sup>†\*</sup> would be the most proper place to take some Notice of the *Direction of its Activity*, yet because one of the Experiments, that belong to This head, is of great use to facilitate the trial of many of those, that follow, throughout this whole Collection; we will no longer delay to say something of this matter, namely, in what Line, or, if you please, towards what part the frigefactive vertue of cold Bodies does operate the furthest and the most strongly.

2. 'Tis a Known Doctrine among Philosophers, that the Diffusion of Heat tends chiefly upwards, as the / flame of a Candle will burn many things held over it at a greater Distance, then it would considerably warm them at, in case they were held beneath its level, or even by its sides: and 'tis true, that in all cases vulgarly taken notice of, the observation, for reasons elsewhere discoursed of, holds well enough; and therefore it may be worth enquiry, whether in Cold, which is generally looked upon as the contrary Quality to Heat, the diffusion (from cold bodies) be made more

strongly downwards, then either upwards or towards the sides.

About this matter, I can as yet find among my Notes but the two following Experiments, and those not both together.

[A very thin bubble was blown at a Lamp, and purposely made flat at the bottom, that it might be the more exposed to the cold, and it was suspended

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by a string within a pretty deal less then an inch of a mixture of beaten Ice and Salt, wherewith we had half fill'd a conveniently large wide-mouth'd glass, but we / could not find, that a cold, Capable of freezing, did strike so high upwards, for the water in the bubble remained altogether unfrozen; which agrees very well with what we have observed, that a mixture of ice and salt did not congeal the vapours, that wandered through the Air, above half a barley corns breadth higher, then the mixture in the Glass reached.]

3. [A mixture of snow and salt being put into a vial with a long neck, the round part of it was by a weight kept under water, out of which being taken after a while, the outside of the glass beneath the surface of the water was cased with solid Ice, *N.B.* especially about the bottom of the vial, of greater hardness and thickness then one could easily imagine.]<sup>†\*</sup>

4. Thus far the notes, from which nevertheless I will not *positively* conclude, though they seem to perswade it, that the tendency of the cold produced by Bodies qualified to freez others, is greater *downwards* then *upwards*: For, the satisfactory determination of that matter may, for ought I know, require Trials more / artificial and nice, then those we have been reciting. And I could wish, that I could find the last of them to have been carefully repeated and registred, because it seems somewhat strange, that the Ice should be much thicker at the bottom of the vial, then elsewhere, in regard, that when we have, as we very frequently have, put mixtures of snow and salt into vials, and left them in the open Air, we generally observ'd, that the outside of the Glass was cas'd with Ice, or covered with hoar frost, directly over against that part of the inside of the Glass, wherein the frigorifick mixture was. So that part of the snow and salt resolving one another, and falling down in the form of a liquor to the bottom, the unmelted part of the mixture would float upon this liquor, and the external Ice would appear over against the floating mixture, by which it was generated: So that as the mixture grew thinner and thinner, so would the Zone or girdle, if I may so call it, of external Ice, grow narrower and narrower, till at length, when the / snow was quite melted away, the external Ice would quickly also vanish. But from this observation (which we frequently made) That as in such vials the Ice did not appear (as I just now related) above half a corns breadth higher then the mixture in the glass; so I remember not to have observed it much lower beneath the mixture; from those things, I say, it may be probably conjectured, that even the coldest Bodies (at least unless their Bulk alter the case) do not diffuse their freezing vertue, either upwards or downwards to any considerable distance.

5. These trials, as I was intimating, may suggest some difficulties about the last of the two Experiments, transcribed out of my notes. But as 'tis evident these observations were made in the open Air, by the freezing of its roving vapours, and the mentioned Experiment was made under water, so how much this difference of *mediums* may alter the case, as to the way of the Diffusion of cold, I dare not, till further trial, boldly determine, especially since / one Circumstance, to be under the next Title mentioned, about the

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freezing of Eggs, may pass for an additional Experiment as to our present Enquiry: For the Cases obtain'd by frozen Eggs suspended under water, seem to argue, that the Diffusion of their cold was

made every way, since they were quite enclosed in the Ice, they had produced.

6. Though the Experiment of freezing water by the Intervention of *salt* and *snow* be not a new one for substance, yet I hold it not amiss, to make a further mention of it on this occasion. Because that what I am to deliver about it, is a Particular not taken notice of (that I know of) by others; the premising of which, will, according to what we lately intimated, much facilitate the trial of many of the Experiments to be set down in the following part of these papers, and will indeed appear to be of no small moment in our whole Attempt of Framing an *History of Cold*. For it has long seemed to me one of the chief things, that has hindered men from making any considerable / progress in this matter, that whereas glass-vessels are generally much the most proper to freez liquors in, because their transparency allows us to see what changes the Cold makes in the liquors exposed to it; the way of freezing with salt and snow, as it has been hitherto used, does almost as little, as the common way of barely exposing vessels to the cold Air in frosty weather, prevent the unseasonable breaking of the glasses. For in both these ways, the water or other liquor, usually beginning to freez at the top, and it being the Nature of Glaciation, as we shall see anon, to distend the water and Aqueous liquors it hardens, it is usually and naturally consequent, that when the upper crust of Ice is grown thick, and by reason of the Expansion of the frozen liquor bears hard with its edges against the sides of the glass, contiguous to it, the included Liquor, (that is by degrees successively turned into Ice) requiring more Room then before, and forcibly endeavouring to Expand it self every way, finds it less difficult to burst the / glass, then lift up the Ice; and consequently does the former, and thereby spoils the Experiment, before it be come to perfection, or have let us see what Nature would have done, if she had not been thus hindred in her work.

7. The consideration of this invited me to alter the common way of freezing, and order the matter so, that whensoever I pleased, the exposed liquor should not begin to freez at the top or sides, but at the bottom, which I concluded it very easie to do, by mingling the salt with that part only of the snow, which was to lye beneath and about the bottom of the glass I placed in it. For by this means the snow, that was contiguous to the sides, was able but to cool the water, and dispose it to Glaciation, whereas the mingled snow and salt, on which the bottom of the glass rested, did actually turn the neighbouring Liquor into Ice, and lift up the incumbent liquor toward the higher and empty parts of the glass: And this liquor also I could afterwards freez at pleasure, without danger of / breaking the vessel, only by so applying salt and snow to the sides of the glass, that they never reached, except perhaps at the very conclusion of the Experiment, so high by a reasonable distance, as the upper surface of the liquor in the glass; so that the superior parts of that liquor were always kept fluid, and capable of being easily impell'd higher and higher by the Expansion of the freezing parts of the subjacent liquor.

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8. The Speculative inference, that may be drawn from this Experiment, of making water begin to freez at the bottom, not the top, will be more properly taken notice of in another place;<sup>†@</sup> In the mean time I shall only intimate by the way, that there is no great necessity of any nice proportion of salt to snow, nor of any exquisite mixture of them: a third or fourth part of thereabouts of Sea salt, in reference to the snow, will not do amiss; nor do I usually put salt to all the snow at once, unless in some case, wherein I have a mind to freez a liquor quickly, and make a speedy resolution of the snow and salt in order / thereunto; to which I shall only add, that by the way above mentioned, I do upon particular occasions make the exposed liquor freez, not at the bottom or the top, but next to what side of the Glass I please, according to the Exigency of the Experiment. But though it may suffice to have *hinted* the Speculative Inference, that may be drawn from this way of freezing Liquors, it will be expedient to give *explicitely* this practical Advertisement, concerning it, that whereas it seems to have been taken for granted, that snow is necessary in this Artifice, and we our selves were for some time led away with the rest, by that supposition; yet that is but a presumption, and ought to be removed as one very prejudicial to those that with us design the prosecuting Experiments, in order to the *History of Cold*. For snow is but seldom to be found on the ground in

comparison of Ice, and being but a Congeries of many small Icesicles with much Air intercepted among them, it is not (*cæteris paribus*) near so durable as the more intire Body of / solid Ice; and yet we have found by frequent Experience, that Ice well beaten in a Mortar, will serve our turn for Artificial Glaciations, as well (if not in some respects better) as snow, and therefore in this History of Cold we indifferently prescribe *Snow and Salt*, or *Salt and Ice* as the Ingredients of our Glaciating Mixtures. /

## Title VI.

### *Experiments and Observations touching the Preservation and Destruction of (Eggs, Apples, and other) Bodies by Cold.*

1. It is a Tradition common enough, though not here in *England*, yet among those that have given us Accounts of very cold countreys, that if Eggs or Apples, being frozen, be thawed near the fire, they will be thereby spoiled, but if they be immersed in cold water, the internal cold will be drawn out, as they suppose, by the external, and the frozen Bodies will be harmlessly, though not so quickly, thawed. This Tradition I thought fit to examine, *not only* because it may be doubted, whether it will succeed in / our more Temperate Climate, and because I love not to relye upon Traditions, when I have

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the opportunity to examine them (especially if no one Credible Author affirms them upon his particular knowledge,) *but also* because I thought the Experiment, if true, might be so varied and made use of, as to become *luciferous* enough, and afford us divers *Phænomena* of cold, not so easie to be produced by the more known ways of experimenting. And accordingly having exposed some of these Bodies to a cold that was judged sharp enough, we afterwards put them in water, but found not the event answer our expectations, no Ice appearing to be generated; nevertheless we were not hereby so discouraged, as not to repeat the Experiment (which we judged to be not unlikely) with more sollicitousness and advantage then before; and having thereby brought it to succeed, we afterwards made several trials of it with several distinct aims, but cannot now find any Entry of divers of them. But those I have hitherto met with / among my Notes, I shall subjoyn, as having in them some Particulars, that may afford useful hints to an Enquirer into the *History and Nature of Cold*. And I shall set down together, and that in this place (though it would not otherwise be the most proper) those I have met with, because some Circumstances of one or other of them may be of use to us on several occasions in the present Treatise.

2. [An Egg weighing twelve drachms and one grain wrapt in a wax'd paper (to keep it from the liquor of the thawing snow) and frozen with snow and salt, wanted four grains of that weight: put into a dish of fair water there crusted as much Ice about the outside as made the Egg and Ice fifteen drachms and nine grains; the ice being taken off from the shell, and the shell very well dried, the Egg was found to weigh twelve drachms and twelve grains; the Egg being broken, was found almost quite thawed; the Egg frozen swam in water, being thawed it sunk.] /

3. [We took two Eggs strongly frozen, and in a room where there was a good fire, we put one of them into a deep wooden-dish full of very cold water, and set the other by it, upon a table about two yards from the fire, that they might be in Air of the same temper as to heat and cold; then perceiving the Egg, that lay under water, to have obtained a thick crust of Ice, we took it out, and having first freed it from the Ice, broke it, and found that some part of the white was not yet freed from a pretty store of little parcels of Ice, but the rest of the white (which was much the greater part) and the Yelk seemed to be much what of the same consistence, as if the Egg had not formerly been frozen, whereas the other Egg, that lay upon the dry table, had not only its whole white frozen into a consistent Body, but the Yelk it self, though we saw no distinct particles of Ice in it, was grown so hard, that it cut just like the Yelk of an Egg over boiled, and being cut quite through, shewed us certain concentrical circles of somewhat differing / Colours, with a speck much whiter



then any of them in the middle of the Yelk; which last circumstances, whether they were accidental or no, further observation must determine.]†\*

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Note, that though we have not found above once, that frozen Eggs would swim, yet when we had broken such Eggs, the frozen white would swim, but not the yelk.

4. We afterwards repeated the Experiment of laying two frozen Eggs near together in the place above mentioned, the one under water, and the other out of it, till *that* put in water had got a thick Icy crust, and by breaking of them both, presently after one another, were confirmed in the Perswasion, that frozen Eggs will thaw by great odds (*cæteris paribus*) faster when immersed in water, then when surrounded only with Air.

5. [We likewise took a frozen Egg, and from a fix'd place suspended it so by a slender packthread, that it hung quite under water without yet touching the vessel, that the water was in. This we did partly upon / another Design, and partly to observe, whether or no the Ice would in this case be considerably thicker or thinner against the lower parts of the Egg, as we formerly mention'd our selves to have observed it to be very manifestly at the lower parts of a glass, which having Ice and Salt in it, was immersed under water; but when we took out the Egg, after we saw that its Icy case had covered the packthread it was hung by, we found the case upon breaking it, of a thickness uniform enough to keep us from concluding any thing from this trial; since, though there were a pretty deal of Ice generated at so small a distance from the case of the Egg, that it seemed to owe its Production to the same cause; yet, which was somewhat odd, we did not find, that this Ice stuck to that which did immediately embrace the Egg, though we had some faint suspition, that the Rudiments of it might have been very early parted from the Egg, by some little shaking of the table occasioned by peoples passing to and fro in the room.] /

6. [We took some Pippins, and exposing them to freez all night, and putting them the next morning into a Bason of very cold water (though in a warm room) they were not long there without being inclosed with cases of Ice of a considerable thickness; Where note, 1. That that part of a floating Apple, that was immersed under water, had a very much thicker coat then the other part which remained above it. 2. That the extant part seemed likewise to be harder then the immersed. 3. That one of these Pippins being purposely left out of the Bason, but layed by it, seemed upon cutting to be harder and more frozen then those Apples which had been put into the water, which scarce seemed to be at all harder then ordinary Pippins, that had never been set to freez, at least as to those parts of the Apples that were near the rinde, and consequently near the Ice. 4. That neither frozen Pippins nor frozen Eggs, notwithstanding their great power of turning part of the contiguous water into Ice, did appear to Us to detain or congeal / any of the roving vapors of the Air, as Ice or Snow included with Salt in glasses is, (as we have formerly observed) accustomed very remarkably to do.]

7. [We took Eggs, and froze them with ice and salt, till the shells of them were made to crack, then we took them out, and put one of them in Milk, two of them in a wide Drinking Glass full of Beer, and two more in a large

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Glass, wherein we covered them with Sack, that was poured in till it reached much higher in the Glass then the Eggs. But none of these trials produc'd, as we could perceive one grain of ice.]†\* And being desirous to see, whether the Acid salt of Vinegre, or the Cold in a well frozen Egg, would have the chief Operation, if those two Bodies were put together: I found upon Trial, that the Saline parts of the Vinegre began to dissolve the Egg-shell, as appeared by the much altered Colour of it, but the Cold of the ice in the Eggs was not able to freez any part of the water or phlegm of the

8. We had also thoughts of trying whether or no pieces of Iron of several shapes and bignesses, being for divers days and nights exposed to the freezing Air, and afterwards immersed in water, would produce any ice, as frozen Eggs and Apples do. For the Brittleness of the Laths of Stone-Bows in sharp frosts, together with other observations elsewhere mention'd, seem to argue, that (to use a popular phrase) the Frost does also get into these Bodies. And I have been assured by one, whom the Trials, I had made with Eggs and Apples, invited me to consult, that a great Cheese, he immersed in water in a Cold Countrey, was presently covered over with ice. But though, as I said, I had thoughts of making the above mentioned Trials, yet for want of a frost sufficiently durable, I was not able to effect what I design'd. But thus much I tri'd, That though I kept good Lumps of Iron, and as I remember of other Metalls, besides pieces of Glass, and a stone or two of a convenient size, in snow and salt, I know not how much longer, then / would have suffic'd to make Eggs or Apples, or such kind of things fit to produce store of ice in water, upon their being thaw'd therein; yet we could not find, that upon the immersing the several newly nam'd Mineral Bodies, there was the least ice produc'd in the cold water, where we kept them covered. I must not nevertheless omit to make some mention of that which lately seem'd to happen at the door of our own Laboratory (respecting the North East) where some Glasses, newly brought from the shop, and not imployed, lying in a Basket, as they poured water into one of them to rince it, part of it was presently turned into ice, whilst one of my Domesticks held it in his hand, who coming presently to show it me, I suspected the ice might have come from, or rather with the water that was poured into the Glass, but upon enquiring was assured of the Contrary.

9. But here I must not omit another trial relating to the former Experiments, which may seem somewhat odd, if its Event prove constantly / the same, as when we tried it. For after these and divers other Experiments made, with frozen Eggs and Apples, we thought it might be worth the examining, whether or no Ice and the Liquors of these Concretes would produce the like effects, as Frozen Eggs and Apples; and because 'tis usually an easier way, then that which is more common of bringing Bodies, whose degree of cold is more languid, to freez water, to include them with ice or snow in a single vial,

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and so put them upon acting only upon the minute, and easily congealable vapors that wander in the Air: we took that Course in the trials we are mentioning, whose success is thus briefly set down in one of our notes.

[10. Ice and Juice of Pippins well shaken together in a single vial, produced abundance of dew, but we could not satisfie our selves, that it produced any Ice.]

[11. Also Ice and the white of an Egg moderately beaten into a Liquor, were tried, with just the like success: But these trials having scarce / been made above once, and at most but twice, are to be repeated.]<sup>†\*</sup>

12. As for what is said, That Eggs and Apples thaw'd in the water, are better preserv'd then thaw'd by the fires side, we tri'd it in Pippins (for in Eggs the Experiment is not so easily and quickly made) and as far as we could discern, found it true, and somewhat wondred to see, how soon, and how much putrefaction was induc'd into those loosely contexed Bodies by an overhasty thawing.

13. If we may believe the Relations of Navigators, and others of good Credit (of one or two of whom I had the opportunity to make Inquiry) there may be good use made of what happens in the different ways of thawing Eggs and Apples, by applying the Observation to other Bodies, and even to Men, that happen to be dangerously nipp'd by excessive Cold. For it is a known Observation among those, that have inhabited or visited the Northern Climates, that if those, whose hands or

feet, or faces happen to be frozen, approach / them too near or hastily to the fire, they are in danger of losing, or at least much prejudicing the overhastily thaw'd parts. (*Upon divers of us* (says Captain *James*, speaking of his companions) *had the cold rais'd Blisters as big as Walnuts. This we imagined to come, by reason that they came too hastily to the fire.*)<sup>†\*</sup> And therefore they that are more careful to be safely then quickly deliver'd from the painful cold, are wont, before they come near the fire, whether it be open or in Stoves, either well to wash their hands, or other frozen parts, in very cold water, or else to rub them well with Snow it self. And this brings into my mind, that I sometimes indeavoured to find by trial, what Beef long exposed to freez, and differinglly thaw'd, would teach me by way of confirmation of this Tradition; but being then oblig'd to unseasonable Removes from the place where I made my Trials, they did not for that Reason afford me the satisfaction I desir'd; but meeting with an intelligent person, that had been a housekeeper in *Muscovy*, and enquiring / of him whether he had observed any thing about this matter, he told me, that having once had two very large Cheeses frozen, he thaw'd one of them in water, and the other in a Stove, but found, that thawing in water was much the better way of the two; and I was well pleased to be answered by him, that the Cheese, thaw'd in water, did soon acquire therein a Crust of ice.

14. But more memorable is that Relation, which I remember I have read in the experienc'd Chirurgion *Fabritius Hildanus's* Treatise of *Gangrenes*, where he relates from credible Testimony, how the whole Body of a Man was successfully thaw'd, and which is more strange, cas'd all over with ice, by being handled as our Eggs and Apples were. His own words, because the

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Narrative may prove of some use, I shall subjoyn, and they are these;<sup>†@</sup> *Narravit mihi vir quidam nobilis & fide dignus, se, cum eas, Regiones peragraret, incidisse aliquando in Viatorem secundum Viam frigore rigidum, ac pene mortuum, quem plaustro suo impositum, cum deduxisset / in Diversorium, hospes illico demersit in frigidam, quo facto undiquaque ita erupit Gelu, ut ipsius Corpus glacie, seu ferreo Thorace contectum conspiceretur. Tum quoque propinatum illi aiebat Cyathum ampliozem Hydromelitis, quo illi seu potu ordinario utuntur, addito pulvere Cinamomi, Caryophyllorum & Macis, unde sudor in lecto provocatus est; atque ita ægrum ad se rediisse amissis duntaxat manuum & pedum extremis Articulis. Hinc intelligimus hanc Methodum sanandi congelatos veram ac tutam esse, ac eam etiam probat Summus Philosophus qui regiones illas frequentavit, &c.*

15. The Experiment deliver'd at the Beginning of this Title, (of speedily producing ice on the outside of frozen Eggs and Apples, by immersing them in Cold water) I take to be one of the two or three most illustrious, I have hitherto met with about congelation; and as likely as any to assist us to investigate the causes of it. But though the *Phænomena* seem very favourable to their *Hypothesis*, that suppose congelation to be effected by the ingress of frigorifick Atoms into the water or other Bodies / to be congealed; yet (for some reasons) I shall not here offer to draw any speculative inference from the Experiment, contenting my self to have here, and at the beginning of this Section hinted *in transitu* the hopefulness of its proving Luciferous.

16. But I remember that the Title of this Section promises something concerning the preservation and destruction of other inanimate Bodies, as well as Eggs and Apples, by Cold; but as that intimated promise makes the last part of the Title, so what I have to deliver on this subject must not be expected to be other then the last part of this Section. And indeed to be able to add much to that little, which is generally known about this subject, I should either have liv'd in colder Climates then ours, or have had, which I had not, the opportunity of making Experiments, that require length of time. And therefore I shall only propose a general Consideration about this matter, and subjoyn a few of the chief Observations I have met with in / Navigators or others about it. *That* then, which I would premise in general, is only this, That whether Bodies be frozen by the ingress of frigorifick Atoms, which by their intruding in swarms, can scarce avoid discomposing the Texture of the Body,



Congelation, more strongly agitate its parts; which way soever, I say, freezing is effected, 'tis manifest, that the Nature of a frozen Body is, at least for the time, much alter'd, and therefore we thought fit to place it among our general Articles of Inquiry about Cold, what the effects of it may be as to the Conservation or Destruction of the Textures of Bodies. But as for the duly prosecuting this inquiry, we do, as we lately intimated, want the time and conveniency, we judge needful for such a work, the matter seeming to require, that it be watchfully and considerably manag'd, and that both the Nature of particular Bodies, and the differing degrees of Cold, and the differing times wherein the Condition of / the expos'd Body is estimated, be taken into Consideration. For we find, that a moderate degree of Cold preserves many Bodies, and that glaciation destroys, or at least prejudices most others (probably by discomposing or vitiating their Texture) when they come to be thaw'd, though whilst the Frost is in them, it keep almost all Bodies from disclosing any putrefaction.

17. This being the general Consideration I intended to propose, it remains that I add out of credible Writers, or other Relators, some Observations to illustrate and confirm the chief particulars comprehended in it.

And first, that a moderate degree of cold conduces much to the preservation of the greatest part of inanimate Bodies, is a thing vulgarly taken notice of and acknowledg'd. And I do not readily remember any instances that manifest, that any degree of Cold, though more than moderate, provided it fall short of freezing the Bodies expos'd to it, does spoil them. *Regii Mutinenses* (says / the industrious *Bartholinus*) *nivem hoc fine arctè compactam servant in Cellis Nivariis, in quibus fervente æstate vidi carnes mactatorum Animalium à putredine diu se conservasse.*<sup>†@</sup> The next thing I shall mention to our present purpose, is a memorable passage in Captain *James's* Voyage, which shows, that so great a Degree of Cold, as may be suppos'd to have reign'd in his ship, that was frozen up all the Winter in one of the Coldest Regions of the World, was not great enough to spoil the meat and drink, that had layen all that time under water, because it seems by the story, that they were not actually frozen; the words of his Journal are these.<sup>†@</sup> *By the Ninth of May we were come to, and got up our five Barrels of Beef and Pork, and had four Buts of Beer, and one of Cyder, which God had preserved for us: it had layen under water all the Winter; yet we could not perceive, that it was any thing the worse;* which is the more remarkable, because of what we shall note by and by, both out of other Books, and even out of this, about what became / of a stronger Liquor then Beer, once brought to Glaciation: And it seems our Navigator found Cold, if extremely intended, so destructive a thing, that he thought fit to take notice in his Journal, That even a *Cable* having layen under the ice all the Winter, was not *in June found a jote the worse.*<sup>†@</sup>

18. And it seems by a passage in *Simlerus's* account of the *Alpes*, that even Intire Bodies may be very long preserved by snow, and, as far as I can guess by the story, without glaciation. *Refert* (says *Bartholinus*, speaking of him<sup>†@</sup>) *in Rhetis apud Rinwaldios, nivium è monte ruentium moles sylvam & proceras Abietes dejecisse; accidisse etiam Helvetio milite per Alpes iter faciente ut 60. homines & plures eadem nivis conglobatione opprimerentur. Hoc igitur Nivium tumulo sepulti, ad Tempus Æstatis delitescunt, quo solutâ nonnihil Nive Deciduâ, Corpora mortua inviolata patent, si ab amicis, vel transeuntibus quærantur. Vidimus ipsi triste hoc spectaculum, &c.*

19. Secondly, I could alledge many instances to show, that many, if / not most inanimate Bodies, (I say *inanimate*, because of the *Gangrænes* and *Sphacelations* that often rob living men of frozen Toes, Noses, and sometimes other parts) if they be actually frozen, will not disclose any putrefaction, whilst they continue in that state. Nor is this much to be wondred at, since whether



we will suppose, that in Glaciation the moist and fluid parts are wedg'd in by intruding swarms of frigorifick Atomes, or that those restless particles, that were wont to keep the Body fluid or soft, are called forth of it, be the cause of glaciation; which soever of these two ways we pitch upon, we must in frozen Bodies conceive an unwonted rest to be produced of those movable particles, whose internal commotions, and disorderly coalitions and Avolutions, are either the Causes, or the necessary Concomitants of Corruption.

20. On this Occasion I remember, that meeting with a knowing Man, whose affairs stopp'd him during the Winter upon the Coasts of *Sweden* and *Denmark*, being desirous to learn / of him, how long they could in those colder Climates preserve in Winter Dead Bodies unburied, and yet uncorrupted, he told me, he had opportunity to observe, that though the frost lasted, as it usually did in that season, three or four moneths together, or longer, the Bodies might without any Embalming, or other Artificial way of preservation, be kept untainted by the bare coldness of the Air. Of Bodies lasting long unputrified in ice, Navigators and others have afforded us several instances, but we will mention two, because they contain something more remarkable then the rest. The one is thus delivered by *Bartholinus*.<sup>†@</sup> *Notandum, Corpora occisorum hyeme eodem positu, eademque figurâ permanere rigidâ, quâ*

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*ante eadem depræhensa sunt. Visum id extra urbem nostram, quum 11. Feb. 1659. oppugnantes hostes repellerentur, magnaque strage occumberent: alii enim rigidi iratum vultum ostendebant, alii oculos elatos, alii ore diducto ringentes, alii Brachiis extensis gladium minari, alii alio situ prostrati jacebant. Imo ex mari gelato, primo vere resoluto, / eques equo suo insidens integer emersit, nescio quid manibus tenens.* The other instance is afforded us by *Captain James's Journal*, and is by him thus delivered.<sup>†@</sup> *In the Evening (of the 18. of May) the Master of our ship, after Burial returned aboard ship, and looking about her, discovered some part of our Gunner under the Gun-room ports. This man we had committed to Sea at a good distance from the ship, and in deep water near six moneths before. The 19. in the morning I sent Men to dig him out, he was fast in the Ice, his head downwards, and his heel upwards, for he had but one Leg; and the Plaister was as yet at his wound: in the afternoon they digged him clear out, after all which time he was free from noisomness, as when we first committed him to Sea. This alteration had the Ice and water and time only wrought on him, that his flesh would slip up and down upon his Bones like a Glove on a mans hand.* But there is one pertinent particular more, which if it be strictly true, is so very remarkable, that I cannot on this occasion forbear to annex it, which is, That according to the relation of the / Merchants of *Copenhagen*, that return thither from *Spitzburg*, a place in *Greenland*, the extreme Cold will there suffer nothing to putrifie and corrupt, insomuch that Buried Bodies are preserved 30. years intire and inviolated by any rottenness.<sup>†@</sup>

21. Thirdly, though whilest Bodies continue frozen, the cold (as may be supposed) by arresting the insensible particles, from whose tumultuary motions, and disorderly Avolutions Corruption is wont to proceed, may keep the ill operations of Cold upon the violated Textures of Bodies from appearing; yet when once that impediment is removed, divers bodies make haste to discover, that their Texture was discompos'd, if not quite vitiated by the excessive cold. I might alledge on this occasion, that I have shown divers ingenious Men by an Experiment I have taught in another *Treatise*,<sup>†@</sup> that the change produc'd in the Textures of some Bodies by glaciation, may be made manifest even to the sight. For by freezing an Oxes Eye, the Crystalline humour, / which in its natural state is transparent enough, to deserve its Name of Crystalline, though not fluid enough to deserve the Name of humour, lost with its former Texture all its Diaphaneity, and being cut in two with a sharp knife, appeared quite throughout very white. But for confirmation of this I shall rather add, that I remember, that the person formerly mention'd, that had made trial of the two Cheeses, confess'd to me, That, though that which had been thaw'd in Cold water, was very much the less spoil'd, yet they were both of them manifestly impair'd (and the other of them was so in its very consistence) by the Frost, though the Bulk of the Cheeses was very considerable, and though they

22. The next thing I shall alledge to this purpose, is the Observation of the *Hollanders*, even by such a degree of cold as they met with in *Nova Zembla*, before the middle of *October*, at which time their strong Beer, by / being partly frozen, had its Texture so vitiated, that the reunion of its unfrozen to its thaw'd parts could not restore it to any thing near such a spirituous Liquor, as it was before. *We were forc'd* (says *Gerad de Veer*, that wrote the story<sup>†@</sup>) *to melt the Beer, for there was scarce any unfrozen Beer in the Barrel, but in that thick Yeast that was unfrozen, lay the strength of the Beer, so that it was too strong to drink alone; and that which was frozen tasted like water, and being melted, we mix'd one with the other, and so drank it, but it had neither strength nor taste.* And in the next Moneths Journal he tells us, that *their best Beer was for the most part wholly without any strength, so that it had no savour at all.* But a more remarkable instance to our present purpose, is afforded us by our Countrey-man *Captain James*, because it manifests the Cold to have the same effect upon a much stronger and more spirituous Liquor. *I ever doubted* (says he in his Journal<sup>†@</sup>) *that we should be weakest in Spring, and therefore had I reserved a Tun of Alegant Wine unto this time. Of this by putting seven parts of water to one / of Wine, we made some weak Beverage, which (by reason that the Wine by being frozen, had lost his virtue) was little better then water.*

23. And I remember that a learned Man, whom I ask'd some questions concerning this matter, told me, that in a Northern Countrey, less colder then *Muscovy*, he had observed, that Beef having been very long frozen, when it came afterwards to be eaten, was almost insipid, and being boil'd afforded a Broth little better then common water.

24. If I had not wanted opportunity, I should here subjoyn an Account of some Trials, for which I made provision, as thinking them not absolutely unworthy the making, though extravagant enough not to be likely to succeed. For I had a mind to try, not only whether some plants, and other Medicinal things, whose specifick virtues I was acquainted with, would lose their peculiar Qualities by being throughly congeal'd, and (several ways) thaw'd; and whether thaw'd Harts-horn, of which the Quantity of Salt and Saline / spirit of such a determinate strength should beforehand be tri'd by distillation, would, after having been long congeal'd, yield by the same way of distillation the same Quantity of those actual substances, as if the Harts-horn had not been frozen at all. But I had also thoughts to try, whether the Electrical faculty of Amber, (both the Natural, and that factitious imitation of it I elsewhere teach) and whether the attractive or directive Virtue of Loadstones, especially very weak ones, would be either impair'd, or any ways alter'd by being very long exposed to the intensest degrees of Cold within my power of producing. But to have nam'd such extravagancies, is that, which I think enough, and others I fear may think too much.

25. Yet some few things I shall subjoyn on this occasion, because it will add somewhat not impertinent to the Design of this Treatise (which is to deliver

the *Phænomena* of Cold) as well as countenance what I have been proposing; and those things are, That I can by very credible Testimony / make it appear, that an intense Cold may have a greater operation upon the Texture even of solid and durable Bodies, then we in this temperate Climate are commonly aware of. I shall not urge, that even here in *England* 'tis generally believ'd, that Mens Bones are more apt to break upon falls in Frosty, then in other Weather, because that may possibly be imputed to the hardness of the frozen Ground. Nor, that I remember when I was wont to make use of Stone-Bows, I found it a common observation, that in Frosty Weather the Laths, though of Steel, would, by the Cold, be made so Brittle, that unless extraordinary care were had of them, or some Expedients were us'd about them, they would be apt to break. Nor yet, that an Ingenious Overseer of great Buildings has informed me, that those that deal in Timber and other Wood, find it

much more easie to be cleft in hard Frosts, then in Ordinary Weather. These and the like instances I do, as I was intimating, forbear to urge, because these effects of / Cold are much inferior to those that have been met with in more intemperate Regions.

26. And to begin with its Operation upon what we were last treating of, *Wood*. Of *Charleton-Island* Captain *James* has this passage about the Timber, they imploy'd upon their work, *The Boys* (says he†@) *with Cuttle-axes must cut Boughs for the Carpenter; for every piece of Timber, that he did work, must first be thaw'd in the fire*. And a little before, he tells us, that even when they found a standing Tree, *They must make a fire to it to thaw it, otherwise it could not be cut*.

27. And I remember, that two several persons, both of them Scholars, and strangers to one another, that had occasion to travel as far as *Mosco*, assur'd me, that they Divers times observ'd in extreme frosts, that the Timber-work (whether the Boards or the Beams) of some Houses, which, according to the Custom of that Countrey, were made of wood, and perhaps not well seasoned, would, by the operation of the Cold, be made to crack in divers / places, with a Noise, which was surprizing enough to them, especially in the Night.†\*

28. I remember also, that a Physician, who liv'd for some years in one of the Coldest Plantations of the *West Indies*, related to me, that he had observ'd the Bricks, he had imploy'd about Building, to be very apt to be spoil'd by the long and vehement frosts of the Winters there; where he likewise said, that 'twas a usual thing for the Houses builded of Brick, to decay in fewer years by far, then here in *England*, which he said was generally, and, as he thought, truly imputed to the excessive Cold, which made the Bricks apt to crumble, and moulder away. But though I dare not lay much weight on this Observation, unless I knew, whether the Bricks were sufficiently burn'd, and free from pibbles, calcinable by the heat that burn'd the Bricks: yet we must not deny, that extreme Colds may be able to shatter or dissolve the Texture of

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as close and solid Bodies as Bricks, especially if the Aqueous Moisture be not sufficiently / driven away, if we will admit, what I remember I have mention'd in another Treatise, out of a very Learned and credible Author, of the power, that a freezing Degree of Cold has had to break even solid Marble. And much less shall we doubt the possibility of what the Physician related, if we will not reject the Testimony of the Learned *Olaus Wormius*, according to which, Instruments made even of so hard a Metal as Brass, are not priviledg'd from the Destructive Operations of some Degrees of Cold. For *Ex ære facta opera* (says he in his Curious *Musæum*†@) *vi frigoris quandoque rumpuntur, quod tamen pauci credunt, id tamen expertus est Eratostenes, & Nostras Johannis Munckius in difficillimo suo Itinere, quo per fretum Christianum transitum in mare Australe invenire moliebatur*. To which, perhaps most Writers, would, if they met it, add this passage out of the Dutch-mens Voyage to *Nova Zembla*. The 20. (of October) *it was calm Sunshiny weather, and then again we saw the Sea open, at which time we went on Board, to fetch the / rest of our Beer out of the ship, where we found some of the Barrels frozen in pieces, and the Iron Hoops that were upon the Josam Barrels, were also frozen in pieces*. But though this Testimony seems to prove, that extreme Cold may break even Iron it self, and though possibly such an Affirmation might in the general not be erroneous, yet I shall forbear to draw that inference from this passage, because I suspect, that since the Irons, that were broken, were Hoops, and since it seems probable by the story, that there were Barrels not Hoop'd with Iron, broken also by the same Frost; the breaking of the Hoops may have been the effect, not of the violence of the Cold, as acting immediately upon the Iron, but of the Liquor in the vessels, which being by the Cold that froze it, turn'd into ice, was so forcibly expanded, as to burst, what ever oppos'd its dilation, according to what we shall have occasion in its due place more fully to deliver. /

*An Appendix to the VI. Title.*



Inquiring of the formerly mention'd Physician to the *Russian* Emperor,<sup>†\*</sup> what experience teaches about some of the matters treated of in this (sixth) Title, in those cold Climates, where the effects of freezing are more notable: He told me, that the tradition (mention'd above touching the safest way of thawing) is in *Muscovy* generally receiv'd, and that 'tis usual for Men, that have their Cheeks and Noses frozen, to rub them well with snow, and escape unharmed; whereas if they go immediately into their Stoves, they often lose the Tops of their Noses, and introduce into their Cheeks a kind of paralytick Distemper, or benumbedness, that they cannot get rid of in many Moneths.

And having also inquir'd of the same Ingenious person, whether Wine frozen, and then permitted to / thaw, till the unfrozen Liquor had quite

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resolved the ice, was not thereby spoil'd by having its Texture vitiated, he answered, that in very strong Claret-wine he found the Colour scarce at all destroy'd, nor the Liquor otherwise much impair'd; but that in weaker Claret-wine the Colour was spoil'd, and the Liquor was otherwise much the worse. But note, that in the French-wine there remain'd a third part or more unfrozen, so that it seems not to have been expos'd to near so extreme a cold, as that of the *Hollanders*, or of *Captain James*;<sup>†\*</sup> and that Physician likewise told me, that of some very strong Beer, that he had in great part frozen, the ice had some Taste of the Hops, but was dispirited like phlegm.

Having inquir'd how long dead Bodies would keep, he told me, that if they were thoroughly frozen, they would be preserv'd incorrupted till the thaw, though that perhaps might not happen within four or five Moneths after the Death of the Man. He added, that he had the Venison of Elkes sent him unsalted, and yet / untainted, out of *Siberia* (which is some hundreds of leagues distant from *Mosco*) and that Beef and other flesh well frozen, would keep unputrified for a very long time; and when I ask'd whether the freezing did not impair it, he answered, that with long keeping it congeal'd, it will grow very dry and be impair'd in Taste, and will not make so good Broth as meat that was never frozen. And he further told me, that in case frozen meat were leisurely thaw'd, it would be far the less impair'd, and might be well rosted, but if before it was thaw'd, it were laid down to the Fire, it would not ever be well rosted, and would eat very scurvily; and though a shoulder of Mutton, for instance, were kept very many hours turning before the fire, yet it would continue raw in the middle.

Having inquir'd about the rubbing Bodies with Snow to unfreez them, he told me (agreeable to what I noted him to have said above) that he had seen several persons, that had been frozen, & that when a man is told, that he is frozen, and having ask'd whereabouts / (for the party himself usually knows it not) is inform'd, that it is in this, or that place, which is commonly the Nose or the upper part of the Cheek, or perhaps the Tip of the Ear, he usually rubs the part very well with Snow, and lets it thaw by degrees, else, if without that preparation he should go immediately to the Stove, he would be in danger to lose his Nose, or other frozen part. The Doctor added, that they use to rub the frozen Meat and Fish with Snow, and that he once examin'd a Man, who in his youth had been frozen all over, and inform'd the Doctor, that having had occasion in a journey to quit his Sled for a while, and do some Exercise, that had almost made him sweat, being careless of himself when he return'd to the Sled again, he was frozen all over, and had so died, had not the Company by Accident taken Notice of him, and by rubbing him over with Snow, and by the use of the like means recover'd him again; but he told the Doctor, that by this whole Accident he was put to no pain, save / that when he came to himself again, he felt such a pricking all his Body over, as men are wont to find in an Arm or Leg benumm'd by having been long lean'd upon.

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When I ask'd whether the sharpness of the Cold, did not work upon the stones, he answer'd, That as to Flints he could not tell, but as to other stones, and such as are oftentimes us'd for Building, the



violence of the Cold made them frequently moulder into Dust. And to satisfy my Curiosity about the Effect of Cold upon Wood, he told me, that he had very often in the night, especially when their keen frosts were unaccompani'd with Snow, heard the Trees cleave and crack with very great, and sometimes frightful noises, and that the outside of the Fir-Trees, that were laid upon one another in their Buildings, and was expos'd to the Air, would do the like, and that he had often seen the gaping Clefts sometimes wide enough to put in his fingers, which would remain in the Trees, and in the Fir-wood, till the thaw, after which they would pretty well close of themselves. /

## Title VII.

### *Experiments touching the Expansion of Water, and Aqueous Liquors by Freezing.*

1. That water and other Liquors are condensed by Cold, and so much the more condensed, by how much the greater the degree of Cold is that condenses them, has been for many ages generally taught by the Schools, and taken for granted among men, till of late some more speculative than the rest, have called it in question upon the account of the levity of Ice, since which I have met with two modern writers, that have incidentally endeavoured to prove, *that Ice is water, not condensed, but rarified* by the intumescence of water exposed to freezing in vessels fitly shap'd. /

These Attempts of these learned Men putting me in mind of what I had tried to this purpose, when I was scarce more than a Boy, invited me to consider, that by the usual ways of Glaciation, such as these ingenious Men employ'd, the Experiment is wont to meet with a Disaster, by the breaking of the Glasses, which not only makes the Event liable to some objections of theirs, that befriend the common Opinion, but (which is more considerable) hinders them from judging what this Expansion of water, that is made by freezing may amount to: wherefore we will now set down what we have done to ascertain (and yet limit) the Experiment, as also to advance it further.

2. Whereas then these two learned Men, we have been mentioning,<sup>†@</sup> do so expose the water to freez, that it is turn'd into Ice at the top as soon as elsewhere; the inconveniences of which way we have already noted, we, by

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freezing the water, as we have formerly taught, from the bottom upwards, can easily preserve our Glasses entire, and yet turn the / whole contained water into Ice; so that if according to this way You so place a Bolthead or a Glass-egg, in whose Cavity the water ascends to the height of an inch, or thereabouts, within the stem or shank, in a mixture of Ice, or snow and salt, as that the water is first turned into ice at the bottom and sides, and not till the very last at the top, you shall manifestly see, that the ice will reach a good way higher in the neck, than the fluid water did, and that upon a gentle thaw of the ice, the water, it returns to, will rest at the same height in the stem, to which it reached, before it was exposed to be frozen.

3. We have likewise used other ways unspoken of by the lately mentioned writers, to evince, that water is expanded by being frozen; as first, that we took a strong earthen vessel of a Cylindrical form, and filling it with water to a certain height, we exposed it unstopped, both to the open Air in frosty nights, and to the operation of snow and salt, and found, that the ice did manifestly reach higher than the water did, before / it was congealed.

Besides, if a hollow Pipe or Cylinder made of some compact matter, be stopped at one end with wax, or some things else, which it may be more easie to drive out, than to burst the Cylinder, and if at the other end it be filled with water, and that orifice also be stopped after the same manner, this Pipe suspended in a sufficiently cold Air, will have the included water frozen, and by that change, if the Experiment have been rightly made, the water will upon congelation take up so much more room than it did before, that the above mentioned stoppels, or at least one of them will be thrust out,

and there will be produced a rod of Ice a good deal longer then the pipe, at each of whose ends (or at least at one of them) a Cylindrical piece of Ice of a pretty length may be broken off, without meddling with the Pipe, or the ice that fills it.

Divers other ways of proving the same Truth might be here alledged, but that, though these were not, as they are, sufficient, the matter would yet / be abundantly confirm'd by divers of the Experiments, that will here and there come in more opportunely in the following parts of this Treatise.

4. But here it will not be altogether impertinent or unseasonable, to take notice, that not only those School Philosophers, who have considered the breaking of well stopt Glasses in frosty weather, (an accident but too frequent in Apothecaries Shops, and Laboratories) but divers modern *Virtuosi*, are wont to ascribe the *Phænomenon* to this, that the Cold of the external Air, contracting the Air and Liquor within, the Ambient Air must break the sides of the Glass to fill that space, which being deserted upon the condensation of the included Air, the liquor would otherwise leave a *vacuum* abhorr'd by nature; and even those few Moderns, that are loath to ascribe this *Phænomenon* to Natures abhorrency of a *vacuum*, either not being acquainted with the weight of the Air, know not, what probable account to give of it, or if they acknowledge that weight, are wont to / ascribe it to that, and to the great contraction of the internal Air, made by the Cold of the External.

5. But as for the Peripateticks, the above mentioned Experiments

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sufficiently evince, that in many cases, 'tis not the shrinking, but the Expansion of the liquors contained in the stopt vessels, that occasions their bursting, and therefore in these cases, we need not, nor cannot fly to I know not what *fuga vacui* for an account of the *Phænomenon*; and whereas it may be objected, that even glasses not half full of distill'd waters, if they be exactly stopt, are often broken by the frost in Apothecaries shops: I answer, That neither in this case do I see any need of having any recourse, either to the *fugavacui*, or to the weight of the external Air, for even here the Expansion of the freezing liquor may serve the turn, for in such inartificial glaciations the liquor begins to freez at the top, and the ice there generated, fastning it self (as on other occasions we declare) very strongly to the sides of the Glass, contiguous to its edg, as / the liquor freezes deeper and deeper, this crust of Ice increases in thickness and strength, so that the water is included, as in a vessel Hermetically sealed betwixt this Ice at the upper part, and the sides and bottom of the Glass every where else, and consequently, the remaining water being incapable of Congelation without Expansion, when the ice is grown strong enough at the top to make it easier for the expansive endeavour of the freezing water to crack the sides or bottom of the Glass, then to force up that thick cake of Ice, the vessel will be broken, how much soever there be of it empty above the surface of the Ice. And this Conjecture may be confirmed by these two Particulars, the one, That when water is frozen in a broad vessel, which is too strong to be broken or stretch'd by the frost, the surface of the ice contiguous to the Air will be convex or protuberant, because that though the glaciation began at the top, the thickness and Compactness of the vessel makes it easier for the expansive endeavour to thrust up that cake of ice in those / parts of it, that are the remoter from the sides, whereunto they are strongly fastned, then to break so solid a vessel.

6. The other Particular is afforded us by that Experiment of ours (mention'd in the Vth. Title foregoing) wherein if a vessel half full of water be made to freez, not first at the top, but at the bottom, that liquor may be turned into ice without danger to the glass. But we will now add an Experiment, on whose occasion we have set down these Considerations. For being inclined to think, that the spring of the Air, shut up in a vessel stopped, will preserve it expanded, or at least keep it from considerably shrinking, notwithstanding a very great degree of Cold, in case the vessel be strong and close enough to fence it from the pressure of the external Air, we conjectured that the bare weight of the outward Air added to the Refrigeration of the included Air, would not be

sufficient to break much weaker glasses, then those we have been speaking of. And therefore partly to satisfy some ingenious / Men, that this Conjecture made me dissent from, and partly to show the Peripateticks, and those that adhere to them in the question under consideration, that either the Cold alone cannot always, as they teach us, contract the Air, or that if it do, the breaking of well stopp'd glasses in frosty weather is much fitter to evince, that there may be a *vacuum*, then that there can be none, we made the following Experiment.

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7. We took three glass-bubbles of differing shapes and sizes, which we caused to be blown with a Lamp, that, to make the Experiment very favourable for our Adversaries, we might have them much thinner, and consequently, weaker then those glasses that are wont to be made use of to keep liquors in, and which notwithstanding are wont to be broken, though they be not full by the frost.

These Bubbles, when the Air was at a convenient temper within, were (as easily they might be) nimbly seal'd up with care, to avoid the heating of the Air in them, and being afterwards expos'd sometimes to the Air it / self in very frosty weather, and sometimes to that greater Cold, which is produced by the placing them in a mixture of snow and salt, we could not nevertheless find, that any one of the three was at all broken or cracked, so that in case the included Air were condensed into a lesser room, the space it deserted may be concluded empty, or else it will hardly appear, what necessity there can be, that Nature should break, as the Peripateticks pretend, very much stronger glasses in Apothecaries shops, to prevent a *vacuum*.

8. Having shown, that water it self, acquires a considerable Expansion by Cold, we will next shew, that Aqueous Bodies, or those that abound with waterish parts, do divers, if not all of them, the like.

We took Eggs, and exposing them to a sufficient Degree of Cold, we observ'd, that when the contain'd liquors were turn'd into Ice, they burst the shells asunder, so that divers gaping Cracks were to be seen in them, as long as they continu'd frozen. /

9. Milk, Urine, Rhenish-wine, and good spirit of Wine, being set to freez in distinct glass Eggs, neither of the three former liquors was observ'd to subside before it began to rise. The Event in sum was, that the Urine was much longer, then either of the two other liquors, before it began to swell, but rose to a far greater height, then they, afterwards. The Wine did not leave the mark above an inch beneath. The Milk ascended about two inches, and the Urine by guess six or seven.

10. A strong solution of *Dantzick* Vitriol, being put into a Cylindrical Pipe, seal'd at one end so, that the liquor fill'd the Pipe to the height of about six or eight inches, being frozen with snow and salt, the congeal'd liquor grew very opacous, and look'd as if it had been turn'd or shot into Vitriol, save a little that remain'd fluid, and transparent near the bottom. And this Ice as appeared, rose considerably higher then the liquor did before Congelation.

It were perhaps worth trying, whether or no even several Bodies of / a stable consistence, and durable Texture, might not be found to receive some, though less manifest Dilatation by excessive Cold. And methinks those, who attribute Glaciation to the plentiful Ingress of frigorifick Atoms into Bodies, should by their *Hypothesis* have been invited to make some Trials of this kind, since we see that the invisible Moisture of the Air against rainy weather, does seem manifestly enough to alter the Dimensions of doors, window-shuts, and other such works made of wood not well season'd. And even without supposing the truth of the *Epicurean Hypothesis*, if we consider, that in Bread,

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though we are sure, that much more water was added to the Meal, or Flower, then was exhal'd in the Oven, yet there appears not the least drop of water distinct in the Concrete, and that Harts-horn, Sponges, and many other Bodies, that seem very dry, will afford by distillation good store of

phlegm or water, and more then can probably be ascrib'd to any transmuting Operation of the Fire; If, I say, we consider / these and the like things, it may seem worth while to try (which I want the conveniency to do) by accurate measures, whether the invisible and interspers'd water, its comminution notwithstanding, will not upon freezing swell the Body that harbours it. And I would the more gladly have been satisfi'd in this, because I hop'd it might help me to unriddle a strange *Phænomenon*, afforded us by the Narrative of the Dutch-mens Voyage to *Nova Zembla*, wherein they relate, *That the Cold was so great, that their Clock was frozen, and would not go, though they hung more weight upon it then before*: So that they were fain to measure their Time by hour-glasses. For though this odd Effect might be suspected to proceed from some little Isicles sticking to some of the Wheels, or the Line, in regard they not far off tell us, that the steams of their Bodies, and other things within their close house, did so fasten themselves to the walls, to the Roof, and even to their Cabins, / as to line them with Ice, of no less then two fingers thick;†@ yet besides, that it cannot be probably suppos'd, that they, who had so great need of their Clock, during the tedious absence of the Sun for many weeks together, should not all the Winter long be aware of this.

Besides this, I say, I find that in Captain *James's* wintering at *Charleton*,†@ his Clock and Watch were so frozen too, *That they could not go, notwithstanding they were still kept by the fire side in a Chest, wrapt in clothes*. So that in case it appear, that according to what we formerly noted out of *Wormius*, the frost can get into Metals, it can also distend them, and other stable Bodies:†\* We might conceive, that the stopping of the Clocks might proceed from the stiffness, or the swelling of the line, to which the weight was fastned, or a swelling even of some of the wheels, or other Metalline parts of the Clock, that may spoil the necessary congruity between the Teeth, &c. as I have tri'd, that some parts of an Iron Instrument, I caus'd to be made, would by no means fit one within another, / when expanded by much Heat, (and though Cold be the cause of the expansion, the Effect may be the same) though at other times they would. And if we knew whether Springs lose any thing of their Elasticity by the violence of the Cold, we might thence also be assisted to guess, whether the frosts Operation upon the Spring of Captain *James's* Watch (for he mentions that, as distinct from his Clock) might contribute any thing to the forcing it to stand still. But these are bare Conjectures, from which I will therefore pass on to the following Section. /

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## Title VIII.

### *Experiments touching the Contraction of Liquors by Cold.*

1. But notwithstanding all the former Experiments, we must not conclude universally, that all liquors are dispos'd to be expanded by Cold, neither by a moderate degree, nor even by so intense a degree of it as suffices to freez or congeal the liquors exposed to it; this we have tri'd, not only in spirit of Wine, *Aqua fortis*, Oyl of Turpentine, and divers other liquors, that we could not bring to freez, but also in oyl congeal'd by the Vehemence of Cold, so that as to the change of Dimensions produc'd in Liquors by Cold, there must be a great difference allowed betwixt water and aqueous liquors on the one side, and oyl and divers other liquors, that are some of them of an / oleaginous, and some of a very spirituous, or a very highly corrosive nature, on the other side. Nor have we yet made trials enough to reduce this matter to a certainty. For though we could not bring some strong Saline spirits, nor the most of Chymical oyls to freez, yet in some our Attempts succeeded not ill. But I remember not, that in any liquor we could by Cold produce any sensible expansion, but rather a manifest Condensation, unless we could bring it actually to freez.

2. The trials we made of the Efficacy of Cold to condense liquors, were many, but it may, for the present, suffice to set down two or three differing ones, that occur to us in our Collections.

To the entry of the Experiment, lately recited, of the expansion of Milk, Urine, and the Rhenish Wine, there are subjoyned these words.



[But the Egg that held the spirit of Wine, though it were much smaller then we usually employ, and fitted with a proportionably slender stem, and though it were kept divers / hours partly in Ice, and Salt, and partly in Snow and Salt, yet it froze not at all, but subsided by degrees below the first mark to the quantity of  $\frac{3}{4}$  of an inch in the stem; and though it afterwards seemed to rise a little, yet it never swelled up again to the said first mark.]

3. [We took a round Bolthead of about  $\frac{1}{2}$  in Diameter, and poured in *Mercury* till it reached a pretty way into the neck, which was purposely drawn more slender then ordinary, and having, without approaching it to the fire, freed it from some of the larger bubbles of Air, that appeared at the sides, we put it into a mixture of Ice and Salt, where the Cold so wrought upon it, that watching it attentively, we could discern not only its having moved, but its motion, downwards, which it continued (though not visibly in the progress, as at the first) till it was subsided in the neck two inches or better, which was far more then could be attributed to the contraction of any sensible Aerial Particles, though they had lost not only the 30. part of their / Dimensions, as we have sometimes observed, of the Air, but had been contracted to a point;

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and we observed too, that the Quicksilver once thus infrigidated, though not frozen, retained some of the acquired Cold, for many hours after, as appeared by its keeping below the mark of its first height, though we had kept it all night in a warm room.]

4. [We took a small Egg with a proportionably slender stem, into which we poured common oyl, till it rose a pretty way (but not much) above the oval part of the glass, then having put a mark upon the station of the liquor, we placed the vessel in snow and salt, and observed it not to swell as other liquors, but to subside, with Cold, till being quite frozen or congeal'd, it appeared to be shrunk about an inch or more beneath the mark, then being thaw'd, it swelled again to the mark.]  $\frac{1}{2}$

5. The Experiment was repeated the second time, with not much worse success, but we found, that if the glass were removed out of the snow into some place near the fire, / the hot Air would not only thaw it, but so rarifie it, as to make it ascend above the mark. A third time we seal'd up the same oyl in the same glass, and repeated the Experiment with like success to that, we had the second time, and that the frozen oyl was really condensed, we found, because it would sink in oyl of the same kind cold, but unfrozen; and this, notwithstanding divers bubbles, which we observed usually to be made about each lump of congeal'd oyl, that we cast in, upon its beginning to sink in the fluid oyl. This we tri'd, both with oyl well congeal'd (or if another word please better, Incrassated or Curled) by snow and salt, and with oyl less congeal'd, frozen by the bare cold of the Ambient Air; but this latter seemed to sight to sink more slowly then the other, as being less congealed and ponderous, yet would not lumps of the mass of oyl sink or continue immersed. I say not in common water, but in Sack or Claret-wine, and if thrust down into either of these liquors, they nimbly enough emerged. /

6. Whether or no Chymical oyls, though, like expressed oyls, they shrink with a moderate degree of Cold, would by congelation be, like them, contracted, or like Aqueous liquors expanded, we could not satisfie our selves by Experiment, because we were unable to advance Cold to a degree capable of bringing such oyls to congelation, only we had thoughts to make a trial with oyl of Aniseeds, distilled with water in a Limbeck, in regard, that though it be a very subtile liquor, and as Chymists call it, an *Essential* oyl, and though in the Summer time, and at some other seasons (if the weather be warm) it will remain fluid, yet in the Winter, when the Air is cold, it will, if it be well drawn, and genuine, easily enough lose its fluidity, and therefore we thought it might do well to pour some of it in moderate weather, into a conveniently shap'd glass, and then to freez it externally by the application of Ice and Salt, that we might observe, whether upon congelation it would shrink or be expanded. And accordingly, though / we were not provided with any Quantity of this oyl, yet in weather that was not sharp, we did by the help of some Ice which we procur'd, when the season made it a Rarity, surround a

glass pipe fill'd with fluid oyl of Aniseeds, and found, though the Pipe were but short, yet the inclosed substance, when it had lost its fluidity, had considerably lost of the height which it reached to before.

7. And because the *Empyreumatical* oyls, that are driven out of Retorts by somewhat violent fires, seem'd to be of a nature differing enough from those *Essential* oyls (as Artists call them) which are drawn in Limbecks by the help of water, as well as fire: And because we observ'd, that some of the firmer oyls may be us'd in Physick in much larger Doses, then 'tis thought safe to give the latter in: Conjecturing from hence, that probably Empyreumatical oyls may be less hot, and so less indispos'd to Congelation, we thought fit to make trial (no body else in probability having done it) whether the Cold in our Climate / could be brought to freez these oyls, and whether it would expand or condense them; wherefore exposing, in conveniently shap'd vessels, some good oyl of *Guajacum*, that was diaphanous enough, though very highly colour'd, to the greatest Cold we could produce, we attempted, but in vain, to deprive it of its fluidity. All that we were able to effect, being to make it very manifestly shrink. /

## Title IX.

*Experiments in Consort, Touching the Bubbles from which the Levity of Ice is supposed to proceed.*

1. Since the first thing that made the Moderns suspect, that water is expanded by freezing, is the floating of Ice upon water, it will not be amiss for confirmation of that Argument, to take some notice of the levity of Ice in respect of water; This is best observed in great Quantities of Ice, for whereas in small fragments or plates, the Ice, though it sink not to the bottom of the water, will oftentimes sink so low in it, as scarce to leave any part evidently extant above the surface of the water, in vast quantities of Ice, that extancy is sometimes so conspicuous, that / Navigators in their Voyages to *Island*, *Greenland*, and other frozen Regions, complain of meeting with lumps, or rather floating rocks of Ice, as high as their main Masts. And if we should meet with Cases, wherein we might safely suppose the Ice to be as solid as entire pieces of Ice are wont to be with us, and not to be made up of icy fragments cemented together, with the interception of considerable Cavities filled with Air, it would not be difficult for any that understands *Hydrostaticks* to give a pretty near guess at the height of the Extant part, by the help of what we lately observ'd of the Measures of water's Expansion, and by the knowledge of the immersed part; which, supposing that the Ice were of a prismatical figure, and floated in an erected posture, would in fresh water amount to about eight or nine times the length of the part of the Prisme superior to the surface of the water.

2. But because perhaps the great disparity in the degrees of Cold, whereby water is in this, and in those / gelid Climates turn'd into Ice, may breed a difference in the expansion of the frozen water, and because some other circumstances may be needful to be taken into consideration, about the height of floating Ice above water, and these will be more properly taken notice of under the following Title, I shall only upon this head (*of the Levity of Ice*) subjoyn the ensuing transcript of one of our notes concerning That subject.

[We found, that pieces of Ice, clear and free, for ought the Eye could take notice of, from bubbles, would not be made to sink in spirit of Wine once distilled from Brandy, and it floated likewise in strong spirit of Wine drawn from quick Lime; but if the spirit of Wine were well warmed, such Ice, as I mentioned, would sink in it, though as it grew cold the same Ice would slowly ascend, and sometimes remain for a while, as if it were suspended without sensibly rising or falling. But all this while the Ice, thawed apace in the water whereinto it was dissolved, did manifestly seem to run down like / a stream through the lighter body of the spirit of Wine, the Diversity of the Refractions

making this easie to be taken notice of; yet common water, though heated as hot as I could indure to hold the glass in my hand, would not let the fragments of the same parcel of Ice sink into it: but in oyl of Turpentine, and in thrice Rectifi'd spirit of Wine, the Ice would sink like a stone.]\*†

3. That the levity of Ice in respect of water proceeds from the bubbles that are produc'd in it, and make the water, when congeal'd, take up more room then when fluid, has scarce been doubted by any, that has consider'd the Texture of Ice, as well as taken notice of its levity. But if this be the true and only reason, we may conjecture, that there must be great store of bubbles in Ice, extremely minute, and undiscern'd by the naked Eye. For though in very many parcels of Ice, the bubbles are as well conspicuous as numerous, insomuch that they render the Ice whitish and opacous, yet we have observed, that other pieces would swim, / which yet were of an almost crystalline clearness. And therefore we thought fit to look upon some clear pieces of Ice in a Microscope, and we shall subjoyn the Event, because that when we beheld some of this ice in one of our Microscopes, which has been counted by several of the curious, as good a Magnifier, as perhaps any is in the world, we could not discover such store of bubbles, as it seemed there should appear upon the supposition, that the adequate cause of the levity and expansion of frozen water is but the interspersion of such bubbles.

The Observations I have been mentioning, I find thus set down among my Notes.

[A piece of Ice, that to the Eye look'd clear like crystal, being put into the great Microscope, appear'd even there free from bubbles, and yet the same piece of Ice being presently mov'd, and cast into common water, would swim at the top, and if it were forcibly duck'd, would swiftly enough emerge.

Another piece of Ice, that to the / naked Eye was not so clear as the

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former, appear'd in the same Microscope to have store of bubbles, some of them appearing there no bigger then a small pins head, and some of them being yet lesser, and scarcely visible in the Microscope it self.]

And here, because it seems a considerable doubt, and well worth the examining, whether or no water, when frozen into Ice, grows heavier or lighter, not in reference to such water as it was generated of (since it is evident, that upon that it will float) but more absolutely speaking, we judg'd it not amiss to exmaine this matter by an Experiment, but we could not discover any difference between the weight of the same parcel of water fluid and frozen, as will appear by the ninth

*Paragraph* of the Experiment to be a little beneath recited.]\*†

But since that, whether or no we allow any other cause, together with the bubbles, to the levity of Ice, it seems a thing not to be doubted, that its expansion and lightness is mainly, if not only, due to the interspersion of / bubbles, the generation of them seems to be one of the considerablest *Phænomena* of Cold, and the Investigating by what cause those cavities are produced, and in case they be perfectly full, what substance 'tis that fills them, is none of the meanest enquiries, that should exercise the industry of a searcher into the Nature of Cold.

4. Mr. *Hobs*, and some others seem to think, that the expansion of water by congelation, is caus'd by the Intrusion of Air, which constitutes those numerous bubbles wont to be observ'd in Ice;]\*† we might here demand, why in case that upon freezing there must be a considerable accession of Air from without, when oyl is frozen, it is, notwithstanding the ingress of this Air, not expanded, but condens'd; but because these conjecturers do not allow glass to be pervious to common Air, we shall at present press them with this Experiment, which we have divers times made.

We took a glass-Egg with a long stem, and filling it almost with water, we seal'd it *Hermetically* up to / exclude the pretence that some adventitious Air might get in, and insinuate it self into the water, and yet such an Egg being exposed to congelation, the frozen water would be manifestly expanded, and swell'd by numerous bubbles, which oftentimes gave it a whitish opacity.

To which we may add, that new metalline vessels being fill'd with water, and carefully stopp'd, the liquor would nevertheless, when exposed to the Cold, be thereby expanded, and turned into Ice furnished with bubbles.

5. If it be objected, that in the Experiment of the *Hermetically* seal'd glass, the produced bubbles might come from the Air, which being seal'd up together with the water, might by the expansion of that water be brought to mingle with it: I answer, that this is very improbable. For 1. if the bubbles must cause the expansion of the water, how shall the water be at first expanded to reduce the Air to a Division into bubbles. Next, 'tis evident by the Experiments we shall ere long relate, that / the Air as to the Body of it,

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retains its station above the water, and preserves it self together in one parcel, since it suffers a compression, that oftentimes makes it break the glass that imprisons it, which it would not need to do, in case it dispers'd it self into the Body of the water; for then there would appear no cause, why the Air and water should after congelation require more room then they did before. 3. In this Experiment we usually begin to produce Ice and bubbles in the water, contiguous to the bottom of the vessel (that part being by the snow and salt first refrigerated) in which case there appears no reason, why the Air, which is a *thousand times* lighter then the water, should against its nature dive to the bottom of the water, and if it were disposed to dive, why should we not see it break through the water in bubbles, as is usual in other cases, where Air penetrates water. 4. In metalline vessels, and in Glasses quite filled with water, before they are stopped, there is no pretence of the diving of the Air from the top, / there having been none left there. 5. and lastly, If all the bubbles of Ice were made *by*, and filled with true Air descending from the upper parts of the vessels, and only dispersed through the water, then, upon the thawing of this Ice, the Air would emerge, and we might recover as much of real Air as would fill the space acquired by the water upon the account of its being turned into Ice, which is contrary to our Experience. And this Argument may also be urged against any that should pretend, (for I expect not to see him prove it) that though Air, as numerous experiments evince, cannot get out of a seal'd glass, yet it may, in such a case as this, get into it. But we find upon trials, that the Cavities of these bubbles are not any thing near filled with Air, if they have in them any more Air at all, then that little which is wont, as we have elsewhere shewn, to lurk in the particles of water, and other liquors. And the making good of this leads us to the second Enquiry, we were proposing about these bubbles, namely, whether or / no their cavities be fill'd, and fill'd with Air.

6. The full resolution of this whole Difficulty would be no easie Matter, nor well to be dispatched with so much brevity as my occasions exact. For it would require satisfactory Answers, to more then one or two Questions, since, for ought I know, it may lead us to the debate of those two grand Queries, whether or no Nature admit a *Vacuum*, and whether a great part of the Universe consist of a certain Ethereal matter, subtile enough to pass through the pores, not only of liquors, but of compact bodies, and even of glass it self: we should also be obliged to enquire, whether or no Air, I mean true and permanent Air, can be generated anew, as well out of common water, as many other liquors, and whether it may be generated by Cold it self, and perhaps we should be oblig'd to inquire into the *Modus* of this production, and engage our selves in divers other difficulties, whose full Prosecution, besides that they would as much exceed our present / leisure, as Abilities, seems more properly to belong to the more general part of Physicks, where such kind of general Questions are fittest to be handled.



Wherefore we will now only consider this Particular Question, whether or no the Cavities of the Bubbles wont to abound in Ice, be *filled* with

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common Air; and even this question, though it seem but one, comprizes two: for to resolve it, we must determine, whether there be any *true Air* contained in those Cavities, and whether in case there be, they be adequately filled with that Air, (by *true Air* I mean such an invisible fluid, as does permanently retain a spring like the common Air.)

7. The former of these two Questions, I must confess my self not yet resolved about, my Experiments having not hitherto succeeded uniformly enough to satisfie so jealous an observer. But yet I shall annex our trials, *not only* because the thing has not been, that we know of, so much as attempted by others, and our ways of Experimenting, if they be duly prosecuted, / seem as promising and hopeful (if the Question be reducible to any certain Decision) as perhaps will be easily lighted on; but because *also* we have, if we mistake not, resolved the second Question, by shewing that there is but a small part of true Air contained in the Bubbles of Ice, whatever Ingenious men, that rely upon probable Conjectures without consulting Experience, have been pleas'd to believe to the contrary.

That the bubbles observed in Ice cannot all be filled with the Aerial particles lurking in the water, seems evident enough by the expansion of the water, and the Quantity of space taken up by those bubbles, which how the interspers'd, and formerly latitant Air can adequately *fill*, unless the same parcel of Matter could truly *fill* much more space at one time then at another (which I take to be physically impossible) I do not yet apprehend.

But two ways of trial there are, which we imployed to shew, that the Icy bubbles are nothing near *filled with true Air*, whether Men will have / that pre-existent in the water, or stollen in from without, or generated anew; the former of the two ways of trials probably arguing, that these bubbles proceed not *only* (for that they may proceed *partly* we do not at all deny) from the Air pre-existent in the water, and the latter concluding more generally, that but a small part of the icy bubbles are filled with genuine Air.

8. And 1. we were invited to conjecture, both, that sometimes, or in some cases, the Air latitant in the water might contribute to generate icy bubbles, though it was unable adequately to fill them; and again, that sometimes or in other cases such bubbles would be almost as numerously generated, notwithstanding the recess of far the greatest part of that latitant Air, by the three following Experiments taken *verbatim* out of our Collections.

I. We took fair water, and having kept it in the exhausted Receiver of our Pneumatical Engine for a good while, till we perceived it not to send up any more bubbles, we presently / transferred it into snow and salt, where it was long enough before it began to freez, and then we observed, that the water did not swell so much as common water is wont to do, and the ice seemed to have few or no bubbles worth taking notice of: but when I afterwards placed it between my Eye and the vigorous flame of a Candle, I could perceive, that it was not quite destitute of bubbles, though they were extremely small, in comparison of those, that would probably have appeared in ordinary water.

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Thus far the first Experiment; the second follows, which was made at another time.

II. The water that had been freed from the bubbles in the Receiver, though it afforded an ice, that seem'd to have smaller bubbles, yet this ice being thaw'd, part of the water was gently poured into a pipe of glass, wherein being frozen, it swell'd considerably enough above its first level, and besides burst the glass, being also very opacous by reason of the bubbles. /

The third Experiment was more industriously prosecuted, as may appear by this ample Narrative of it, transcribed out of our Collections.

III. We took a small Egg with a pretty long neck, and pouring in water till it reach'd an inch within the stem, conveyed it into a long slender Cylindrical Receiver, provided on purpose to make trials with such tall glasses, the Air being by degrees drawn out of the bubbles appeared from time to time greater and greater, and when the Receiver was well exhausted, the water seemed to boil a longer time than one would have expected, and sometimes the bubbles ascended so fast and great, that we were in doubt, whether the water did not boil over the top of the Pipe: the exhausted Receiver was permitted to be so for a good while, till the water had discharged it self in bubbles of its Air, and then the glass-Egg was removed into a vessel furnished with ice and salt, and there left ten or twelve hours, that all the water, save that in the neck, might be thoroughly frozen, and then we found / it to have risen a great way above its first height, and removing it into an Air temper'd like that wherein the first part of the Experiment was made, & having left it there in a quiet place for ten or twelve hours to thaw leisurely (lest too warm an Air, or too much stirring the glass might be an occasion of generating new bubbles,) in the exterior part of the ice near the glass, we saw pretty store of bubbles, but when that was thaw'd, the rest of the ice appeared of a peculiar and unusual texture, having no determinate bubbles, that I could easily distinguish, but seeming almost like a piece of frosted glass, where the Parts, that made the Asperity, were exceeding thick set, but this ice swam in the water, whereinto the rest had been dissolved before it was all thawed: when there yet remained a lump about the bigness of a small Walnut, we reconveyed it into the Receiver, to try whether upon the exuction of the Air, the ice would be presently melted, but the alteration produced, was so small, if any, that we durst not ground any / thing upon it. The Receiver being exhausted, there did at length appear some bubbles in the water, but they were not numerous, and a hundred of them seem'd not to amount to one of those larger ones, the same water had yielded us the first time it was put in: in the ice also some small bubbles disclosed themselves, which we did not perceive there before, wherefore we took out the Egg, and found (the ice being now thaw'd) that the water was subsided to the mark we had made, before it was expos'd to congelation, if not some very little way beneath it: Then we went about to find the Proportion wherein this dispirited water was expanded by glaciation, but in pursuing this there hapned a mischance to the glass, which kept the Experiment from being so accurate as

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we designed,<sup>†\*</sup> And therefore, though it seemed to us, that it amounted to about the twelfth part, which is less then that of the undispirited water, yet we designed the repetition of the Experiment. Only in this we could not be mistaken, that the expansion was considerable, since / the water rose three inches and a half in the stem, though the whole water in the Egg and stem too, weighed but two ounces and a half. If the vessel had not been unluckily cracked, we should have frozen the water once more, and then sealing up the glass Hermetically, and suffering the ice leisurely the thaw, should have inverted it, and broken it under water, and have proceeded with it as we had done with some other glasses in the formerly mentioned Experiments.

9. [A little glass Cylinder open only at one end, of a convenient length, was thrust into a deep and wide mouth'd-glass about half filled with a mixture of Ice and salt: but the Cylinder was neither so quite filled, that the water should run over, nor yet far short of being so; that, (for all the opacous mixture of Ice and Salt) we might guess at the freezing of that part of the water, that we could not see by the changes appearing in the other. Then conveying all into a Receiver, that we had in readiness beforehand, we quickly pumped out the Air, upon / which there came both from the upper & lower parts of the water, great store of Bubbles to the top, where most of them brake into the Receiver, having found upon trials purposely made, that the Engine had continued stanch all the while, and perceiving by the intumescence of the superior parts of the water, that the other were frozen, we let in the external Air, and having removed the Receiver, and taken out the mixture before the Ice was half melted, we found the water, as high as the mixture reached, to be turn'd into

ice, which besides some large and conspicuous bubbles had small ones enough to render it opacous; and upon the account of this expansion it was, that the water did in the free Air continue a good deal higher then the mark, it was but level with, when the Cylinder was exposed to freez.]

10. The other way we employ'd to examine what was contained in icy bubbles, and which seemed clearly enough to manifest, that they are very far from being filled with true and springy Air, is intimated in the / last clause of the foregoing narrative, but will be best understood by the annexed Experiments transcribed just as I find them registred in my Collections: and though they be prolix, and contain some few Particulars, that make not directly for the purpose I alledge them for, yet I think not fit to dismember or to epitomize them, or otherwise to alter any thing in them, *partly*, that the inference I make from them, may be the less mistrusted, *partly*, because the way of Experimenting being altogether new, will be best apprehended by the subjoyned Examples, and *partly* too, because those Particulars that relate not directly to the occasion of our mentioning these trials, may be useful to illustrate or confirm some thing that is already delivered, or is hereafter to be delivered in the present *History of Cold*.

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11. [We took this day<sup>†@</sup> a glass of the form of an Egg, but of double the capacity, out of whose obtuse end rose up a long Cylindrical neck, capable to receive the end of my little finger, and no more, this being fill'd / with common water, till the liquor reached a pretty way within the pipe, and the surface of the water being carefully marked on the outside, was placed in a vessel, wherein ice very grossly beaten, was mingled with a convenient Proportion of salt (according to our way of Glaciation) the Mixture not reaching up to the mark by above an inch. The Experiment afforded us these Particulars.

I. A heedful Eye did not perceive the water sensibly to subside before it began to freez.

II. The water began to swell, and some parts of it next the side or bottom of the glass, to freez within a quarter of an hour.

III. The ascent of the water in the pipe increased so fast, that within an hour, from the time the glass was put in, it did rise four inches<sup>†\*</sup> above the mark, & afterwards the swelling connutied so, that we took it out, though a good part of the water remain'd unfrozen, it had reach'd five inches and somewhat more then a half above the first Mark.

IV. The ice and salt being purposely / kept always beneath the surface of the water, the lower parts of the water were frozen, and never the upper surface.

V. During all this great Elevation of the water, there appeared no bubbles worth taking notice of in the unfrozen parts of the liquor, but the ice was very full of them, divers of which toward the latter end of the Experiment were very large Bubbles (but not all of them round) some being about the bigness of hail shot, some small like Mustard seed, and others again not much inferior to little pease.

VI. Having taken out the glass, when the water was at the highest mark, we did upon a certain design, pour in as much sallet Oyl as swam about two inches above it, and then the glass was nimblely at the flame of a Lamp seal'd up, during which time the included water subsided a little, but the glass being again put into the ice and salt, the Cold quickly restored the water to its former height, and there remained about an inch and a half of the seal'd glass unpossessed / by the two contain'd liquors.

VII. Then with a good pair of scales we weigh'd the glass-Egg first in the Air, and then in the water (the better to discern, whether any shrinking of the glass interven'd in the case,) where it hung

freely, and was left hanging on its *Equilibrium* with its opposite weight.

VIII. Whilest it thus hung, upon the thawing of the ice many bubbles, great and small ascended (the great ones with a wrigling motion) and vanish'd at the top.

IX. As the ice thaw'd, the water and oyl descended, till the whole ice was return'd to water, at which time we observ'd these two remarkable things,

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the one, That the *Equilibrium* remain'd the same; the other, (which was more considerable) that the water was subsided again as low as the first mark, with which it was level before it began to swell, without falling beneath it, notwithstanding the recess of such a multitude of Bubbles, divers of which were very large.

X. The glass being inverted, the / seal'd end, which was drawn slender, was gently broken under water, of which some, being impell'd in, did sensibly reduce the Air at the opposite end into a narrower room; and, as one of the spectators observ'd, into a much narrower, which is consonant enough to reason.

XI. The glass being again inverted, and held till it was settled, we found, that the water drawn in together with the water it found there, and the oyl, possess'd the same places, (as appeared by the marks in the Cavity of the Receiver,) that they did, when it was seal'd up.

XII. And lastly, having thrown out the oyl, and employing, where need was, a little water of the same kind we had made use of all this while, we found the glass fill'd to the highest mark, to weigh 4374. grains, when it was fill'd but to the lowest mark, 4152. grains, and when quite empty'd 1032. So that the water contain'd betwixt the highest and lowest mark, and rais'd by the Glaciation, was about a fifteenth part of the water set to freez, and / probably would have amounted to much more, if the water had been all frozen.]

12. [A large glass-Egg being taken with a proportionably big stem,<sup>†@</sup> we poured water into it, till it reached about an inch above the bottom of the stem, and fastning a mark there, we exposed it all night to freez in snow and salt, which was so placed, as not to reach so high as the bottom of the stem; the next day about ten of the clock we found the water risen in the stem about 15. inches above the mark, the whole Cylinder of water being fluid by reason of the snows not reaching to it. (Then upon a design to be elsewhere mentioned, we seal'd up the glass by a very slender pipe, that had been before purposely drawn out to a pretty distance from the body of the Cylinder, that the glass might be seal'd, in a trice before the flame of a Candle could sensibly rarifie the Air, and after a while we broke off the Apex of this slender pipe in prosecution of our former Design.) Then suffering the water to swell freely, within seven / or eight hours it reach'd the very top of the glass, a drop or two running over at the slender Orifice thereof, so that in all, the water ascended about 19. inches above the first mark: then we tried by the flame of a candle to seal the glass, but by reason of the Rarefaction of some of the water, by the Heat, into vapours, by which some of the other water was, from time to time, spurted against the flame of the Candle, we found it troublesome enough to seal it up, the vessel being removed into a warm place, till next morning, and all the ice in the belly of it (for the water in the stem continued fluid) being thawed, the water subsided, not only to its first mark, but a little beneath it, by reason of that which was thrown out, upon occasion of the sealing of the

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glass: but when we came to invert this, after the manner above mention'd, into a vessel of water, to see how much of the space deserted by the thaw'd Ice, was fill'd with Air, and how much was fill'd



with a subtiler substance, or empty, just then a mischance frustrated our Expectation.]†@/

13. [An Egg about the same bigness with the former,†@ was placed to freez in beaten ice and salt, and in less then a quarter of an hour, it was risen near an inch above the Mark, where the surface of the water was at the first, and the water in the ball and the joyning of the neck was frozen into *Laminae*. After an hour and a quarter, those *Laminae*, that before appeared in the beginning of the neck, now disappear'd, but the ball seem'd frozen into a white ice, and the water in the neck was risen above the first mark four inches and a half. There now appear'd abundance of small bubbles, continually ascending through the neck (which so continu'd all the time after, till it was quite thaw'd) and the white ice appear'd full of bubbles. The Experiment being further pursu'd, the water ascended higher and higher, till it had reach'd about eight inches above the first mark: Then the top of the pipe, being with a Lamp drawn out, into a very slender Cylinder (for the conveniency of sealing up) the glass was again put into the ice, that the Air / heated by the Lamp might cool, upon which the water continued swelling, till it began to run over at the orifice of the slender pipe, which being held by in the flame of a candle, was in a trice seal'd up, so that the whole glass now appear'd full of water, bating an inconsiderable Quantity of rarifi'd Air, (not amounting to the bigness of half a small Pea) that remain'd contiguous to the seal'd part; the Egg being brought into a warm room, was kept there all night, and a good part of the next morning, before the ice was quite thaw'd, which when it was, the water was found subsided to the first mark, and which being done, the glass was inverted, and the seal'd end immers'd a good way under water, where being broken, the external Air impell'd the water in the Bason into the Cavity of the pipe, insomuch, that when we took it out, which we did, as soon as we thought no more water was impell'd up, re-inverting the glass, we found, that the admitted water reach'd seven inches above the first mark, and left / an inch and a half of the stem, before it began to be wire-drawn, besides as much of the slender part of the stem, as by guess amounted to a quarter of an inch or more, so that it seem'd, that the Bubbles, which made the water swell, and appear'd in the Ice, amounted to an inch and three quarters of Air, which consequently seem'd to be for the most part generated by this operation, and to seven inches either of a *vacuum*, or some subtile substance, which by its having no spring to resist the Pressure of the outward Air, appear'd not to be Air: We could not exactly measure the Quantity of water we had in all, and the proportion of it betwixt the marks, because having left the glass in the window, to try whether time or Cold would make the admitted water

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shrink (which we did not find it to do) the weather was so sharp, that beginning (as we concluded) to freez the water in the stem, the increasing ice burst out the belly of the glass into many pieces.]

*Another time.*

14. [A seal'd glass being broken / under water, there was impell'd into the Cylinder ten inches and a little above a half. And the mark, it should have risen to, was eleven inches and a quarter above the first and lowest mark.]

*Another time.*†@

15. [In the same Bolthead, wherein the greatest condensation of the Air was tri'd, the water was by the Cold made to swell very near a foot above the mark it rested at, when it began to freez; then the glass being nip'd up, the contain'd water was removed, and suffered leisurely to thaw, and upon the Dissolution of the ice, the water fell back to the former mark: lastly, the glass being inverted, the Apex was broken off under water, and the water in the stem was by the outward Air, pressing upon the water in the Bason, with some *Impetus* and noise driven up into the Cavity of the glass; and, the glass being seasonably and warily remov'd from the Bason, we found there had been impell'd up of the water in the Bason, a little more then eleven inches, so that there seem'd to be near / 7/8 of an inch of Air generated or separated by the former operation.]

16. [In the same glass we made the water to swell about ten inches, and inverting the stem, and breaking the Neb under water, we found about ten inches of water to have been impell'd into the stem; so that in this there seem'd no generation of Air.]

17. To all these Experiments we shall subjoyn, in two words, that as in water, so in some aqueous liquors we found, that the icy Bubbles were not fill'd with Air (though we did not think fit to take the pains to measure their respective Expansions by being congeal'd:) For in that elsewhere mention'd Experiment, where we expos'd Milk, Urine, and Rhenish-wine to freez, when all those liquors were risen above their former marks, as is there related, our Notes inform us, that the Experiment was thus prosecuted.

18. [Being seal'd up (the foregoing words mention'd the above-named expanded liquors) and suffer'd to thaw, the several liquors subsided / to their first marks or thereabouts, and the glasses being inverted and broken under water, we were by an accident hindred from observing what we desir'd in that which had the Wine, though when it was taken out of the freezing pot, it had ice, but not much, swimming in it. But into the glass that had the Milk, the water was manifestly impell'd by the outward Air, and so it was into the glass that had the Urine, which being remov'd from the Bason, and re-inverted, appear'd to have as much new liquor in its stem, as amounted by guess to five or six inches.]

19. To which Experiment we may add, that another time a seal'd glass of

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partly frozen Claret-wine being broken under water, the water was impell'd up between half an inch, and an inch above the mark, beyond which it would not have ascended, if the bubbles had been full of true and permanent Air.

20. If it be said, that though I have delivered too many Particulars about so empty and slight a Theme as Bubbles, I have this to answer, that / possibly all these Experiments have rather shew'd us, what it is not that fills them, then what it is, so that more then all these Experiments appearing requisite to clear up the Difficulties about them, I shall not think I have altogether mis-spent my time, especially if so many past Experiments, both new, and not altogether impertinent, by their not having taught us enough about so despicable a subject as a Bubble, shall, as they justly may teach us Humility. /

## Title X.

### *Experiments about the Measure of the Expansion and the Contraction of Liquors by Cold.*

1. To the Experiments (mention'd in the Seventh and Ninth Titles) which shew, that water has an Expansion, it will be proper to subjoyn some of those, whereby we endeavoured to measure that Expansion. And here we shall not content our selves to say, that whereas the Authors, we had formerly occasion to point at,<sup>†</sup>\* take notice of their having raised water in a Bolthead half an inch or an inch by freezing, we have made it ascend a foot and a half and more; This, I say, we shall pass by, because that though by such Experiments we have very clearly and undeniably manifested the Expansion / of the water, yet unless the Capacity of the vessel be known, they will signifie but little towards the determining the *Quantity* of that Expansion, which yet is the thing we are now enquiring after, wherefore we shall add, that we employ'd two differing ways to *measure* this Expansion.

2. The one was, by putting in, by weight, such a number of ounces of water, into a Bolthead, till the

water was risen a pretty way in the long stem, wherewith it was filled, then marking on the outside, to what height every freshly added ounce of water reach'd in the stem, we afterwards poured out a convenient Quantity of the liquor (yet leaving enough to fill the whole cavity of the spherical or obtuse end of the vessel, and of the lower part of the stem) then leisurely freezing this remaining water from the bottom upwards, we observed, that when it was frozen, the ice that was made of 82. parts of water, filled, as one of our Notes informs us, the space of 91. and (if I mistake not the Character) an eight, so that by / this troublesome way of Examination, we found that the water by the Expansion, it received from Cold, was made to possess about a ninth part more space then it did before congelation.

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3. [In another of our notes, we find as follows, 55. parts of water extended themselves by freezing into sixty and a half, about six of those parts remaining unfrozen, so that in this Experiment the waters Expansion was not much (though somewhat) differing from what it was in that last mention'd.]<sup>†\*</sup>

4. The other way we made use of to measure the Dimensions, that water gains by freezing, was, to take a Cylindrical pipe of glass seal'd at one end, and left open at the other, at which we fill'd it with water to a certain height, that we took notice of by a mark appli'd to the outside, and then keeping it in an erected posture, and freezing it from the bottom upwards, we found, that it had acquir'd by a tenth part or thereabouts, greater Dimensions in the form of ice, then it possessed in the form of water. / But the nature of the particular parcel of liquor exposed to the Cold (for it is not necessary that all waters should be equally dispos'd to be expanded by freezing) and some other circumstances, not now to be discours'd of, may well beget some little variety in the success of this sort of trials. For in one that we made carefully, we found the Expansion somewhat greater, then that last mentioned, as may appear by the following Note, which compar'd with what was lately delivered, of the trials we made by weight of the water's Expansion, may invite us to think, that we cannot much err by estimating in general, that the room that Ice takes up more then water, amounts to about a ninth part of the space possessed by the same water, before it was turned into Ice. The note we were speaking of, is this.

5. [In a more then ordinarily even Cylindrical glass, we exposed some water to freez, to measure its Intumescence, and found that it expanded its self to about an eighth part, or at least a ninth upon glaciation; / this we tri'd twice, and thought that the Intumescence might have been more considerable, but that in a Cylinder the freezing did not seem to succeed so well.]

But here we must resolve a difficulty, which though ordinary Readers may take no notice of, yet may breed a scruple in the minds of those that are acquainted with *Hydrostaticks*. For to such Readers this Account of ours may seem to be contrary to the Experience of Navigators into cold Climates, who tell us (as we shall have occasion to take notice in due place) of vast pieces of Ice, as high, not only as the Poops of their Ships, but as the Masts of them; and yet the Depth of these stupendious pieces of Ice, seems not at all Answerable to what it may be suppos'd to be, in case we compare together the Estimate above deliver'd of the Expansion of water, and that grand *Hydrostatical Theorem* demonstrated by *Archimedes* and *Stevinus*,<sup>†\*</sup> *That floating Bodies will so far, and but so far, sink in the Liquor that supports them, till the immersed part of the Body be equal to a Bulk of water, / weighing as much as the whole Body.* For Captain *James* in his often cited Voyage, makes mention of great pieces of Ice, that were twice as high as the Top-mast-head of his Ship.<sup>†\*</sup>

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6. And the *Hollanders* in their famous Voyage to *Nova Zembla*,<sup>†\*</sup> mention one stupendious Hill of Ice, which I therefore take notice of here, not only because it has been thought the greatest that men

have met with, but because they deliver its Dimensions, not as Captain *James* and Navigators are wont to do, by comparison with the unknown heights of some of the Masts of their Ships, but by certain and determinate Measures, which in the Icy Island, we are speaking of, were so divided by the surface of the water, that there was 16. fathome extant above it, though there were but 36. beneath it, which though a vast depth in it self, yet does but little exceed double the height.

And the *Danish* Navigator *Janus Munckius*, imploy'd by his King to bring him an Account of *Greenland*,<sup>†@</sup> mentions some floating pieces of Ice, / that he met with and observ'd in that Sea, which though but somewhat above 40. fathome under water, were extant 20. fathome, that is (near half as much) above water, whereas it seems, that according to our above mention'd Computation of the Expansion of water, the part under the water ought to be eight or nine times as deep, as that above the water is high.

7. To clear this difficulty, I shall represent these three particulars.

First, that in our Computation the Ice that sinks so deep, is suppos'd to float in fresh water, whereas in the Observations of the above nam'd Navigators, those vast pieces of Ice floated on the Sea-water, which by reason of its saltness, being heavier then fresh-water, Ice will not sink so deep into that, as into this. And that salt may hugely increase the weight of the water, wherein it is dissolv'd, may be clearly gather'd from the ponderousness of common Brine, and from the practise of several sorts of Tradesmen, who to examine the strength of their *Lixiviums*, / and other Saline Liquors are wont to try, whether they will keep an Egg floating, which we know common water will not do. And I have also by the Resolution of some Metalline Bodies in fit *Menstruums* made Liquors, that are yet much more ponderous, then is sufficient for the support of Eggs.

But yet we must be so candid, as to take notice of what some Modern Geographers deliver with probability enough, namely, That nearer the poles the Seas are not wont to be so salt, as in the temperate and the Torrid Zones, and those Northern being not so salt as our Seas, there is the less to be allow'd for the difference in gravity (and consequently in the power to keep Ice from sinking) betwixt those Seas and ours.

8. But secondly, this lesser saltness of the water in the Northern Seas, may, as to our case be recompenc'd by the greater coldness of it. For though, as we have formerly observed, the Condensation of fresh water, effected here by a degree of Cold capable to make it begin to freez, is / not so great as most men would imagine; yet besides that, I have often taken pleasure to make the same Body to sink or ascend in the same water, by a much less variation of

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Cold then that we have been mentioning; it is to be consider'd, that the degree of Cold, to which water was brought in the Experiment deliver'd in the fourth Section, to which we are now looking back, was but such a degree as would make fresh water begin to freez; whereas the salt Sea-water, being indispos'd to congelation, may by so vehement a Cold as reigns in the Winter season in those gelid Climates, be far more intently refrigerated, and thereby more condens'd then common water is here, by such a measure of Cold, as may begin to freez small portions of it. But though, what we have hitherto represented, may well be look'd upon as not inconsiderable to the purpose for which it has been alledg'd, yet the main thing, that is to remove the scruple suggested by the height of Icy hills above the water, is,

9. Thirdly, that such Hills of Ice / are not to be look'd upon as intire and solid ones, but as vast piles or lumps, and masses of Ice, casually and rudely heap'd up and cemented by the excessive Cold, freezing them together by the intervention of the water that washes them, which piles of many pieces of Ice are not made without great Cavities intercepted, and fill'd only with Air, between the more solid Cakes or Lumps; so that the weight of these stupendious pieces of Ice, is not to be



estimated by the bigness they appear of at a distance from the Eye, but considering how much Air there is intercepted between the Icy Bodies, of which they are compiled, there may be a hollow structure of Ice reaching high into the Air, and yet the whole Aggregate or Icy pile, will press the subjacent water on which it leans, no more then would as much water, as were equal in Bulk only to the immers'd parts; as we see in Barges loaden with Boards, which though pil'd up to a great height above the water, make not the vessel to sink more then a Lading that would make / a far less show, and oftentimes be all contain'd within the Cavity of the vessel, provided it be more ponderous *in specie*. But to enter into any further Consideration of these *Hydrostatical* matters, would be improper in this place, especially since we have elsewhere treated of them.<sup>†@</sup> And that these floating Hills and Islands of Ice are not intire and solid pieces of it, we shall elsewhere have occasion to shew out of Navigators, and even in the Observation, we have mentioned out of *Janus Munck*, the Learned Relator of it *Bartholinus*, takes notice, that those vast pieces of Ice (we have been mentioning) that reach'd 20. fathome above water, were *compiled of store of Snow frozen together*.<sup>†@</sup>

10. These Considerations may serve to render some Account of those stupendiously tall pieces of ice, whose extant part bears so great a proportion to the immersed part, when the whole mass does really float. But I confess I doubt, that not only in the Examples we have alledg'd, but in other eminent ones of / mountains of ice, if I may so call them, there may be a mistake, and that the height of them above the water, would be far less, and the depth under water far greater, if the ice had water enough to swim freely. For Sea-men by reason of the difficulty, are not wont to measure the height of those

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pieces that float at liberty in the Sea. And as for those that are on ground, as their heights lye far more convenient to be measured, so the measurers not knowing how long they may have been on ground, for ought I know, much of that admir'd height, may be attributed to the snows, that from time to time fall very plentifully in those frozen Regions, and are compacted together, either by the Sun, whose Beams sometimes begin to thaw it, and sometimes by the water of the waves that beat against the Ice, and being congeal'd with the snow, does as it were cement the parts of it together, and sometimes by both of these causes. So in the instance alledg'd out of Captain *James*,<sup>†@</sup> of pieces of ice that were twice as high as his Top-mast-head; / it is said also, that they were on ground in 40. fathome. And in the other Example mention'd out of *Bartholinus*, though there be 40. fathome attributed to the immersed part of the ice, yet that measure is not exclusive of a greater, for it is said, that the ice reach'd downwards above 40. fathome; and how much downwards, and whether as far as the ground, we are left at liberty to guess. And in that stupendious piece of Ice recorded in the *Nova Zembla* voyage, to have been in all 52. fathome, that is, 300. and twelve foot deep, though it be granted what they affirm, that it was 16. fathome above the water, which is almost a third part of the whole depth; yet I observe, that of this Icy mountain it is said, that *it lay fast on the ground*. So that as on the one side it seems probable, that the upper part of Islands of ice may be increas'd by snow; and as I remember, that in that famously inquisitive Navigator Mr. *Hudsons* voyage for the discovery of the North-west passage, 'tis related, that his company was / *so well acquainted with the Ice, that when Night, or foggy or foul weather took them, they would seek out the Broadest Islands of Ice, and there come to Anchor, and run and sport, and fill water that stood on the Ice in ponds very fresh and good*.<sup>†@</sup> So on the other side we know not, how much lower the Dutch-mens Ice and Captain *James's* would have reach'd into the Sea, in case the ground they rested on, had not hindred them. For though one might probably think, that these are the greatest depths that any Hills of Ice have been observ'd to attain, that mention'd by the *Hollanders* reaching 36. fathome beneath the water, and that mention'd by Captain *James*, no less then 40. fathome: yet I find in Mr. *Hudsons* Voyage, that the English in the *Bay*, that bears his Name, met with more then one or two Islands of Ice, of a far greater depth underwater. For among other things, the Relator has this memorable passage; *In this Bay, where we were thus troubled with Ice, we saw*

*many of those mountains of Ice a ground, in six or seven score fathome water. And if the Sea had been deep / enough, even these stupendious moles of Ice would probably have sunk much lower, and so have lessened the heights of the mountains.*

11. I know that delivering the measure of the Expansion of water alone, I have not said all that may be said about the Expansion of Liquors: But

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because, as it has not yet appeared to me, that any Liquor is expanded by Cold, unless by actual freezing; I doubted, whether Aqueous Liquors, as Wine, Milk, Urine, &c. were otherwise expanded by congelation, then upon the Account of the water or phlegmatick (and, in a strict sense, *congealable*,) part contain'd in them; and whether it were worth while, for a man in haste, to examine, their particular Expansions. Notwithstanding which, I would not discourage any from trying, whether or no by the differing Dilatations of Aqueous Liquors, some of them of the same, and some of them of differing kinds, we may be assisted to make any estimate of the differing proportions they contain, of phlegm, and of more / spirituous or useful Ingredients.

12. After what has been hitherto delivered concerning the *Expansion* of Liquors by Cold, it may be expected we should say something of the measure of their *Contraction* by the same Quality. But as for water, which is the principal Liquor, whose Dimensions are to be consider'd, I have formerly declar'd, that I could seldom or never find its contraction (in the Winter season when I tried it) to be at all considerable. And I shall now add, that having for greater certainty, procur'd the Experiment to be made by another also, in a Bolthead, the Account I received of it, was, that he could scarce discern the water in the stem to fall beneath its station, (mark'd at the upper part of the pipe,) when the water in the Ball was so far infrigidated as to begin to freez. Though I will not deny, that in warmer Climates, as *Italy*, or *Spain*, the contraction of the water a little before glaciation begins, may be somewhat considerable, especially if the Experiment be made in Summer, or in case (either / there or here) the water expos'd to freez be put into a vessel very advantageously shap'd, or brought out of some warm Chamber or other place, where the heat of the Air, that surrounded it, had rarifi'd it. But to examine the measures of Contraction in the several Liquors, and with the nice Observations, that such a work, to be accurately prosecuted, would require, would have taken up much more of my time then I was willing to imploy about a work which I look'd not on as important enough to deserve it. And therefore I shall here add nothing to what I have said under the Title of the *Degrees of Cold*,<sup>†\*</sup> touching the contraction of spirit of Wine, and of oyl of Turpentine, by the differing degrees of that Quality. And as for the condensation of Air, the vastest fluid we deal with, I did indeed think fit to measure how much Cold condenses it. But the account of that Experiment will be more opportunely deliver'd in one of the following Discourses.<sup>†@/</sup>

## Title XI.

### *Experiments touching the Expansive Force of Freezing Water.*

1. Having shewn that there is an Expansion made of water, and Aqueous Bodies, by Congelation, let us now examine how strong this Expansion is,

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and the rather because no body has yet, that we know of, made any particular trials on purpose to make discoveries in this matter, so that although some unhappy Accidents have kept our Experiments from being as accurate as we designed, (and as, God assisting, we may hereafter make them) yet at least we shall shew this Expansion to be more forcible, then has hitherto been commonly taken notice of, and assist men to make a somewhat less uncertain Estimate of the force of it, / then they seem to have yet endeavour'd to enable themselves to make.

2. And 1. we shall mention some Experiments, that do in general shew, that the Expansion of freezing water is considerably strong.

We took a new Pewter-bottle, capable to contain, as we guess'd, about half a pint of water, and having fill'd it top full with that Liquor, we scru'd on the stopple, and exposed it during a very frosty night, to the cold Air, and the next morning the water appeared to have burst the Bottle, though its matter were metalline, and though purposely for this trial we had chosen it quite new, the crack appeared to be in the very substance of the Pewter. This Experiment we repeated; and 'twas one of those bottles fill'd with Ice that had crack'd it, which a *Noble Virtuoso* would needs make me (who should else have scrupled to amuse, with such a Trifle, so great a Monarch, and so great a *Virtuoso*) bring to his Majesty, to satisfie him, by the wideness of the crack, and the Protuberance of the Ice, that shewed it self in / it, that the water had been really expanded by Congelation.<sup>†\*</sup>

3. We also tried, whether or no a much smaller Quantity of water, would not, if frozen, have the like Effect, and accordingly, filling with about an ounce of water a scru'd Pewter box (such as many use to keep Treacle & Salves in) quite new, and of a considerable thickness, we found, that upon the freezing of the included water, the vessel was very much burst.

Afterwards filling a Quart Bottle (if I mistake not the capacity) with a congealable liquor, and tying down the Cork very hard with strong Pack-thread, we found that the frost made the liquor force out the stopple in spite of all the care we had taken to keep it down.

But afterwards we so well fastned a Cork to the neck of a quart bottle of Glass, that it was easier for the congealing liquor to break the vessel, then to thrust out the stopple, and having for a great many hours expos'd this to an exceeding sharp Air, we found at length the bottle burst, although / it were so thick and strong, that we were invited to measure the breadth of the sides, and found that the thinnest place, where it was broken by the Ice, was  $\frac{3}{16}$  of an inch, and the thickest  $\frac{3}{8}$  that is twice as much; we also by the help of the frost broke an earthen bottle of strong Flanders metal, of which the thinnest part that was broken, was equal by measure, to the thinnest part of the other.

4. But the above mention'd Instances serving only to declare in general, that the Expansion of water by Cold is very forcible, I thought fit to attempt the reducing of the Matter somewhat nearer an Estimate less remote from

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being determinate, and because the water expos'd to congelation, may be probably supposed to be Homogeneous, we judg'd, that the quantity of it, may very much vary its degree of Force, and because some may suspect, that the Figure also may not be inconsiderable in this matter, we thought fit to make our Trials in a Brass vessel, whose Cavity was Cylindrical, and which to / make it stronger, had an orifice but at one of its ends: and whose thickness was such, that we had reason to expect, that whilst the top remained covered, but with a reasonable weight, the included water would find it more easie to lift up that weight, then break the sides. To this Cylinder we fitted a cover of the same mettal that was flat, and went a little way into the Cavity, leaning also upon the edges of the sides for the more closer stopping of the orifice; the cavity of this Cylinder was in length about five inches, and in breadth about an inch and three quarters. This Cylinder being fill'd top full with water, and the cover being carefully put on, was fastned into an Iron frame, that held it erected, and allowed us to place an iron weight, amounting to 56. pound, or half a hundred of common English weight, which circumstance I mention (because the common hundred that our Carriers, &c. use, exceeds five score by twelve.) But this vessel being exposed in a frosty night, to the cold Air, the contain'd water did not the / next morning appear to be frozen, and the trial was another time that way repeated with no better success, as if either the thickness or clearness of the mettal had broken the violence of the external Airs frigefactive Power, or the weight that oppressed the Cover had hindred that Expansion of the water, which is wont to accompany its Glaciation.



Wherefore we thought it requisite to apply to the outside of the vessel a mixture of salt with ice or snow, as that which we had observed to introduce a higher degree of Cold then the Air alone, even in very frosty nights; and though this way it self, the glaciation proceeded very slowly, and sometimes scarce at all, yet at length we found, that the water was by this means brought so far to freez, that on the morrow the ice had on one side swelled above the top of the Cylinder, and by lifting the cover on that side, had thrown down the incumbent weight; but in this trial the cover having been uniformly, or every where lifted up above the upper orifice of the Cylinder, we repeated / the Experiment divers times, as we could get opportunity, sometimes with success, and sometimes without it; and of one of the chief of our Experiments of this sort, we find the following account among our Collections.

5. [The hollow brass weight, being about one inch and three quarters in Diameter, and the brass cover put on, was loaded with a weight of 56. pound upon the cover, and expos'd to an excessively sharp night, the next morning the cover and the weight were found visibly lifted up, though not above (that we could discern) a small Barley-corns breadth, but the thickness of the brass cover was not here estimated, which was much less then half an inch, which according to former observations, one might expect to see the ice ascend. But that which we took particular notice of, was, that the inclosed Cylinder of ice, being by a gentle thaw of the superficial parts taken out, appear'd so full of bubbles, as to be thereby made opacous: Also when in the morning

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the Cylinder was brought into my / Chamber, before the fire was made, the 56. pound weight being newly taken off, at a little hole, that seemed to be between the edge of the Brass and Ice, there came out a great many drops of water, dilated into numerous bubbles, and reduced into a kind of froth, as if upon the removal of the oppressing weight the bubbles of the water had got liberty to expand themselves, but this lasted but a very little.]

6. After this, the difficulty we have often met with in the placing of great weights conveniently upon the cover of a Cylinder, and the Expectation we had to find the Quantity of the water, we made use of, capable upon its Congelation, to lift up a much greater weight, invited us to make trial of its Expansive force, by somewhat a differing way, which was, to fit a wooden plug to the Cavity of the Cylinder (after we had suffered it to soak a convenient time in water, that, swelling as much as it would before, it might be made to swell no more by the water, which would lye contiguous to it in the vessel) / and then to drive it forcibly in, till by considerable weights appended to the extant part of the plug, when the Cylinder was inverted, we could not draw it out; the success of one of these Trials is thus set down in our Collections.

7. [A Plug was driven into the Cavity of a Brass Cylinder, first filled with water, the Plug being also well soaked, then the Cylinder being inverted, the Plug took up half a hundred and a quarter of a hundred weight, and would possibly have taken up much more, and being exposed to a very sharp night, the freezing water thrust out the plug about a barley-corns breadth, quite round above the upper edge of the Cylinder, and it freezing all that day and the next night, it was again exposed, the plug not being yet taken out, and then the plug was beaten out a little more, namely (in all) near a quarter of an inch.]<sup>†\*</sup>

8. Thus we see, that the expansive endeavour of the water forced a resistance, at least equal to that which would have been made by a weight / of 74. pound, and probably, as the note intimates, would have appear'd able to do more, if we had had convenient weights and Instruments, wherewith to have measure'd the strength of the waters endeavour outwards, which some subsequent Trials, made us think very considerable, though not finding their Events set down in our notes, we think it fit at present to leave them unmentioned.

But one thing there is in these trials, that I think not unworthy a Philosophers notice, and his



considering, namely, that this endeavour of the water to expand it self, is thus vigorous, though the uttermost term to which it would expand it self, in case it were not at all resisted, would be but to about a ninth, or at most an eight part of the space it possess before it began to freez; whereas Air may by Heat (which yet we have elsewhere shewn, will not reduce it to any thing near its utmost expansion<sup>†@</sup>) be brought to possess (though not to *fill*) according to the the diligent *Mersennus*'s observation,<sup>†@</sup>

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seventy times, the Dimensions it had before Rarefaction, / and consequently the Air expanded by Heat, does by its endeavours, tend to acquire above 60. times the space that the water does, when expanded by so high a degree of Cold, as is capable to turn it all into Ice: not to mention that the expansion to which the Air tends upon the Account of its own spring, is, (as we shew in another place<sup>†@</sup>) many times greater then that to which *Mersennus* could bring it upon the bare Account of Heat.

9. There remains yet one way, whereby we hop'd, though not to measure the Expansive force of freezing water, yet to manifest it to be prodigiously great, or in case we fail'd of this aim, to produce at least some other *Phænomena* relating to Cold, that would not be inconsiderable. And though our endeavours succeeded not, yet because a happier opportunity may bring them to be one way or other successful, we shall annex, That we caus'd to be made, an Iron Ball of between two and three inches in Diameter, which Ball was solid, save that in the midst there / was a small Cavity left to place a little water in, together **with a female screw**, as they call it, reaching from the outward surface of that internal cavity; and to this was applied a strong Iron screw, as to fill it with as much exactness as could be obtained. And this screw was made to go so hard, that it requir'd to be screw'd in by the help of a Vice, that it might not be forc'd out, without breaking the Iron it self. Our design in imploying this Instrument was, that having well fill'd the internal cavity with water, and forc'd in the screw as far as it could be made to go, the Instrument thus charg'd with water, might be expos'd to the highest degree of Cold we could produce. For having thus ordered the matter, we thought we might expect, either that the water how much soever we heightned and lengthned the Cold, would not freez at all, being hindred from the Expansion belonging to Ice in comparison of water; *or*, if it did freez, that one of these two things would happen, / *either* that the expansive force of that little water, would by forcing such an Iron Instrument, manifest its strength to be stupendious, *or* by not breaking it, present us with ice without Bubbles, or at least not rarer and lighter, then the water it was made of; but for want of a sufficient Cold our designs succeeded not, so as to satisfie us, though we more then once attempted it. For the great thickness of the Iron being consider'd, we were not sure that the waters not freezing, might not proceed rather from the thickness and compactness of the metal, then from its resistance to the expansion of water. And therefore we must suspend the inferences, this Experiment may afford us, till we have opportunity to make trial of it, with a Cold not only very intense, but durable enough, the want of which last circumstance keeps us from daring to build any thing on our Experiment.<sup>†\*</sup>

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10. And here we may take notice, that it may be an inquiry, more worthy a Philosopher, then easie for him, whence this prodigious force, / we have observ'd in water, expanded by glaciation, should proceed. For if Cold be but, as the *Cartesians* would have, a privation of Heat,<sup>†\*</sup> though by the recess of that Ethereal substance, which agitated the little Eel-like particles of the water, and thereby made them compose a fluid body, it may easily enough be conceiv'd, that they should remain rigid in the Postures wherein the Ethereal substance quitted them, and thereby compose an unfluid Body like Ice: yet how these little Eels should by that recess acquire as strong an endeavour outwards, as if they were so many little springs, and expand themselves too with so stupendious a force, is that which does not so readily appear. And on the other side in the *Epicurean* way of

explicating Cold, though the *Phænomenon* seems somewhat less difficult; yet it is not at all easie to be salv'd: For though, granting the Ingress of swarms of Cold Corpuscles, the Body of water may be suppos'd to be thereby much swell'd and expanded, yet besides that these Corpuscles / stealing insensibly into the Liquors they insinuate themselves into, without any shew of boisterousness or violence, 'tis not so easie to conceive how they should display so strange a force against the sides of those strong vessels that they break, when they may as freely permeate or enter them:†\* besides this, I say, we observe that in Oyl, which requires a far greater degree of Cold to be congeal'd to a good degree of hardness, the swarms of frigorifick Atoms that invade it, are so far from making it take up more room then before, that they reduce it into less, as may appear by those former Experiments which manifested, that Cold does not expand, either oyl or uncongealable Liquors, but condense them.

11. After what I have thus largely delivered, concerning the expansive endeavour of freezing water, I hope I may be allow'd to leave to others (if they shall think it worth the labour) the prosecution of the like Experiments upon Wine, Milk, Urine, and other Liquors abounding with Aqueous parts, concerning which / we shall only in general remind those that may have forgotten it, That by some of our Experiments it appears, that such Aqueous Liquors are expanded by congelation, and, that their endeavour outwards is considerably forcible, seems more then likely from what we formerly noted out of the Dutch Voyage to *Nova Zembla*,†\* where 'tis related, that by the extreme Cold, both some of their other Barrels, and some of those that were hooped with Iron, were, as they speak, *frozen in pieces*, that is, according to our Conjecture, burst together, with the Hoops, whether of Wood or Iron, by the expansive force of the imprison'd Liquors brought to freez.

12. To which I shall add, that when I asked an Ingenious person,†\* whether in *Russia*, where he liv'd a good while, Beer and Wine did not, when brought to congelation, break the vessels they were frozen in; He Answered, *That he*

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*had not observed wooden vessels to have been broken by them, (perhaps because of their yielding) but glass and stone Bottles often. /*

## **Title XII.**

***Experiments touching a New way of estimating the Expansive force of Congelation, and of highly compressing Air without Engines.***

1. There is yet another way, that I bethought my self of, at once to measure the force wherewith freezing water expands it self, and to reduce the Air to a greater degree of condensation, then I have as yet found it brought to by any unquestionable way of compressing it: But whereas by this method to determine exactly the Expansive force of the water, it were requisite not only to know the quantity of the water, and that of the Air exposed to the Cold, but to make the Experiment in vessels conveniently shap'd to measure the / Dilatation of the one, and the compression of the other; our Experiments being made in a place where we were not provided of such glasses, we were not able to make our trials so instructive and satisfactory, as else we might have done; nevertheless we shall not scruple to subjoyn those of them, that we find noted down among our Collections, allowing our selves to hope, that will not be unacceptable or appear impertinent, not only upon the account of their novelty, but for two other reasons.

2. The first, because though they do not accurately define the Expansive force of freezing water, yet they manifest, that it is wonderfully great, better perhaps then any Experiment that has been hitherto practised (not to say, thought of) as may appear by comparing what we have delivered in another Treatise,†\* of the great force requisite to compress Air considerably, with the great compression of Air that has already been this way effected.

3. The second, because this new way affords us one of condensing the / Air much farther then hitherto it has, by any method I have heard of, been unquestionably reduced, I say, *unquestionably*, because though the diligent *Mersennus*,<sup>†\*</sup> and others, seem to have conceived himself, to have reduced it in the wind-Gun into a very narrow room, yet besides that, by our Expedient, we have compressed it beyond what these Ingenious *Men* pretend to: Besides this, I say, I have long much questioned, whether the way of compressing Air in a wind-Gun, which both they and we have imploy'd, may safely be relied on; for the oyl or some other analogous thing, that is wont this way to be imploy'd, and the overlooking of several circumstances, that are more necessary to be taken into diligent consideration, then wont to be so, may easily

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enough occasion no small mistake in assigning so great a degree to the compression of the Air; but our Exceptions against this way of measuring it, may be more opportunely discours'd of in another place. And therefore we will now proceed to take notice, that of the two known / ways of compressing Air, the clearest and most satisfactory, seems to be that which is performed in the *wind Fountain*, as 'tis commonly called, where yet I have seldom, if ever, seen the Air, (that I remember) by all the violence men could use to syringe in water, crowded into so little as the third part of the capacity of the vessel. And an ingenious Artificer that makes store of these Fountains, being consulted by me, about the further compressing of Air in them, he deterr'd me from venturing to try it, by affirming to me, that both he and another skilful Person of my Acquaintance, had like to have been spoiled by such attempts; for endeavouring to urge the Air beyond a moderate degree of compression, it not only burst some Fountains made of Glass, but when the Attempt was made in a large, but thick vessel, made of strong and compact Flanders Earth (the same with that of Jugs and stone Bottles) the vessel was by the over-bent spring of the Air burst with a horrid noise, and the pieces thrown off with that violence, that / if they had hit him, or his Friend that assisted him in the Experiment, they might have maimed him, if not killed him out right, so that the greatest unquestionable Compression of the Air seems to have been that, recorded in the Fifth Chapter of our *Defence* against the learned *Linus*, where nevertheless, we could reduce the Air by the weight of a Cylinder of *Mercury* of about 100. inches, (which consequently might near countervale a Cylinder of six score foot of water) but into a little less then a fourth part of its usual extent;<sup>†\*</sup> but how much further the Air may be compressed by our new purposed way, it is now time to shew by the ensuing notes, of which we have not omitted any that we could find, both that some scruples, which might else arise about the way we imployed, may be prevented, or satisfied, and that the way, we imployed in practising this method, might by some variety of Examples be the better understood.

4. [We took a large glass-Egg,<sup>†@</sup> with a Cylindrical stem about the / bigness of my middle finger, and pouring in water, till it reach'd about a fingers breadth higher then the bottom of the stem, we set it to freez in snow and salt, for some hours, with the stop of the stem (which was drawn out into a very slender pipe almost at right angles with the stem) open, and there left it for some hours, and the water was risen betwixt six and a half, and seven inches. This we did in order to another Experiment, but then easily and nimbly sealing up the slender pipe above mentioned, that the Air in the stem might not be heated, we let it continue in the snow, sometimes adding fresh for about 24. hours to observe, to what degree the water, by expanding it self, would compress the imprison'd Air. The length of the Cylinder of Air to be condens'd at the time of the sealing, was (accounting by Estimation for the

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slender pipe newly taken notice of) almost 9 7/8 inches. This space we observed the ascending water as the ice increas'd below, to invade by degrees: (for we watch'd it, and measur'd / it from time to time) so much, till at length the water reach'd to 8. inches and 7/8 almost, above the station (which we had carefully mark'd with a Diamond) in which we found it, when the glass was seal'd up, leaving but about an inch of Air at the top, so that of the whole space before possess'd by the



Air, the water had intruded into near nine parts of ten; then being partly apprehensive the glass would hold no longer, but have its upper part blown off, as it happened to us a little before with another vessel, and partly being desirous to try that which follows, we leisurely inverted the glass, that the Air might get up to the ice, for all the water in the stem had been purposely kept unfrozen, and having provided a Jar to receive the water that should be thrown out, we broke the slender pipe which we had seal'd up, and immediately as we expected, the compressed Air with violence and noise, blew out of the stem into the Jar about ten inches of water, which was somewhat more (between half an inch and a whole inch, by reason / of the *Impetus* of the self expanding Air) then the space possess'd by the Air, before it began to be compress'd. And besides this, such a strange multitude of Bubbles, that were formerly repress'd, did now get liberty to ascend from the lower part of the glass to the top of the remaining water, that it somewhat emulated that which happens to bottled Beer; upon the taking out of the Cork. *N.B.* when the Air was compressed beyond seven inches, we observ'd divers times, that the inside of the glass possess'd by the Air, and nearest to the water, was round about, to a pretty height, full of very little drops like a small dew, but when we came to break the glass, we took noe such notice, whether the rising water had lick'd them up, or their concourse made them run down into it, or for some other reason, we determine not.]

*Another.*<sup>†</sup>@

5. [We took a single vial filled with water, about half an inch above the lower part of the neck, and leaving about two inches of Air in the / remaining part of the neck, which was drawn out into a slender pipe, like that of the glass last mentioned, we seal'd it up, the Air being first well cool'd, and exposing it to freez, we observ'd a while after, that it had by guess condens'd the Air into lesser room. A while after, being in another Chamber, we heard a considerable noise, and imagining what it was, we went directly to the glass, whose upper part consisting of about an inch of the neck, besides the slender pipe, we found had been blown off from the table upon the ground, the body and part of the neck remaining in the snow; but this glass was of a mettal that uses to be more brittle then white glass.]<sup>†\*</sup>

*Another.*

6. [A round white glass, almost fill'd with water, was seal'd up with care to

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avoid heating the included Air, which amounted to a Cylinder of about two inches and  $\frac{7}{8}$ ; after a while the water swell'd and compressed the Air almost two inches, that is full two thirds: and then (as we conjectur'd, because the snow reaching / too high, froze it in the neck) we found the glass crack'd in many places of the Ball, and the top thrown off at some little distance from it.]

*Another.*

7. [A large single vial seal'd, in whose neck the Air was not condens'd to half its former room, just as we were going to break it under water, to observe the sally of the compress'd Air, suddenly blew off with a good noise, and threw from the table almost the whole neck of the Vial in one intire piece, which is near four inches long, and at the *Basis* above an inch broad.]

8. [A glass about the bigness of a Turkey Egg, and of an oval form, with a Neck almost Cylindrical, but somewhat wider at the lower then the upper part, was fill'd with water, till there was left in the neck four inches and a half, whereof the last quarter of an inch, and a little more, was much narrower then the rest, being drawn into a conical shape, that it might be easily seal'd at the Apex; along this Cylinder, from the surface of the water, to the top of the glass, / was pasted a list of Paper, divided into inches and quarters, and then the glass being carefully and expeditiously seal'd up by the flame of a candle, we observ'd, that by holding the glass a while in a warm hand, and a



room where there was a good fire, the water was swell'd up near a quarter of an inch, but placing the glass amongst solid pieces of ice mixt with salt, the water quickly began to subside upon the Infrigidation, and a while after beginning to freez, it began to swell, and by degrees compress'd the Air, till it had crowded it into less then a 17. part, by what seem'd indisputable, for by estimate it seem'd to some to be crowded into less then a 20. part if not a much lesser part of the room it formerly possess'd, which difference of Estimates, notwithstanding the divided Paper, proceeded from the change of the figure of the upper end of the glass, from the Cylindrical, and to shew that there was no leak at the place where the glass was seal'd; besides, that by prying diligently, we could discern none; besides this, / I say, when the pressure of the thus crowded Air grew too strong for the resistance of the glass, it burst with a noise, that made us come to it from several places of the house; the vessel broke not in the Cylindrical part (as I may so speak) but in the oval, the whole pipe with the seal'd end remaining entire, the ice appear'd full enough of Bubbles, which made it white and opacous, and the water that had ascended into the neck, upon the breaking, was all driven out of it.]

Thus far our Collections, but because we had in another glass, where the operation was sooner dispatch'd, an opportunity of watching & observing somewhat more exactly, we will add,

9. That the last, and possibly the best Experiment we had of compressing Air by freezing, was made in a short and strong glass-Egg, whose ball was very great in proportion to the stem, that the expanding of the water might have the more forcible operation: This vessel being exactly seal'd, and having a divided list of / paper pasted along the stem, was set to freez with snow (or

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ice) and salt, and the contain'd water did quickly begin to crowd the Air into a lesser room, and for a good while ascended very fast, till at length it having thrust the Air into so small a part of the Cavity of the pipe, that we vehemently suspected there might be some unheeded flaw or crack of the glass, at which the Air had stollen out, we drew near the vessel, and attentively prying all about it, to try if we could discover any ground of our suspition, we found (as far as the divided list, and other circumstances could inform us) that the Air (supposing none of it to have got away) was reduc'd by our Estimate into the 19. part of the space it possess'd before. And this our curiosity prov'd not unseasonable, for whilst we were narrowly surveying the glass, to spy out some flaw in it, we were quickly satisfied there had been none, by a huge crack made upon the Eruption of the included Air, whose spring being by so great a compression made too strong for the glass to resist, it did with a great / noise break the ball of the glass into many pieces, throwing the unfrozen part of the water upon me, and also throwing off the stem of the Egg, which yet I had the good fortune to recover intire, and which I yet keep by me as a rarity.

10. Thus far we then proceeded in compressing the Air, which being done in vessels Hermetically seal'd, where no Air can get in or out, seems to me a more unexceptionable way, then those that have hitherto been thought of. But further, we could not then prosecute it for want both of convenient glasses, and of ice or snow, of which if we were provided, and particularly of strong glasses, we should little doubt of reducing the Air to a yet more considerable degree of compression.

11. We may add on this occasion, that we look'd upon the same way as somewhat less unpromising then others, that have been hitherto us'd to try the compression of water; for though hitherto neither the Experiments of Ingenious Men, nor those made by our selves have fully satisfi'd / us, that water admits any more compression, then it may suffer upon the account of the little parcels of Air, that is wont to be dispersed among it, yet the unsuccessfulness may perhaps (for I propose it but as a mere conjecture) be imputed to the porousness of the vessels, wherein by the ways already practis'd, the Experiment must be made, whereas in this new way of ours, not only the force wherewith the compress'd Air presses upon the water, grows at length to be exceeding great, and is appli'd not with

a sudden *Impetus*, as when a Pewter vessel is knock'd with a Hammer, but by slow and regular degrees of increase, but the water is kept in a vessel impervious to its subtilest parts, so that it may indeed crack the glass, but cannot get out at the pores, as water compress'd is wont to do at those of metalline vessels. The prosecution of this Experiment to bring it to any thing of Accurateness, we omitted, partly through forgetfulness and Avocations, and sometimes for want of conveniency to try it. But by the first of / the lately mention'd Experiments, about the condensation of Air, it seems by the strong multitude of Bubbles, which upon the breaking of the glass appear'd in the water that had been compress'd betwixt the Air and the Ice, that those two Bodies had very violently compress'd it: and this we are the more apt to believe, because that

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another time, when we had seal'd up some Air, and water in a glass-Egg, and permitted the water to swell by the operation of the Cold, but till it had reduc'd the Air, included with it, to about three quarters of the space it possess'd before, even then (I say) to try whether the subjacent water were not also compress'd by the Air it urg'd, we broke off the seal'd Apex of the glass, and perceiv'd, as we expected, the water to ascend, and that to the height of a quarter of an inch, as we found by measure. But such trials having not been, as we just now acknowledg'd, duly prosecuted, we shall at present content our selves to have nam'd this way of attempting the compression of water, without grounding any Inferences upon it. /

### **Title XIII.**

#### ***Experiments and Observations touching the sphere of Activity of Cold.***

1. The sphere of Activity of Cold, or to speak plainer, the space, to whose extremities every way the action of a Cold body is able to reach, is a thing very well worth the enquiring after, but more difficult to find, then at first one would imagine: For to be able to assign the determinate limits, within which, and not beyond them, a cold Body can operate, several things are to be taken into consideration; as first, what the degree of Cold is, that belongs to the assigned Body: For it seems rational to conceive, that if a cold Body as such, have a diffusive vertue, those that have greater degrees of / Cold, as Ice and Snow, will be able to diffuse it to a greater distance, as we see that a coal of Fire will cast a sensible heat much further then a piece of wood, that is heated without being kindled. Secondly, the *Medium* through which the Diffusion is made, may help to enlarge the Bounds, or straiten the Limits of it, as that *medium* is more or less dispos'd to receive or to transmit the Action of the cold Agent. Thirdly, Not only the Consistence, and Texture of the *Medium*, but its Motion, or Rest may be considered in this case. For in frosty and snowy weather, men observe the winds that come from frozen lands, to blow more cold, then winds from the same Quarter would do, in case there were no Ice nor Snow in their Passage. Fourthly, There may be made very differing Estimates of the Diffusion of Cold, according to the Instrument that is imploy'd to receive, and acquaint us with the Action of Cold. For a liquor or other Body may not appear cold to him, that examines it with a Weather-glass, whilst he shall feel / it cold with his hand; and, as we elsewhere also note, to that sensory it self, as 'tis variously dispos'd, the same object will seem more or less cold; so much may the Predisposition of the Organ impose upon the unskilful or unwary. Fifthly, The very bulk of a cold Body may very much inlarge or lessen its sphere of Activity, as we may have occasion to shew ere long. And besides there may be divers other things, that may render it very difficult to ascertain any thing in this matter. And therefore I shall reserve them for other opportunities, and observe now in general,

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that in such small parcels of Ice it self, as in our Experiments we are wont to deal with, we have found the sphere of Activity of Cold exceeding narrow, not only in comparison of that of heat in fire, but in comparison of the Atmosphere, if I may so call it, of many odorous Bodies, as Musk, Civet, Spices, Roses, Wormwood, *Assa dulcis*, *Assa fætida*, *Castoreum*, *Camphire*, and the like;

nay, and even in comparison of the sphere of Activity of the more vigorous / Loadstones, insomuch that we have doubted, whether the sense could discern a cold Body, otherwise then by immediate Contact?

2. And to examine this, having taken a piece of Ice, we did not find upon trials, that I partly made my self, and partly caus'd in my presence to be made by others, that if a mans Eyes were close shut, he could certainly discern the Approach of a moderately siz'd piece of Ice, though held never so near his fingers ends. Nay, which is more considerable, having had the curiosity to make the Trial, with one of those very sensible Thermoscopes I have formerly mention'd (wherein a pendulous drop of liquor plays up and down in a slender pipe) I found, that by holding it very near to little Masses of snow (somewhat compacted too) the movable drop, did not betray any manifest operation of so cold a neighbouring Body; but if the glass were made to touch the snow, the effect would then be notable, by the hasty descent of the pendulous drop, or its motion towards the obtuse part of the / Instrument, in case that were not perpendicularly, but laterally appli'd to the snowy Lumps. But this languidness of operation, may perhaps proceed in great part from the smallness of the Pieces of Ice that were imploy'd: For hearing of a Merchant, that had made divers Observations about Cold in *Greenland*, I desir'd, by the mediation of a very learned Friend, to be inform'd, whether or no in the night they could perceive those vast heaps, or rather mountains of ice, that are wont to float up and down in that Sea, by any new and manifest accession of Cold, and was inform'd by way of Answer to that Question, that being at Sea, they could know the approach of Ice, as well by the increase of Cold, as by the glaring light which the Air seem'd to receive from the neighbouring Ice.

3. But that which makes me suspect, that there may in this account be some mistake, is, that I have not yet met with any like observation in any of the voyages into gelid Climates, that I have had occasion to / peruse, though in some of them the Navigators frequently mention their having met with vast rands (as some call them) and Islands of mountainous ice in the night. And 'tis, as I remember, the complaint of one or two, if not more of them, that the Ship lay close by such vast pieces of ice, without their being aware of it, by reason of the fogs. By which it seems that there was no sensible Cold diffused to any considerable distance, whereby they might be advertised of the unwelcome neighbourhood even of so much ice: But possibly the approach of far smaller masses of ice, would have been sensible to them in such a Climate as ours, where the organs would not have been indisposed to feel, by a long accustomance, of any thing near so intense a degree of Cold, as that which then reigned in those Northern Seas.

4. Whilest we were considering the Difference, betwixt the operations of

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even the Coldest Bodies at the very nearest Distance, and upon immediate Contact, we thought it an Experiment not altogether unworthy / to be tri'd, whether, though ice and snow alone, that is, unassisted by salts, would not in some of our formerly mention'd Experiments freez water, through the thickness even of a thin glass, they may not yet do it when the water is immediately contiguous to them. And I remember, that we took a conveniently shap'd Glass, and having frozen the contained water for some hours, from the bottom upwards, till the ice was grown to be of a considerable thickness, we mark'd, what part of the glass was possess'd by the unfrozen water, and then removing the vessel to a little Distance from the snow, and salt, it stood in before, we let it rest there, to try whether the ice would freez any part of the contiguous and incumbent water; but some intervening accidents hindred us from being able to derive any great satisfaction one way or other from our trial.

5. Wherefore we shall add by way of Compensation, that the diligent *Olearius* relates,<sup>†@</sup> that at *Ispahan*, the Capital City of *Persia*, though it be / seated in a very hot Climate, and though it seldom freez there above a finger thick, and the ice melt presently at Sun-rising, yet the Inhabitants have Conservatories, which they furnish with solid pieces of ice of a good thickness, only by

pouring at night great store of water at convenient intervals of time, upon a shelving floor of Free-stone or Marble, whereon, as the water runs over it, the most dispos'd of its parts, are in their passage arrested, and frozen by the contiguous ice, which by this means (says my learned Author) may be brought in two or three successive nights, to a very considerable thickness.

6. We several times gave order to have this Experiment tried in *England*, but *partly* through the negligence of those we imploy'd, and *partly* upon the score of intervening circumstances, our expectation was but ill answered. And in this case I mention *intervening circumstances*, because having caus'd a servant to pump in the night, upon a not very thin plate of ice, that was laid shelving / upon a Board, and another flat piece of Ice being about the same time laid under a place, where water derived from a neighbouring spring, is wont continually to drop, he brought me word, that not only in this last nam'd place, the ice melted away, but that under the pump, instead of increasing in thickness by the waters running over it, it was thereby rather dissolv'd. At which somewhat wondring, I went in the morning my self to the pump, and causing a good flake of ice to be in a convenient posture plac'd under it, I observed the water as it came out of the pump, and was falling on the ice, to smok, as if the depth of the Well had made the water, though very Cold to the touch, somewhat warm in comparison of the ice, and thereby fitter to resolve than to increase it; (which inconvenience may be prevented by suffering the water of deep Springs and Wells, to stand to cool in the Air, before it be put to the Ice,) and this, though the neighbouring Air were, as I found by manifest proofs, so cold, that I was not tempted to / impute the

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unsuccessfulness of the Experiment, rather to its want of a sufficient coldness, than the water's: So that till I have an opportunity of making a further Trial, I cannot say more to the *Persian* way of augmenting ice. But to proceed, our having met with but an unsatisfactory Account of this Experiment, which we were the more troubled at, because this seem'd a promising way of trying that, which otherwise is not so easily reduc'd to Experiment; for the Temperature of the Air, must be seriously consider'd in assigning the Cause of divers trials, that may be made for the resolving of the same Question. For to omit other Examples, here in *England* we find, that water poured on snow, is wont to hasten the Dissolution of it, and not to be congeal'd by it; whereas having inquir'd of an Ingenious Person, that liv'd a good while among the *Russians*,<sup>†\*</sup> he inform'd me, that it was their usual way to turn water and snow into ice, by pouring a convenient Proportion of that liquor into a great quantity of snow, and having / also inquir'd, whether ice had not the like operation, he told me, that 'twas usual, and he had seen it practis'd in *Muscovy*, to cement Ice to Buildings, and other things, and also to case over Bodies, as it were, with Ice, by gradually throwing water upon them. But I doubt, whether that Effect be to be ascrib'd barely to the Contiguity of the Ice, because I learn'd of him, that this way of increasing ice is practis'd in very frosty weather, when water thinly spread upon almost any other Body, would be frozen by the vehement sharpness of the Air.

7. The Glaciations, that nature unguided by Art, is wont to make, beginning at those parts of Bodies, at which they are expos'd to the Air, it usually happens, that they freez from the upper towards the lower parts. But how far in Earth and Water (the most considerable Bodies, that are subject to be frozen) the frost will pierce downwards, though for some hints, it would afford, worth the knowing, is not easie to be defin'd, because the deepness of the / frost may be much varied by the degree of Coldness in the Air, by which the Glaciation seems to be produc'd, as also by the greater or lesser Duration of the frost, by the looser or closer texture of the Earth, by the nature of the Juices wherewith the Earth is imbu'd, and by the constitution of the subjacent, and more internal parts of the Earth, some of which send up either actually warm, or potentially hot and resolving steams, such as those that make corrosive liquors in the bowels of the Earth; so that the frost will not seiz upon, or at least cannot continue over Mines; and I have seen good large scopes of land, where vast quantities of good Lime-stone lay near the surface of the Earth, on which I have been assur'd by the Inhabitants, that the snow will not lye. There are divers other things, that may vary the depth to which the frost can penetrate into the ground, (I say, into the ground, because in most



cases it will pierce deeper into the water.) But yet that we may not leave this part of the *History of Cold* altogether uncontributed / to, we will add some of our Notes, whereby it will appear, that in our Climate the frost pierces far less into the ground, then many are pleas'd to think.

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8. The notes I find about this matter are these that follow, which I transcrib'd unaltered, because 'twere tedious, and not worth while to add the way we imploy'd, and the cautions we us'd in making the observations, but we shall rather intimate, that the following trials were made in a Village about two miles from a great City.

[I. *Jan.* 22. After four nights of frost, that was taken notice of for very hard, we went into an Orchard, where the ground was level, and not covered with grass, and found by digging, that the frost had scarce pierc'd into the ground three inches and a half. And in a Garden nearer the house, we found not the Earth to be frozen more then two inches beneath its surface.

II. Nine or ten nights successive frost froze the grasless ground in the Garden, about six inches and a half, / or better in depth, and the grasless ground in the Orchard, where a wall shelter'd it from the south Sun, to the depth of about eight inches and a half, or better.]

[*February* the 9. we digg'd in an Orchard near a wall, that respects the North, and found the frost to have pierced the ground about a foot and two inches, at least above a foot: This is the eight day since it was about eight inches and a half.]

[A slender pipe of glass, about 18. inches long, and seal'd at one end, was thrust over night into a hole, purposely made with a Spit, straight down into the ground, the surface of the water being in the same level with that of the Earth, the next morning the Tube being taken out, the water appear'd frozen in the whole Capacity of the Cylinder, but a little more then three inches. But from this stick of ice, there reach'd downwards a part of a Cylinder of ice of about six inches in length, the rest of the water remain'd unfrozen, though it were an exceeding sharp night, preceded by a Constitution of / the Air, that had been very lasting, and very bitter. The Earth in the Garden, where this Trial was made, we guess'd to be frozen eight or ten inches deep, as it was in another place about the same house. But if this Tube had not been in the ground, the ambient Air would have frozen it quite through.]<sup>†\*</sup>

9. Another Note much of the same import, we find in another place of our Collections.

Finding that by reason of the mildness of our Climate, I was scarce to hope for any much deeper Congelation of the Earth or Water, I appli'd my self to inquire of an Ingenious Man, that had been at *Musco*,<sup>†\*</sup> whether he had observed any thing there to my present purpose, as also to find in Captain *James's* Voyage, whether that inquisitive Navigator had taken notice of any thing, that might inform me, how far the Cold was able to freeze the Earth or Water in the Island of *Charleton*, where that Quality may probably be supposed to have had as large a sphere of Activity, as in almost any part of the / habitable world: And by my Inquiries I found, that even in frozen Regions themselves, a congealing degree of Cold pierces nothing near so deep into

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the Earth and Sea, as one would imagine: For the Traveller, I spoke with, told me, that in the Garden in *Musco*, where he took notice of the thing I inquir'd about, he found not the ground to be frozen much above two foot deep. And in Captain *James's* Journal, the most that I find (and that too, where he gives an Account of the prodigiously tall ice they had in *January*) concerning the piercing of the frost into the ground, is this, that *The ground at ten foot deep was frozen.*<sup>†@</sup> Whence by the way we may gather how much sharper Cold may be presum'd to have reigned in that Island,

then even in *Russia*. And as for the freezing of the water, He does in another place occasionally give us this memorable Account of it, where He relates the manner of the breaking up the Ice in the frozen Sea, that surrounds the Island we have been speaking of. *It is first to be noted* (says he<sup>†@</sup>) *that it doth / not freez (naturally) above six foot, the rest is by accident, such is that Ice, that you may see here six fathome thick. This we had manifest proof of by our digging the Ice out of the Ship, and by digging to our Anchors before the Ice broke up.* The rest of that account not concerning our present purpose, I forbear to annex, only taking notice, that notwithstanding our lately mention'd Experiment of freezing water in a glass Tube thrust into the Ground, yet it seems, that at least where Captain *James* winter'd, the water was not much above half so thick frozen as the Earth. But we have already noted the indisposition of salt-water to congelation, and whether fresh water would not have been deeper frozen may be justly doubted. /

#### Title XIV.

*Experiments touching the differing Mediums through which Cold may be diffus'd.*

1. In examining whether Cold might be diffus'd through all *Mediums* indefinitely, notwithstanding their Compactness or the Closeness of their Texture, we must have a Care not to make our Trials with *Mediums* of too great thickness, least we mistakingly impute that to the Nature of the *Medium* which is indeed caus'd by the distance which the *Medium* puts betwixt the Agent and the Patient. For the mixtures of Ice and Snow, wherewith we made our Experiments, will operate but at a very small distance, though the *Medium* resist no more then the common Air, as may appear by some of the / Experiments recorded in this Treatise.

This premis'd, we may proceed to relate, that having plac'd a copious mixture of ice and salt in Pipkins glaz'd within, and in white Basons glaz'd both within and without, we observ'd, that the outside of both those sorts of vessels was crusted over with ice: though, however the bak'd Earth had not been compact, nor the vitrifi'd surfaces of a very close Texture; the very thickness

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of the vessels was so great, that it seem'd it would scarce have been able to freez at a greater distance.

2. By the Experiments formerly mention'd of freezing water in Pewter bottles, it appears, that Cold is able to operate through such mettalline vessels.

3. And this may be somewhat confirm'd by one of the prettiest Experiments, that is to be perform'd by the help of Cold, namely, the making Icy Cups to drink in. The way we us'd was this; We caus'd to be made a Cup of *Lattin* (by which I mean Iron reduc'd into thin plates, / and tinn'd over on both sides) of the shape and bigness I intended to have the Cup of; then I caus'd to be made of the same matter another Cup of the same shape with the former, but every way less, so that it would go into the greater, and leave a competent interval for water, betwixt its convex surface, and the concave of the other. This innermost Cup was furnished with a rim or lip, by which it lean'd upon the greater, and by whose help its sides and bottom were easily plac'd at a just and even distance from the sides and bottom of the other; but the Distance between the two bottoms is made greater, then that between the sides, that the icy Cup might stand the firmer, and last the longer. The interval between the two parts of this Mould being fill'd with water, and the Cavity of the internal Cup being fill'd with a mixture of ice and salt, (partly to freez the contiguous water, and thereby cooperate to the quicker making of the Cup, and partly by its weight to keep the water from buoying up so light a Cup,) / the external part was surrounded with ice and salt, whose Cold so powerfully penetrated to the internal metalline Mould, that the water was quickly frozen, and (the Parts of the Mould being disjoyn'd) appeared turn'd into an icy Cup of the bigness and figure design'd: And these Cups being easily to be made, and of various shapes (and that in the midst of Summer, if snow or ice be at

hand) are very pleasant trifles, especially in hot weather, when they impart a very refreshing coolness to the drink poured into them, and though they last not long, especially if they be imploy'd to drink Wine, and such like spirituous Drinks in, yet whilst some are melting, others may be provided, and so the loss may be easily repair'd; all the difficulty we met with, was to disjoyn the parts of the Mould which are wont to stick very fast to the ice they include. And we tri'd to obviate this, sometimes by annointing the inside of the Mould with some unctuous and not offensive matter, to hinder the Adhesion of the ice, and sometimes by applying / some convenient heat both to the convex part of the external, and the concave part of the internal piece of the Mould, which last mention'd way is quick and sure, but lessens the durableness of the Cup.

(We were lately inform'd, that this way of making Cups of Ice, is set down in *Berkley's Argenis*, and 'tis like enough, that ingenious Man may have learn'd it amongst some of the *Virtuosi* of *Italy* he convers'd with:†\* But if we that learn'd it from none of them, had not been taught it by Experience, we should scarce

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have ventur'd to try it upon the Credit of a Romance; that sort of Composures being wont to be fabulous enough to pass but for Poems in Prose.)

4. The learned and industrious Mathematician *Erasmus Bartholinus*, mentions in his newly publish'd Discourse *de Figura Nivis*, an Experiment, by which he tells us, that some Masters of Natures secrets, do easily, even in the midst of heat, reduce water into Air.†\* For they put a little snow or ice into a Funnel, and thereby so refrigerate and condense the / ambient Air, that there will dew trickle down the sides of the Funnel: By which means it has been said, that some Ingenious Men have hop'd to make an artificial Fountain in the midst of Summer. But I here mention this Experiment *rather*, because 'tis not unlikely to please those to whom 'tis new, and because having purposely tri'd it in large and thick funnels of glass, it may be pertinently enough deliver'd in this place, (where we are treating of the Transmission or Propagation of Cold, through close and thick *Mediums*,) *then* because we expect to make of it that use, especially that Oeconomical use, that has been lately intimated. For first, 'twill be very hard to prove, that 'tis the very Air it self, and not rather the vapours swimming in it, that are by this means transmuted into water. And secondly, 'tis true indeed, that a mixture of snow and salt will condense vapours on the outside of a Funnel, but either they, that hop'd to make this use of the Experiment, have little Experience of it, and write conjecturally, or else / they have made it with a success very differing from ours. For though, we imploy'd a large Funnel, and suspended it by a string (artificially enough ti'd about it) in the free Air: And though the mixture of ice and salt we put in, were sufficiently infrigiding (as will appear by and by) and far more so, then ice or snow alone would have been, yet that mixture being not able to condense the vaporious Parts of the Air into dew, much, if at all, longer then the mutual Dissolution of the salt and snow lasted, the liquor that was this way obtain'd, and dropp'd down at the bottom of the Funnel (whose internal Perforation ought to be carefully stopp'd, least any of the resolved snow and salt should fall through, and spoil the other liquor) was indeed sweet like rain water, but so very little, as well, as so slowly generated, that it amounted not any thing near to that which the snow, imploy'd and spoil'd to make it, would have afforded. So that it may be questioned, whether some cooling liquors, which can as well as this mixture condense / the vapid Air into water, and whose Texture is not destroy'd in this operation, as that of the snow is, might not be more hopefully imploy'd to obtain water from the Air; to which I shall only add this one thing, That the mixture of snow and salt did turn the vapours, that fasten themselves to the outside of the glass, first into Ice, before they dropt down in the form of water; in almost all our Trials of this nature, as well in thick Funnels, as in other and thinner glasses.

5. That in *Hermetically* seal'd glasses, an included mixture of snow and salt

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will freez the vapours of the Air on the outside of the glass, divers of the Experiments of the present Treatise do manifestly evince, which argue, that even so extremely close a *Medium* as Glasses, is not able to hinder the Transmission of Cold. And this is not superfluously added, because in vessels not *Hermetically* seal'd, it may be pretended, that 'tis the internal Air that communicates its Coldness by some unheeded, but immediate intercourse, with the external. /

After this we thought it worth an Experiment, to try, whether, or how, Cold would be diffused through a *Medium*, that some would think a *Vacuum*, and which to others would seem much less disposed to assist the Diffusion of Cold, then common air it self; to compass this, the Expedient we bethought our selves of, was, to suspend a slender glass full of water in one of the small Receivers belonging to our Pneumatical Engine, and when the Air was very carefully pump'd out, to bury the exhausted Receiver in a copious and ready prepar'd mixture of Ice and Salt, to see, whether notwithstanding the withdrawing of the *Medium*, the water suspended in a kind of *Vacuum*, as to Air or gross substances would yet be frozen by the Cold. That Event of our trials, which alone I find among my Notes, is registred in these terms.

6. [A small pipe seal'd at one end, was, at the other, fill'd almost with water, and was put into a Receiver, consisting of a somewhat long and slender Tube of Glass, seal'd at one / end, and inverted upon the Engine plate, then the Air was carefully exhausted, for the pump was ply'd a while after no Air appear'd to come forth in any bubble out of the Receiver, through the external water; nor did the water in the small pipe within, disclose any number of bubbles worth taking notice of: then by the help of an almost Cylindrical plate of Iron, beaten Ice and Salt, were heap'd against the outside of the Receiver, about the height, to which the water in the small pipe reach'd. And at length, though, as we all thought, much more slowly then such a Congelation would else have been perform'd, the water was for the most part frozen in odd kind of flakes from the top to the bottom, and the ice seem'd not to have any considerable number of Bubbles.]<sup>†\*</sup>

7. There is one Experiment, I have made about the Transmission of Cold through indispos'd *Mediums*, which may not be unworthy to be here inserted. For I had once a mind to try, whether a cold Body could operate through a *Medium*, that was, as / to touch, actually hot, and had its heat continually renew'd by a fountain, as it were, of heat, that perpetually diffus'd through it, new supplies of warm Liquor, so that the cold Body could not here, as in other cases, first allay the heat of the *Medium*, and then lessen it more and more, till it had quite extinguish'd it. To compass this, I had soon after an opportunity of making some trials presented me: For being at the Mineral Springs at *Tunbridge*, to drink those wholesome waters for my healths sake, I soon accustomed my self to drink them in considerable Quantities very early in the morning, when they were exceeding Cold, and sometimes drinking them in bed, as well as sometimes at the Springs-head, I had the

Curiosity to observe, whether in case I took them down very fast, they would not through the warm Muscles and outward Parts of the Abdomen, diffuse a sensible Coldness; and upon more Trials then one, I found, that by laying my warm hands on the outside of my Belly, I there felt at least, as it seemed / to me, a manifest and considerable Degree of Coldness. And when I related this to some ingenious Persons, that were better acquainted with those Springs then I, they told me, that there was among those many that then resorted to those famous Springs, a Knight, whose Name I remember not, whose Disease being judg'd formidable, the Physicians enjoyned him to drink in a morning two or three times the Quantity, that afforded me the Observation I was relating, and that when this Knight had fill'd his Belly with so much water, he us'd mightily to complain of the Coldness it diffus'd through his Abdomen, insomuch that he was fain to ply those parts long with hot Napkins clapp'd to them, one after another, which yet, as he complain'd, were soon refrigerated by the excessive Cold that the water diffus'd to the outside of his Belly, which yet nevertheless was not, that I could learn, at all prejudic'd, no more then mine, by so sensible and piercing a Cold.



8. It may be doubted, whether in / case water be not fluid upon the account of a congenite motion in the Corpuscles it consists of, its fluidness may not proceed from the agitation of the ambient Air, either immediately contiguous to the surface, or communicating its agitation to the water, by propagation of its Impulse through the vessel that interposes betwixt them. To contribute to the clearing of this, and some other things, we devis'd the following Experiment. We provided a glass-bubble of about the bigness of a Walnut, and the form almost of a Pear, whose stem was purposely made crooked for the conveniency of suspension. This being fill'd with water (which is troublesome enough to be done, unless one have the knack) we hung it at one end of a thread, whose other end we past through a Cork, by a perforation purposely made: into which, we afterwards fastned the thread, by thrusting in a small peg to rivet it in. Then filling a glass not very broad, but yet furnished with a mouth wide enough to receive the bubble, with oyl of Turpentine, / such as we bought it at the shops, we stopp'd the orifice with the newly mention'd Cork, so that the seal'd Bubble hanging at it, was covered, and every way surrounded by the oyl of Turpentine, which being a liquor, that (at least in such Colds as we here have) will not freez, we plac'd the glass in beaten Ice and Salt, and as it were buri'd it therein, and at the end of about three hours (having been diverted by some occasions from taking it sooner out) we found, as we had conjectured, that notwithstanding that, the oyl of Turpentine continued perfectly fluid as before, yet the Bubble totally immersed in this heating Chymical oyl, was frozen throughout, not excepting that which was harboured in the little Neck or Stalk, and when I came to lift it out of the liquor, the glass being crack'd (as we supposed by the Cold) the string brought up a little part of that which was nearest to it; the rest in the form above mentioned, staying behind and subsiding. And that which was remarkable in this piece of Ice, was, that / when we had taken it out, it

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appeared cleft very deep (from the outside almost to the centre) according to a line drawn from the slenderest part of it, almost as if one should with a knife cut a Pear in two, from the stalk downwards, according to its whole length. And these two pieces were easily enough separable, and (to adde that circumstance) for trial sake we left them divided in the same liquor and vessel, with some thawing Ice and Salt about them, for 14. or 15. hours, without finding them any thing near so much wasted or resolved into water, as most would have expected.

Whilst the above mentioned Bubble was exposed to be frozen, we likewise placed by it in another vessel a Glass-Egg, whose Ball and a little part of its stem we had fill'd with some of the very same parcel of oyl of Turpentine, and placing about the sides of this Egg some ice and salt, we observed, as we expected, that the liquor was, after a little while, made by the Cold to subside about half an inch, so that 'tis worth some / Philosophers considering, why, if according to the lately mention'd Atomical doctrine, Cold be made by the introduction of swarms of real and extended, though Atomical Bodies, they should pervade the oyl, and contract it without freezing it, but freez the water without contracting it, but expending it rather.

9. [A small bubble of the bigness of a very little Nutmeg, fill'd with water, and Hermetically seal'd up, was by a cork and a string suspended in spirit of Wine, so as to be surrounded therewith, and being exposed to the Air the same night, in the stopt glass, was the next morning found altogether frozen, though the spirit of Wine it self were not at all so: But another bubble, by the help of a string Cork, and piece of Lead, carefully suspended in a strong solution of Sea-salt, and exposed at the same time in a like vessel with the former, when they both came to be look'd upon, appear'd to be no more frozen then the brine it self, which was not so at all.]<sup>†\*</sup>

10. [A glass Bubble of the bigness / of a small Nutmeg, fill'd with water, and Hermetically seal'd, being immersed by a weight of Lead fastned to it, beneath the surface of a very salt Brine, but yet not so as to reach the bottom of the liquor or glass, was exposed all night to freez, in weather that was extraordinarily cold, but neither the imprison'd water, nor the other appeared to be at all frozen. The like Experiment we repeated another frosty night, but without freezing either of the liquors. But

to show the usefulness of repeating Experiments about Cold, if there be opportunity, and especially in such cases, where the degree or some other circumstance may much vary the event, we will add, that having exposed a Bubble like that newly mention'd, and immers'd in spirit of Wine, we found the next morning the water in the bubble turn'd into ice, and having likewise exposed such a bubble immers'd in very strong Brine, to be frozen by a mixture of ice and salt, within about two hours after, we found the bubble broken, / as we suppos'd, upon the Expansion of the water upon its growing Ice. And we also found the upper part of the bubble within the Ice sticking to it, and the other part

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of the glass was crack'd, with lines running from a point almost like the Pole and Meridian in a Globe, whence we concluded the glass to have been, as 'tis probable, burst asunder upon the Expansion of the fresh water into ice, and that the Reason why there remain'd but a comparatively little parcel of ice, was probably, that the salt water getting in at those crannies or chinks, dissolved as much of the new made ice, as in a little while it could easily reach.]

*Besides,*

11. [We fill'd a glass bubble with fair water, and having Hermetically seal'd it, we suspended it by a string fastned to the cork in the cavity of a wide mouth'd glass, well stopt, so that the bubble was every way at a good distance from the sides, bottom, and top of the glass. This we did to try, whether a sufficient degree / of Cold at that distance, would be freely transmitted through the glass, without the intervention of a visible liquor, and accordingly we found the suspended Bubble crack'd by the ice that fill'd it.] /

## **Title XV.**

### ***Experiments and Observations touching Ice.***

1. A Great part of our present History, being imploy'd about delivering the *Phænomena* of Congelation, it is not to be expected, that in this Section, where we treat of Ice as a distinct part of our Theme, we should deliver all those particulars, that have occur'd to us, wherein ice is concern'd. And therefore we shall restrain our selves to the mention of those, that belong to ice, considered, as it consists of intire and distinct Portions of congeled water. And though we shall deliver some few Experiments of our own, such as we had any opportunity to make, yet much the greater part of this Section will fitly enough be taken up by Collections / out of Travellers, and Navigators, into those Colder Regions, that afford much considerabler, or at least much stranger Observations concerning ice, then are to be met with in so temperate a Climate as ours. And what we have to deliver in this Section, will naturally be divided into two parts, the one consisting of our own Experiments, and the other containing some Passages, that we have selected out of Voyages, or that have been afforded us by the Relations of credible Travellers. And of these two sorts of Observables, that which has been first mention'd shall be first treated of.

2. Some that have been in the *East Indies* inform us, that in some parts of those Countries, they were looked upon as great Liars, for affirming, that in *Europe* the fluid body of water, was often without any artifice or endeavour of Man, turned in a few hours into a solid and compact Body, such as Ice. And certainly, if custom did not take away the strangness of it, it would to us also appear very wonderful, that so great a / change of Texture should be so easily and inartificially produced. But how solid the Body of ice is, or rather

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how strong is the mutual adhesion of its parts, has not yet, that we know of, been attempted by Experiments to be reduced to some kind of Estimate; and indeed so many things must be taken into

consideration, that it will be difficult to arrive at any more then a fair conjecture in this matter; especially, because (I think) it may justly be doubted, whether or no differing degrees of Cold may not vary the degree of compactness of the ice, and my doubt will not perhaps appear groundless, if I add, that having, to satisfy my self, inquired of an intelligent Person, that liv'd some years in *Russia*,<sup>†\*</sup> he answered me, that he found the ice of those parts to be much harder then that of these.

3. We had in our thoughts divers ways to Estimate the cohesion of the parts of ice, whereof one was, to freeze water in a hollow metalline Cylinder, and taking out the ice, and keeping it in a Perpendicular posture / cast into a scale weigh'd beforehand, and carefully fastned to the bottom of the ice, more and more weight, till the mere weight broke the Cylinder, and this we had thoughts to try in Cylinders of differing Diameters and lengths, but wanted conveniencies to make the Experiments; (which if they were made (as some of our Trials were) in the open Air, and in places exposed to some gelid wind, it would be better secure the ice from being weakned or thaw'd during the Trials.)

4. We therefore attempted by another way, to investigate the strength of ice. For we took a plate of it, of an uniform, and also of a considerable thickness, and with sides cut parallel, that it might serve for a kind of leaver, and plac'd it betwixt two wooden Bars, whose distance we knew, and then laying on it a great weight, the Centre of whose Pression, as near as we could estimate, was equally, or in determinate measures, distant from the wooden *fulcrums*: we endeavoured to try, how / great a weight it would support; but in the Village, where we made the trials, we could not get weights that were conveniently shap'd, and ponderous enough, to break it, and though we caused a Man to stand upon it, yet neither could his weight break it, till he chanced to add an impressed force with his foot, to the weight of his Body. So that being unable to determine, what that additional and impressed force might amount to, almost all that we could safely conclude, either from this Experiment, or some other ways of trial with scales, and other other ways that we made use of (but for want of conveniencies unsuccessfully) was, that the force of ice to support weights, is much greater then men are wont to imagine, which seems somewhat the more strange, because it is not here in *England* so solid a Body, as by this one would guess: for not only glass would readily scratch it deep enough, but even with common Knives we would cut it, and that with great ease.

5. Yet one not inconsiderable Account / I was able to give my self of the strength of ice, which I find in my Notes thus delivered.

[There was taken a piece of ice three inches long, and three broad, and somewhat less then a quarter of an inch thick; this was laid crossways upon a

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frame, so that the two parts, on which the ice lean'd, were distant three inches, then there was taken an Iron, shap'd like the figure of (the common Arithmetical Cypher, that denotes *Seven*) 7, to whose hanging leg, if I may so call it, there was fastned at the end, which was under the middle of the ice, a scale, into which several weights were put, such as by some former Trials we guess'd to be almost as much as the ice would well suffer, after which the horizontal leg of the Iron was very gently laid upon the ice, as near as we could guess, in the middle of the distance, between the two sides of the frame, and consequently parallel to them both, then the weights not proving altogether sufficient to break the ice, we let them hang a while at it, and observ'd how / the edge of the incumbent leg of Iron (which edge was <sup>†@</sup> broad) did work it self downwards into the ice, so that by our guess, when the ice broke, as after a while it did, it had lost at one end of the Incision, if I may so call it, half its thickness, and at the other, about a third part of it.<sup>†\*</sup>

The weights that broke it, amounted to 17. pounds *Haberdupois*, and 117. ounces *Troy*.

6. The Experiment was repeated with all the former circumstances, only the piece of Ice was two

inches and a half broad, and a quarter of an inch thick, the distance of the frame was three inches, as before, the weights that broke it, were 17. pounds *Haberdupois*, and 48. ounces *Troy*. The horizontal arm of the iron had melted somewhat more then half through the ice when it broke, *viz.* more then  $\frac{2}{3}$  of the thickness at one end, and somewhat less then half at the other.

7. We divers times intimated in some of the first Sections of our present History, that the addition of salt to Ice, did hasten the dissolution of it, / which though it may be easily proved by some other *Phænomena* of our Experiments, yet it will not be amiss to mention here a couple of particular trials, by which we have more manifestly evinc'd it: And first, we divers times took a broad and flat plate of ice, less then a  $\frac{1}{4}$  of an inch thick, and having placed it horizontally upon a joyn'd-stool, (a table, or any other flat piece of wood will do as well) we strewed here and there a convenient quantity of Bay-salt upon it, and though we observed, that, if the surfaces of the ice and stool, were not both of them flat, and congruous enough, the ice would be thaw'd indeed, but the other part of the Experiment would not well succeed; yet when we made the trial carefully, and watchfully, the plate of ice partly thaw'd by the salt, would be so firmly frozen to the stool it leaned on, that we were fain with an iron instrument, to knock it all to pieces, before we could sever it from the stool, into whose pores the ice newly generated by the Experiment, did pierce so deep, that notwithstanding / our knocking, many little parcels of ice would continue to stick close to the wood, whose pores

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they had invaded. But the circumstances which in this Experiment made the most to our purpose, are these two; The one, that having sometimes laid the salt but on few, and somewhat distant parts of the plate, the intermediate parts would many of them remain unfrozen to the stool, whilst those, where the salt had been laid, were frozen so hard to it. And the other circumstance is, that the grosser grains of salt, would so far dissolve the ice whereto they were contiguous, as (if I may so speak) to bury themselves therein, whilst the other parts of the ice, upon which, or near which, no salt had been laid, kept their surfaces smooth and intire. We tried likewise two or three times to freez a plate of ice to a flat piece of wood, by making use of *Aqua fortis*, instead of common salt, but the Experiment succeeded not well, though once we brought the ice to stick to the wood manifestly, but not strongly. /

8. To this we shall add, the following Experiment, which when we watchfully made it, succeeded well, and I find it among my notes set down in these terms.

[Solid fragments of ice having pretty store of salt thrown on them, upon the first falling of the salt among the ice, there was produced a little crackling noise, and for a good while after there manifestly ascended out of several parts of the mixture, conveniently held betwixt a candle and the eye, a steam or smoak, like that of warm meat, though the night were rainy and warm, and though the morning had not been frosty.]

The mention here made of the crackling noise made by the ice upon the addition of salt, (which seemed to proceed from the crackling of the brittle ice, produc'd by the operation of the salt upon it) brings into my mind an Experiment I had formerly made, whereof a greater noise of the same kind is a *Phænomenon*: though the Experiment were chiefly made for the Discovery of the texture of Ice: The event of the trial I find thus set down among my notes. /

9. [We took some cakes of ice, each of the thickness between an  $\frac{1}{8}$  and a  $\frac{1}{4}$  part of an inch, but not so very compact ice, as to be free from store of bubbles; some good *Aqua fortis* dropp'd upon this, did quickly penetrate it with a noise, that seem'd to be the cracking of the ice, underneath which the sowre liquor was very plainly to be tasted; Oyl of Vitriol did the same, but much more powerfully, and without seeming to crack the ice which it past through; so that though but three or four drops were let fall upon the plate, it immediately shew'd it self in drops exceedingly corrosive on the other side of the ice. And the like success we had with a trial made with the same liquor upon three such



plates of ice frozen one upon the top of another.]<sup>†\*</sup>

10. Having proceeded as far as we were able towards the bringing the strength of ice to some kind of Estimate, by such Experiments as we had opportunity to make here, we thought it not amiss to seek what information we could get about this matter among the Descriptions that / are given us of Cold Regions: But I have not yet found any thing to have been taken notice

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of to this purpose worth transcribing, except a passage in the Arch-Bishop of *Upsal*, wherein though the estimate of the force of Ice be, as we shall by and by show, made after a gross manner, yet since this it self is more then I have met with elsewhere, I think it worth subjoyning, as our Author delivers it in these terms:<sup>†@</sup> *Glacies* (says he) *primæ & mediæ hyemis adeò fortis & tenax est, ut spissitudine seu densitate duorum digitorum sufferat hominem Ambulantem, trium vero digitorum equestrem Armatum; unius palmæ & dimidiæ, turmas, vel exercitus militares; trium vel quatuor palmarum integram Legionem seu myriadem populorum, quemadmodum inferiùs de bellis Hyemalibus memorandum erit.*

But though this be sufficient to afford us an illustrious Testimony of the wonderful strong cohesion of the parts of ice, yet we mention'd it but as a popular way of estimate, which may better embolden Travellers, then satisfie Philosophers, in regard / that the Author determines only the thickness of the ice, and not the distance of that part of it, that supports the weight from the shore or brink, on which, as on a *Hypomochlion*,<sup>†\*</sup> the remotest part of the ice does lean or rest. And if we consider the ice as a Lever, and the Brink or Brinks on which it is supported, as a single or double *fulcrum*, the distance of the weight may be of very great moment in reference to its pressure or gravitation on the ice, which may much more easily support the weight of divers men plac'd very near the prop, then that of one man plac'd at a great distance from it, as will be easily granted by those, that are not strangers to the Mechanicks, especially to the nature and properties of the several kinds of Levers. But not now to debate, whether in certain cases, the ice we speak of, may not receive some support from the subjacent water, nor whether some other circumstances may not sometimes be able to alter the case a little, our very considering the ice as a single or double Lever, though it may hinder / us from measuring the determinate strength of ice upon *Olaus*'s Observation, yet it will set forth the strength of it so much the more, since by his indefinite expressions he seems sufficiently to intimate, that when the ice has attain'd such a thickness, its resistance is equivalent to such a weight, without examining on what part of the ice it chances to be placed.

11. Thus far our Experiments concerning ice (with the Appendix subjoynd out of *Olaus* to the same purpose.) We will now proceed to some of the observations we have met with in Seamens Journals, and elsewhere. I say to *some*, because to enumerate them all, would spend more time and labour then I can afford, and therefore I shall restrain my self to the mention of some few of the chiefest.

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I. And in the first place for confirmation of what I deliver'd at the beginning of this Section, from the report of a Traveller into *Russia*,<sup>†\*</sup> touching the hardness of ice in those gelid Climates, in comparison of our ice, which I have found it easie / to scrape with glass, or to cut with a knife; I shall subjoyn this passage of Captain *G. Weymouth*, in his Voyage for the Discovery of the Northwest passage.<sup>†@</sup> *As we were* (says he) *breaking off some of this Ice, which was very painful for us to do, for it was almost as hard as a rock, &c.*

II. Next to shew, that it was not a superfluous wariness, that made me in a former Section doubt, that even the ice made of Sea-water might be altogether or almost insipid; I will subjoyn, that I have

since met with some Relations, that seem to justify what is there deliver'd. And in one of our Englishmens Voyages into the Northern Seas, I find more then one instance to my present purpose, though I shall here set down but one, which is so full and express, that it needs no companions: Our Navigator speaking thus;†@ *About nine of the Clock in the forenoon, we came by a great Island of Ice, and by this Island we found some pieces of Ice broken off from the said Island, and being in great want of fresh water, we hoysed out our Boats of both Ships, and loaded them twice with Ice, / which made us very good fresh water.*

But all this notwithstanding, I yet retain some scruple, till those that have better opportunity to make a more satisfactory Experiment shall ease me of it. For though by these Narratives it seems more then probable, that the ice in the midst of the Sea consists but of the fresh Particles of water, that plentifully concur to compose the Sea water, yet besides that, in case the fresh water were taken, as some of that, I have found mentioned in Voyages, has confessedly been, from the top of the ice, it might possibly be no more then melted snow, which, as we elsewhere take notice, does in those extremely cold Regions easily freez upon the ice it falls on, and oftentimes much increases the height of it: Besides this, I say, the Argument from the insipidness of the resolved ice, will conclude but upon supposition, that as that ice was found in the Sea, so it was also made of the Sea water; which though it may have been, yet I somewhat doubt, whether it were or no, since I find some Navigators of the / most conversant in the cold Climates to inform us, That most of those vast Quantities of ice that are to be met with about *Nova Zembla*, and the strait of *Weigats*, and that choke up some other passages, whereby men have attempted to pass into the south Sea, are compos'd of the accumulation of numerous pieces of ice (cemented by intercepted and then frozen water)†\* that are brought down from the great River *Oby*, and others, so that it may

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very well be suppos'd, that these †@ mountainous pieces of ice may be some of these, which, upon the shattering of ice in Bays and straits, partly by the heat of the Sun, and partly by the Tides, may be afterwards by the winds and currents driven / all up and down the Seas, to parts very distant from the shore, and some of these it may be, that our Countreymen met with, and obtain'd their fresh water from: Which I the rather incline to think, because that (as we shall have occasion to observe in another Section) the main Sea it self is seldom or never frozen. But my scope in all this, is, but to propose a scruple, not an opinion.

III. The next and principal thing concerning ice, is the bigness of it, which I find, by the Relations partly of some Acquaintances of my own, and partly of some Navigators into the North, to be sometimes not only prodigious, but now and then scarce credible. And therefore, as I shall mention but few instances, that I have selected out of the best Journals, and other writings I have met with, so I shall add a few more Testimonies to keep them by their mutual support, from being entertain'd with a Disbelief, which their strangeness would else tempt men to.

Of the vastness of single mountains of ice, the most stupendious Example, / that for ought I know, is to be met with in any language but ours, is that, which I formerly took notice of out of the Dutch Voyage to *Nova Zembla*,†\* which was ninety six foot high (that is above twenty foot higher, then on a certain occasion I found the Leads of *Westminster Abbey* to be.) But 'tis probable, that our Captain *James* met with as great, if not greater: For though in some places he mentions divers hills of Ice, that were aground in 40. fathom water, and consequently were as deep under water, as that newly taken notice of out of the *Hollanders*.†@ And though he elsewhere mentions other pieces of no less depth, and twice as high as his top-Mast head, and this in *June*, yet elsewhere, and long after relating his return home, he has this passage;†@ *We have sail'd through much mountainous Ice far higher then our Top-Mast head: But this day we sail'd by the highest that I ever yet saw, which was incredible indeed to be related.*

But the stupendiosest piece (for heighth and depth) of single Ice, that perhaps has been ever observ'd and / measur'd by men, is that which our Famous English Seaman Mr. *W. Baffin* (whose name is to be met with in

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many modern Maps and Globes) mentions himself to have met with upon the coast of *Greenland*, whose whole Relation I shall therefore subjoyn, not only because of the stupendiousness of this piece of ice, but because he takes notice of an observation, which I knew not to have been made by any, and comes somewhat near the estimate, we formerly made, of the proportion betwixt the extant and immers'd parts of floating ice, only the following Estimate makes the extant part somewhat greater then we did, which may easily proceed from other mens having, as Mr. *Baffin* here does, grounded their computation upon what occur'd to them at Sea, or in salt water, where the ice must sink less, then in fresh water, such as my Estimate suppos'd. Our Navigators words then are these,†@ *The 17. of May we sail'd by many great Islands of Ice, some of which were above 200. foot high above water, as I prov'd by one shortly after, which I found to be / 240. foot high, and if the report of some men be true, which affirms, that there is but one seventh part of Ice above water, then the height of that piece of Ice which I observed was one hundred and forty fathoms, or one thousand six hundred and eighty foot from the top to the bottom. This proportion I know doth hold in much Ice, but whether it do so in all, I know not.*

Thus far of the height and depth of single pieces of ice: as for the other Dimensions (the length and breadth) I remember not, that I have read of any, that had the Curiosity to measure the extent of any of them, excepting Captain *James*, whose Ship being once arrested, between some flat and extraordinary large pieces of ice, he and his men went out to walk upon them, and he took the pains to measure some of the pieces, which he says he found to be a 1000. of his paces long.†@ And probably among so many mountains and Islands of ice, there would have been found some intire pieces, of a greater extent then even these, if men had had the curiosity to measure them.

Hitherto we have treated of the / bigness of single pieces of ice, we will now proceed to say something of the dimensions of the aggregates of many of them, among which having selected four or five as the principal, I remember my self to have yet met with, I presume it will be sufficient to subjoyn them only.

*About ten of the clock we met with a mighty bank of ice, being by supposition seven or eight leagues, or twenty four miles long, (says that experienced English Pilot James Hall, in his Voyage of Denmark for the discovery of Greenland.†\*)*

Another of our English Navigators mentions, that even in *June all the Sea* (wherein he was indeavouring to sail) *as far as he could see from the top of a high hill, was covered with ice, saving that within a quarter of a mile of the shore it was clear round about once in a Tide.†\** By which last clause, it seems, that this vast extent

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of ice, was either one intire floating Island, or at least a vast bank or rand (as some Seamen term it) of ice.

But the strangest account of banks of ice, that I have yet met with in / any sober Author, is that which is mention'd by the learned French *Hydrographer, Fournier*, who relates, that in the year 1635. the French fleet sailing to *Canada*, met with several pieces of ice, as high as steeples, and particularly one, whether piece or bank of ice (for the French word *Glace* may signifie either) which they were troubled to coast along for above forty leagues. If this be the same story, (as one may suspect it to be, by the circumstances of the place, and fleet,) there is a great mistake in another



place, where our Author speaks of the vastness of the ice: but if it be another story (as some differing circumstances argue) the French it seems met with ice far more stupendious, then even that already mentioned. For, (says our Author) in the Sea which washes *Canada*, there is often seen, even in the moneth of *August*, to pass by, Ices much bigger then Ships.<sup>†@</sup> In the year 1635. the French fleet sailing there, coasted along, for three days and three nights, one that was above 80. leagues long, flat in some places like / vast Champions, and high in others like frightful hills. The latter part of which passage may confirm what we formerly deliver'd in another Section, concerning the unequal compagination of Icy Islands.

To what has been said touching the extent, and other dimensions of floating, or at least loose pieces of ice, it will be fit to add something of the extent of ice, coherent to one or both of those shores, that bound the water, whose upper part is congeal'd. And in the first place, we shall out of many instances to our present purpose, that might be borrowed from the writings of *Olaus Magnus*, select this one memorable one that shall serve for all:<sup>†@</sup> *Neque minori bellandi impetu* (says he) *Sueci ac Gothi super aperta glacie, quam in ipsa solidissima terra confligunt; imo, ut prius dictum est, ubi antea æstivo tempore acerrima commissa sunt bella Navilia, eisdem in locis glacie concreta, aciebus militari modo instructis, Bombardis ordinatis, habentur horrendi conflictus. Adeo solida glacies est in equestribus turmis sufferendis, ampliter vel strictè collocatis.* / I pretermitt then, what he elsewhere relates of the Voyages and Wars made in Winter by the Northern Nations. They that have liv'd in those Countries, relate, as things most known and familiar (what has been confirmed to me by more then one unsuspected eye witness) the long Journeys that are commonly taken upon the Icy Bridges, or rather plains, by travellers, with all their Carriages to very distant places. And that which may bring credit to these strange relations, by shewing, that no less unlikely ones are sometimes true, is, what all *Europe* knows, that within these three years the whole *Swedish Army*, led on by their King, march'd over the Sea to the Island of *Zeeland*, where *Copenhagen* the

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Capital City of *Denmark* stands.<sup>†@</sup> But it may seem much more strange, which I will therefore add, that as in the North Countries frequently, so sometimes even in the warmer Regions of the East, the Sea it self, has by / the Cold, been congeal'd to a prodigious breadth. *Insolitum est*, (saith *Bartholinus*<sup>†@</sup>) *quod refert Constantinus Manasses in Annalibus accidisse, Theophilo imperante, ut hyems sæva mare cogeret in glaciem ad profunditatem sanè immensam, humidúmque illud Elementum, Lapidis ad duritiem, fluxione prorsus ademptâ redigeret.* And *Michael Glycas* relates,<sup>†@</sup> *That in the year 775. the Winter was so sharp in the East, that along the Coast, the Sea (he means the Mediterranean) was frozen for 50. leagues, and the Ice was compacted as into a rock, 30. Cubits deep; so strange a Quantity of snow, likewise falling, that it was rais'd to the height of 30. Cubits above the Ice*, which likewise agrees very well with what we formerly noted, touching the possible increase of the height of some pieces of ice by the falling of the snow upon them.

IV. It remains now, that we subjoyn a few promiscuous observations concerning ice, that are not so readily reducible to the three foregoing heads.

And we shall begin with what was taken notice of by the *Dutch* in their / *Nova Zembla Voyage*,<sup>†\*</sup> where relating how they fastned their Ships to a great piece of ice, to shelter themselves from the stormy winds, *There* (add they) *we went upon the ice, and wondred much thereat, it was such manner of Ice: for on the top it was full of earth, and there was found about forty eggs, and it was not like other ice, for it was of a perfect Azure colour, like to the skies, whereby there grew great contention of words amongst our men, some saying that it was ice, others that it was frozen land; for it lay unreasonable high above the water, it was at least eighteen fathom under the water, close to the ground, and ten fathom above the water.*

The like blew colour in rocky pieces of ice, I remember I have somewhere found, to have been



taken notice of by a modern Navigator,<sup>†@</sup> or whether the words of *Virgil*, concerning the frigid Zone, *Cærulea glacie concretæ, atque imbribus atris*,<sup>†\*</sup> belong to this subject, I leave others to consider, nor shall I stay to examine, whether this blewness, that has been observ'd in ice, be always an

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inherent or permanent colour, or else sometimes / one of those that are styl'd Emphatical.

'Tis very considerable, if it be true, what is related by *Olaus Magnus*, concerning the degenerating (if I may so speak) of ice, from its wonted hardness in the Spring of the year. For in the same Chapter, where he gives us the lately transcribed account of the strength of Ice in those Northern Countries, after having interpos'd some other passages, he subjoyns these words;<sup>†@</sup> *Liquescente tamen glacie ad principium Aprilis, nullus ejus spissitudini, minus fortitudini, nisi in aurora, ambulando confidit, quia solis diurno aspectu tam fragilis redditur, ut quæ equestres armatos paulo ante portaverat, vix hominem nunc sufferre possit inermem.*

This puts me in mind to add, that oftentimes in the writers of Journies and Voyages, we meet with mention of great noises made by the breaking of ice, and in this very Chapter our Archbishop taking notice of the clefts that sometimes happen in Champions of ice, adds,<sup>†\*</sup> *That when the ice chances thus to open, especially if / it be in the night, the noise of it may be heard a far off, like the loud and horrid noise of thunder, and of earthquakes.* And on this occasion may be subjoyned a couple of passages extant in different places of the formerly mention'd *James Hall's Voyages*.<sup>†\*</sup> The first is thus delivered; *When we met with a huge and high Island of ice, we steering hard to board the same, and being shot a little too Northwards of it, there fell from the top thereof, some quantity of ice, which in the fall did make such a noise, as though it had been the report of five Canons.* But the next passage is more directly pertinent to our present subject, and is couch'd in these words; *About twelve of the clock this night, it being still calm, we found our selves suddenly compassed round about with great Islands of ice, which made such a hideous noise, as was most wonderful, so that by no means we could double the same to the Westward, wherefore, &c.*

Of these kind of *icy thunders* (as some travellers call them) there are divers instances to be met with, mention'd in the several Voyages of the *Hollanders*, & particularly in those / to *Nova Zembla*: But many of those noises seem to be made by the dashing of the great pieces of ice against one another: But if it happen, when the ice (as sometimes it is said to do) seems to cleave, as it were, of its own accord; to us that live in a temperate Climate, it may be a matter of some dispute, whence these loud ruptures of ice may proceed. For *Olaus Magnus*, in the Chapter above cited, does not improbably ascribe them to the warm exhalations, that in some places ascend out of the ground. And I remember, in favour of this opinion, that I once caused divers pieces of thick ice to be brought out of a cool place into a somewhat warm room, and listening, observ'd a noise to come from them, as if it had been produced by store of little cracks made in them, but somewhat or other prevented me

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from repeating the Experiment, and satisfying my self about the Conjecture. But having lately inquired of an intelligent *Polander*, that has travelled much upon these icy plains, he agreed with our Author, and / others, as to the frightful noise, that are produc'd by these cracks of ice, but affirm'd upon his own observation (for that I particularly inquired after) that these great clefts were often made, not by thawing heat, but by excessive cold, and that he had taken notice of them in extremely sharp weather.<sup>†\*</sup> Indeed we sometimes observe, that in very bitter frosts the frozen ground will cleave, as we elsewhere have occasion to take notice. But whether that be not a different case from this, or whether the *Polonian Gentleman* were not mistaken, or whether both

these mention'd accounts of the cleaving of ice, may on different conjunctures of circumstances take place, we leave to farther inquiry.

There is a tradition concerning ice, about the famous *Volcan-Hecla*, in *Island*, which, though verily believ'd among the superstitious vulgar of those parts, is spoken of so slightly by *Blefkenius*,<sup>†@</sup> who being upon that coast, had the curiosity to sail purposely thither, that I think it not worth while to take any farther notice of it. But / 'twere too tedious to set down in this Section, (which the strangeness and variety of the Theme has made so prolix already) the other things, that may be mentioned without impertinency concerning ice; and therefore we shall here desist from so laborious a task, as also omit the handling of snow and hail: For though they are reducible to ice, yet I shall at least suspend the treating of them, partly because *Bartholinus* and *Meteorologists* have sav'd much of my labour,<sup>†\*</sup> and partly for the reason newly intimated, so that we shall conclude this Section as soon as we have taken notice, that there is yet somewhat relating to ice, which, being in it self considerable, and whereof hitherto no experimental account appears to have been given, what we our selves have tried about it, may challenge to be treated of apart. /

## Title XVI.

### *Experiments and Observations touching the duration of Ice and Snow, and the destroying of them by the Air and several Liquors.*

1. It may be an Experiment, as well instructive as new, to determine, what liquor dissolves ice sooner then others, and in what proportion of quickness the solutions in the several liquors are made. For Men have hitherto contented themselves to suspect in general, that there are other liquors potentially hot, wherein ice will sooner dissolve, then it will in water. But this

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opinion either being grounded upon no Experience at all, or taken up upon the sight of what happens to pieces of ice, which no care was taken to reduce / to the same bulk and figure, no more then to measure attentively how long one outlasted the other; we thought fit to try, if we could not bring this matter to Experiment, and make a determination in it, though not exactly true, yet *less* remote from exactness then had been yet, for ought I know, so much as attempted.

2. In order to this we procured some bullet moulds, and having first carefully stopped the little Crevice, that is wont to remain betwixt the two halves of the mould, with a good close Cement, we afterwards filled them with water, and carefully closed up the orifice of the hole, at which the water was poured in, and then setting the mould to freez in ice and salt, we found it difficult enough to keep the water (more of less of it) from running away through some unperceiv'd passage, before the cold could have time by congealing it to arrest it. But after a while, when we had thus made a bullet of ice, we found it a new and greater difficulty to get it whole out of the moulds, / without warming them, for by that way we could indeed loosen the ice, but then we could not avoid thawing it too, and that most times not uniformly: wherefore we tried by greasing the inside of the moulds to keep the ice from sticking so close to them, (notwithstanding the distention the water suffered by its being frozen) but that we might pick out the bullet entire, and this succeeding well enough, we hoped by this way to obtain our end, which was to have a competent number of pieces of ice of equal bulk, and of the same figure to be put at once to thaw in several liquors; but we could by no means procure moulds, which had any number of distinct cells of the same bigness, those long pairs of moulds that were to be met with in shops, having their distinct cells generally made on purpose of very different bignesses, which rendred them altogether useless for our design. Wherefore we were fain, for want of an exacter way, to take a glass pipe of the most even and Cylindrical that we had, and of a bore capable to admit / a big mans little finger, this glass being stopt at one end, and kept open at the other, was filled to the height of about half a foot or more of fair water; and ice, and salt, being heaped up about it, that the cold might reach as far as the water did, it was quickly

frozen. In the mean while, I had caused several wide mouth'd glasses to be brought into my Chamber (wherein, by reason of some indisposition, that hindred me from going abroad, I kept some fire) and having poured several liquors into these glasses, which had been placed all on a row, we suffered them to rest there a while, that the ambient Air might have time to reduce them, as far as it could, to its temper, and consequently to the same temper as to heat and cold, and then with the warmth of ones hand, the included ice being loosened from the glass, as it was taken out, and a ruler divided into inches and eights, being laid alongst it, with a knife a little warmed, the ice was soon, and yet not carelessly, divided into several small Cylinders of three quarters of / an inch, a piece; and these Cylinders thus reduced to as sensible an equality as we could, were nimbly and carefully put

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into the several liquors hereafter to be mentioned, and whilst we our selves watched very attentively, till each of these icy Cylinders was *quite*, and yet but *just* dissolved, we caused others to keep time by the help of a *Pendulum*, whose Vibrations were each a second minute (or 60. part of a Common Minute, whereof 60. go to make an hour) and it was easie for those we appointed, to watch the Vibrations of the *Pendulum*, notwithstanding the Quickness of its Motion, because it was fitted to a little Instrument purposely contrived for such nice observations, wherein a long Index moving upon a divided Dyal plate, did very manifestly point out the number of the *Diadromes* made by the *Pendulum*.

3. This Experiment was afterwards repeated twice with Cylinders of ice, each of them an inch long, and though the successes of these trials were various enough, yet we shall subjoyn / both the last, (as being made with more advantage then the first) that the more light may be gathered from them, and that at least we may discover how difficult it is to make such Experiments in this matter, as that all the nice circumstances of them may safely be relied on.

#### I. Trial.

1. Oyl of Vitriol, where a Cylinder of Ice, of an inch long, being put into, lasted 5. Minutes.
2. Spirit of Wine, (in which the ice sunk) lasted 12. minutes.
3. *Aqua fortis* lasted 12½ minutes.
4. Water lasted about 12. minutes.
5. Oyl of Turpentine lasted (not good) 44. minutes.
6. Air lasted 64. minutes.

#### II. Trial.

1. In Oyl of Vitriol, where an inch of Cylindrical ice lasted 3. minutes.
2. In Spirit of Wine, lasted 13. minutes.
3. In Water, lasted 26. minutes.
4. In Oyl of Turpentine, lasted 47. minutes. /
5. In Sallet Oyl, lasted 52. minutes.
6. In the Air, lasted 152. minutes.

4. We likewise thought it worth trying, whether there would be any difference, and how much difference there would be in the Duration of pieces of ice of the same bulk and figure, some of them made of common water, and others of frozen Wine, Milk, Oyl, Urine, and other spirituous liquors; these several pieces being exposed to be thaw'd in the same Air, or other ambient liquor.

5. We also tried whether Motion would impart a heat to ice, by nimbly rubbing a strong piece of ice upon a plate of ice, and though this seemed to hasten the dissolution in that part of the icy plate, where the Altrition had been made, yet we were unwilling to determine the matter, till further and exacter trial have been made.

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6. And this brings into my mind an Experiment, that has by some been thought very strange. The occasion I remember was, that I received the last Winter the honour of a visit / from a Nobleman of great eminency and learning, who chancing to come in, while I was making some trials with ice, would needs know what I was doing with it, but the presence of a very fair Lady, in whom *Hymen* had made him happy, and of some other Company of that Sex, that he brought along with him, inviting me to give him the answer, that I thought would be most suited and acceptable to his Company, I merrily told him, that I was trying, how to heat a Cold liquor with ice, and to satisfie him, that was no impossibility, I held out an open mouth'd glass, full of a certain liquor (which for some just reasons I do not describe, but do plainly teach it in an opportuner place<sup>†\*</sup>) and desired them to feel, whether it were not actually Cold, and when they were satisfied, it was so, I chose among the pieces of ice, that lay by me, that I judg'd by the eye to be fit for my purpose, (for every piece was not so, for a reason I elsewhere shew,) and throwing it into this liquor, it did not only in a trice vanish in it, but the Lady, / I was mentioning, seeing the liquor smoak, and advancing hastily to try, whether it were really warm, found it so hot, that she was quickly fain to let it alone, and had almost burnt her tender hand, with which she had, in spite of my dissuasion, taken hold of the glass, which Her Lord himself could scarce indure to hold in his. But this Experiment, which for the main I have repeated before competent witnesses, though it be not impertinent to the *History of Cold*, yet I shall not build much upon it, because, how strange soever many have been pleased to think it, I shall elsewhere shew, that I made use of a certain unperceivable slight, which, in my opinion, did as well, as the nature of the liquor and the texture of the ice, contribute to the suddenness and surprizingness of the Effect.

7. But to return to the duration of the effects of Cold, I think those much mistaken, who imagine, thta the effects of Cold do continually depend upon the actual presence and influence of the manifest efficient, as the light of the Air depends upon / the Sun, or Fire, or other luminous body, upon whose removal it immediately ceases. For when cold agents have actually brought a disposed subject to a state of congelation, though the manifest efficient cause cease from acting, or perhaps from being, the effect may yet continue. For in most cases, if a certain texture be once produced in a body, it is agreeable to the constancy of nature, that it persevere in that state, till it be forceably put out of it, by some agent capable to overpower it, and though we usually see ice and snow, as it were of their own accord to melt away, when the frosty constitution of the Air ceases; yet the cause of that may be not barely the cessation of frosty weather, but that those easily dissoluble bodies are exposed to the free Air, which being heated by the Sun beams, and perhaps by calorifick expirations from the earth, is furnisht with an actual cause, upon whose account it destroys the texture of the ice and snow; but even here above

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ground, if snow be well compacted into great masses, in which / by reason of the closeness of the little icickles, but little Air is allowed to get between them, I have seen such masses of snow last so long, not only in thawing, but in rainy weather, as to be wondered at, and if such snow (or ice) be kept in a place where it may be fenced from the Sun, and other external enemies, though the place it



is lodged in, be not any thing near cold enough to produce ice, yet it will, as some trial hath taught me, preserve ice and snow for a very long time.

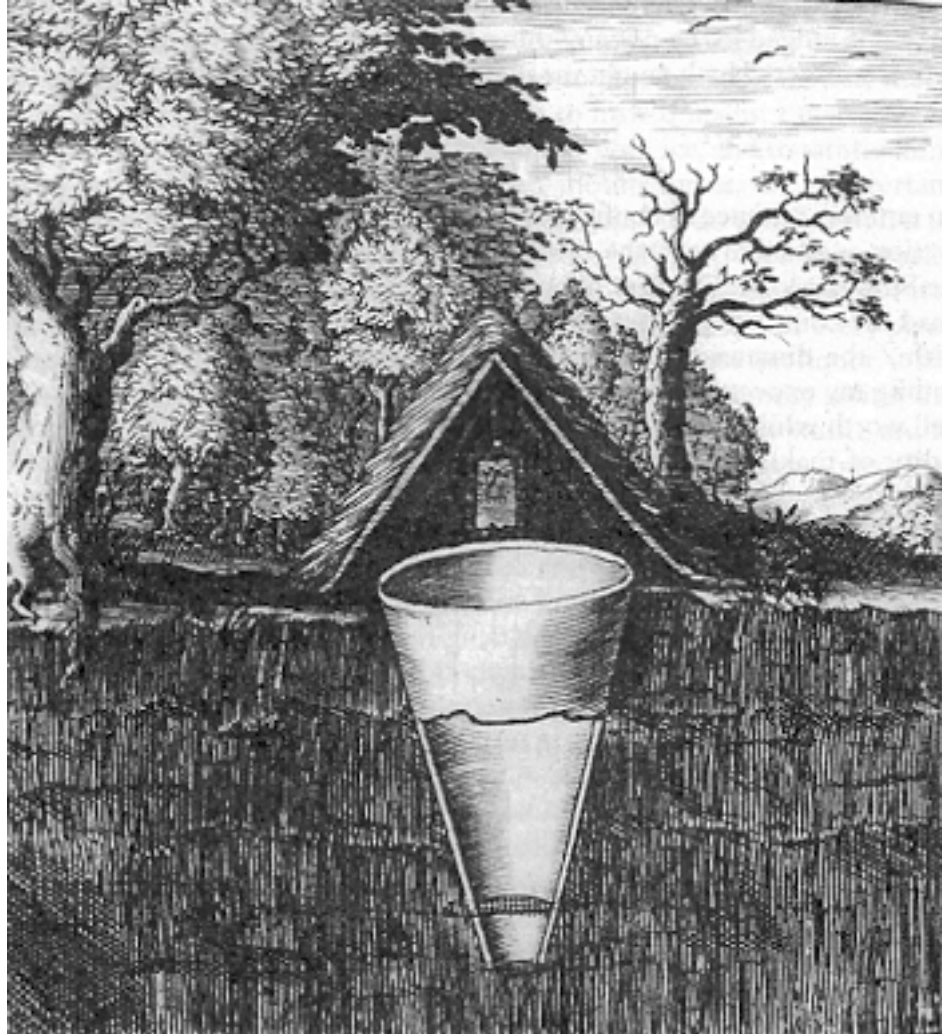
### *Appendix to the XVI. Title.*<sup>†\*</sup>

An eminent instance to confirm what is delivered at the close of the foregoing Section, is afforded us by the conservatories, wherein snow and ice are kept all the Summer long. Of these I have seen in *Italy*, and elsewhere; but supposing I had the command of some *Italian*, and other books, wherein I should meet with / the dimensions, and other circumstances that belong to them, my finding my expectation disappointed by those books, makes me think it very well worth while to subjoyn somewhat about things, that may give us opportunity of making a multitude of Experiments about Cold. And therefore meeting the other day (by good chance) with my ingenious friend Mr. *J. Evelyn*, his inquisitive travels, and his insight into the more polite kinds of knowledge, and particularly Architecture, made me desire and expect of him that account of the *Italian* way of making conservatories of snow, that I had miss'd of, in several Authors; and having readily obtain'd my desire of him, I shall not injure to justly esteem'd a style as his, to deliver his description in any other words, then those ensuing ones, wherein I received it from him.

[The snow Pits in *Italy*, &c. are sunk in the most solitary and cool'd places, commonly at the foot of some mountain or elevated ground, which may best protect them from / the Meridional and Occidental Sun, 25. foot wide at the orifice, and about 50. in depth, is esteem'd a competent Proportion. And though this be excavated in a Conical form, yet it is made flat at the bottom or point. The sides of the Pit are so joyc'd, that boards may be nail'd up on them very closely joynted. (His Majesties at *Greenwich* newly made on the side of the Castle-hill, is, as I remember, steen'd with Brick, and hardly so wide at the mouth.) About a yard from the bottom is fix'd a strong Frame or Tressle, upon which lies a kind of wooden grate; the top or cover is double thatch'd, with Reed or Straw,<sup>†@</sup> upon a copped frame or roof, in one of the sides whereof is a narrow door-case, hipped on like the top of a Dormer, and thatch'd, and so it is complete.

### *To conserve Snow.*

They lay clean Straw upon the grate or wattle, so as to keep the Snow from



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running through, whilst they beat it to a hard cake of an icy consistence, which is near one foot / thick, upon this they make a layer of straw, and on that snow, beaten as before, and so continue a bed of straw, and a bed of snow, S. S. S.<sup>†\*</sup> till the pit be full to the brim. Finally, they lay Straw or Reed (for I remember to have seen both) a competent thickness over all, and keep the door lock'd. This grate is contriv'd, that the snow melting by any accident in laying, or extraordinary season of weather, may drain away from the mass, and sink without stagnating upon it, which would accelerate the Dissolution, and therefore the very bottom is but slightly steen'd. Those who are most circumspect and curious, preserve a tall Circle of shady trees about the pit, which may rather shade, then drip upon it.]<sup>†\*</sup>

Thus far this learned Gentlemans account of Conservatories of Snow. And on this occasion I might add what the *Dutch* in their *Nova Zembla* Voyage relate,<sup>†\*</sup> namely, that *the three and twentieth of June, though it were fair Sunshiny weather, yet the heat was not so strong as to melt the Snow, to afford them / water to drink, and that in spite of their being reduc'd to put Snow into their mouths, to melt it down into their throats, they were compelled to indure great thirst.* But because it was in so cold a Climate, that this duration of the Snow was observ'd, I shall rather take notice, that in the *Alps*, and other high mountains, even of warmer Climates, though the snow doth partly melt towards the end of Summer; yet in some places, where the reflection of the Sun beams is less considerable, the tops will even then remain covered with snow, as we among many others have in those Countries observed.<sup>†\*</sup> And for further confirmation of the Doctrine deliver'd at the end of this 16. Title, I shall subjoyn a Passage, which having unexpectedly met with in an unlikely place of Captain *James's* Voyage, I think not fit to leave unmention'd here, not only because 'tis the sole artificial observation that I yet met with, concerning the lasting of ice, and so may recommend to us the Ingenuity of an Author, whose Testimony we somewhat frequently make use of, but because / the observation is in it self remarkable, and notwithstanding the difference of places may serve for the purpose we alledge it: Our Navigators words are these;<sup>†@</sup> *I have in July, and in the beginning of August taken some of the Ice into the ship, and cut it square two foot, and put it into the Boat, where the Sun did shine on it with a very strong reflex about it. And notwithstanding the warmth of the Ship (for we kept a good fire) and our breathings, and motions it would not melt in eight or ten*

days. And it is also considerable to our present purpose, what the same Author elsewhere has about the durableness of the Congelation of the ground not yet thaw'd at the beginning of June.<sup>†@</sup> *For the ground* (says he) *was yet frozen, and thus much we*

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*found by experience in the burying of our men, in setting up the Kings Standard towards the latter end of June, and by our Well at our coming away, in the beginning of July, at which time upon the land, for some other reasons, it was very hot weather. /*

## Title XVII.

### *Considerations and Experiments touching the Primum Frigidum.*<sup>†\*</sup>

1. The dispute, which is the *Primum Frigidum*, is very well known among Naturalists; some contending for the *Earth*, others for the *Water*, others for the *Air*, and some of the Moderns for *Nitre*: But all seeming to agree, that there is some Body or other, that is of its own nature supremely Cold, and by participation of which, all other cold Bodies obtain that quality.

2. But for my part, I think, that, before men had so hotly disputed, which is the *Primum Frigidum*, they would have done well to enquire, whether there be any such thing or no (in the sense newly express'd.) For / though I make some scruple, resolutely to *contradict* such several Sects of Philosophers, as agree in taking It for granted, yet I think it may be not irrationally *Question'd*, and that upon two or three accounts.

3. For (first) it is disputable enough, as we shall hereafter see, whether cold be (as they speak) a positive quality, or a bare privation of heat, and till this question be determin'd, it will be somewhat improper to wrangle sollicitously, which may be the *Primum Frigidum*. For if a Bodies being cold, signifie no more, then its not having its insensible parts so much agitated, as those of our Sensories, by which we are wont to judge of tactile qualities; there will be no cause to bring in a *Primum Frigidum*, upon whose account particular Bodies must be cold, since to make this or that Body so, it suffices that the Sun or the Fire, or some other agent, whatever it were, that agitated more vehemently its parts before, does now either cease to agitate them, or agitate them but very remisly: So that, till it be determin'd, / whether cold be a positive quality, or but a privative; it will be needless to contend, what particular Body ought to be esteem'd the *primum frigidum* (in the sense above specifi'd.)

4. Secondly, Though it be taken for granted, not only by the *Schools*, but by their Adversaries the *Chymists*, that heat and moisture, driness and gravity, and I know not how many other qualities, must have each of them a *πρῶτον δεκτικόν*,<sup>†\*</sup> or a principal subject to reside in, upon whose account, and by participation of which, that Quality belongs to the other Bodies, wherein it

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is to be met with; though this be so, I say, yet we have elsewhere fully enough manifested,<sup>†@</sup> that this fundamental Notion, upon which much of the Doctrine of Qualities, is both by *Aristotelians*, and vulgar *Chymists*, superstructed, is but an unwarrantable conceit, and therefore not sufficient for a wary Natrualist to build the Notion of a *primum frigidum* upon; there being indeed many qualities, as gravity, and figure, and motion, / and colour, and sound, &c. of which no true and genuine *πρῶτον δεκτικόν* can (for ought I could ever yet discover) be assigned: and because heat and cold are look'd upon as Diametrically opposite Qualities, we may consider, that it will be very hard to show, that there is a *πρῶτον δεκτικόν* of heat; since stones, and mettals, and plants, and animals, and (very few excepted) all consistent Bodies, we are conversant with, may by motion be brought to heat, which to attribute to the participation of some portion or other of the imaginary Element of fire, is not only precarious (being affirm'd by many, and prov'd by none) but erroneous, or at least



needless, as we have more at large declar'd in other papers.<sup>†@</sup>

5. A third thing, that induces me to question whether there be a *primum frigidum*, is, that among those Bodies, that the chiefest Sects of Philosophers, whether Ancient or Modern, have pitch'd upon, there is not any, that seems clearly to deserve the title of the *primum frigidum*. But to make this appear, we must distinctly / (though as briefly as our design will permit) consider those four several Bodies, which we have (at the Beginning of this Section) taken notice of, to stand in competition, in the Opinions of Philosophers, for the title of *primum frigidum*.

6. First, then *Plutarch* and others contend, that it is the Earth;<sup>†\*</sup> but, to omit other Arguments, we see, that the Earth is frozen not by its own cold, but by its vicinity to the Air, as may be argued by this, *viz.* that the congealing cold even in the midst of Winter affects but the surface of the Earth, where it borders on the Air, and seldom pierces above a few feet, or, at most, yards, beneath that part wherein the Earth is exposed, and immediately contiguous, to the Air, as may appear by what we have formerly deliver'd concerning the small depth, to which frosts reach in the ground. And therefore if the Earth be protected from the Air (though by so cold a Body as water) it may be kept unfrozen all the Winter long, as may be gathered from that remarkable practise in the great / Salt-marshes of the French Islands of *Xaintonge*, where, as a diligent Writer of that Countrey, very well vers'd in the making of the French Salt, informs us, when once the season of Coagulating Salt by the heat of the Sun is quite past, the Owners are careful by opening certain Sluces to overflow all the Banks, and Dams, that make and divide the Salt-ponds, and serve for the Workmen to pass to and fro: for (says my Author in

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his own language<sup>†@</sup>) if they left those Marshes (or Salt-works) uncovered, the frost would make such havock amongst them, that it would be necessary to make them up again every year, but by means of the water, they are preserv'd (or kept in repair) from year to year: which practise I the rather mention, because the hint, it affords, as it is considerable to our present purpose, so it may on some occasions be applicable to practises useful to humane society.

7. Besides, the Earth being (according to those we reason with) the coldest, heaviest, and solidest of Elements, it is not so probable, as to excuse / them from the need of proving it, that those excessively cold Agents, that freez the Clouds into Snow and Hail, should be terrene Exhalations carried up to the middle Region of the Air, especially since it must be done by Agents, either hard to be guess'd at, or considerably hot. And 'tis not easie to give a reason, why, if Elementary Corpuscles steaming from the Earth, have such a congealing cold, where they are disunited, and but interspers'd among the particles of Air, the Mass of the Earth it self, whence those exhalations are suppos'd to proceed, should not be able also to congeal water, since the Terrestrial Corpuscles being more thick set, and united in a Clod of Earth, then in an equal portion of the Atmosphere, it seems, that where the frigorifick matter is more dense, the cold should be more vehement, as Philosophers observe, that heat is more intense in a glowing bar of Iron, then an equal portion of the flame of kindled Straw.

8. But (not to repeat what we formerly mention'd about Colds being / a Privation) there is another Argument against the Earths being the *primum frigidum*, and that is taken from the Subterranean fires, which breaking forth in many places of the Earth, as in *Ætna*, *Vesuvius*, *Hecla*, the *Pico of Tenariffe*, &c. seem to argue a Subterranean fire, upon whose existence not only many Chymists build great matters, but even divers Philosophers have adopted it, and the learned *Gassendus* himself seems so far to countenance it, as to imploy it as one Argument of the Earths being naturally neither hot nor cold.<sup>†\*</sup> The mention of this Subterranean fire brings into my mind some things that I have met with amongst good, though not Classick, Authors, and amongst men that have been either diggers of (or conversant in) Mines, not improper to be here taken notice of. For though I do not now intend to declare my opinion about the Central fire, either of the *Chymists*, or



*Cartesians*, and though the Examples newly mention'd, and such other seem to me but very inconsiderable, in reference to the whole Earth, yet / 'tis observable

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to our present purpose, that there should be so much Subterranean heat or warmth, at least generally to be met with: For even where there appear no manifest signs of Subterranean fires, I have known those, that were wont to go to the Bottom of *deep* Mines, complain, that a very little Exercise would put them into a great sweat; and a learned and experienced French Doctor, that hath written in his own Language of Stones and Jewels, affirms, that in *such* Mines the Subterranean Vapors and Exhalations, are visibly so abundant, and likewise so hot, that the Mine-men are constrain'd (which a person I spoke with affirmed to me, touching himself) to work in their shirts, by reason of the great heat they there felt, and though I would have been glad to know, whether those deep places would have appear'd as hot, when judg'd of by a seal'd Weather-glass, as they did to the Mine-mens Sensories, because of some little doubts I harbour'd, whether much of that copious sweating, and seeming heat, might not proceed from the / thickness of the dampish Air, and its unfitness for Respiration;<sup>†@</sup> yet, because a *Virtuoso*, that had a Lead-Mine of his own, in which he wrought himself for curiosity, answered me, that he was not wont to find any difficulty of breathing in the place, where he was so apt to sweat; and since I find not, that others have complain'd of having their respiration incommodated in such places, unless by Accidental Damps, my scruple was much abated, and the rather, because the Author lately mention'd, expressly affirms,<sup>†@</sup> that the Sudorifick heat (if I may so speak) is to be found in the Bowels of the Earth, as well in Summer as in Winter, which prevents the ascribing of it to *Antiperistasis*. And in other places then Mines 'tis generally observ'd, that Wells and Springs freez not, if the place, whence the water is drawn, be very deep, but, as we have observ'd elsewhere, that it oft comes up smoaking, and, as it were, reaking, which argues, that at the least the Earth, wherein it was harbour'd, or through which it pass'd, was, if not / warm, free from such a degree of Cold, as might be expected in the Earth, if it were the *primum frigidum*. Nor can it be reasonably pretended, that the Subterranean heat comes from the Beams of the Sun, since learned Men have observed, that those heat not the Earth above six or seven foot deep even in Southern Countries, and though we should allow them to pierce three times as far, yet that would not be considerable to the depth of the Mines above mentioned, and if the lower part of the Earth were of its own nature cold, and received the heat, it discloses only from the Sun and Stars; the deeper the men dig, the lesser of heat and steams they would meet with, whereas the above cited French Minerallist affirms, that the lower they go, the more vapours, exhalations, and heat they find.<sup>†@</sup>

9. But because this learned man delivers this circumstance in a dogmatical, rather than an historical way, I will add somewhat out of a relation (whence

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I have / elsewhere taken other particulars<sup>†@</sup>) made by a French Physician likewise, that had the curiosity to descend himself into the deep Mines of *Hungary*, some of which, that he went down into, may be collected by his Narrative, to have three or four hundred fathom, that is eighteen, or twenty four hundred foot of perpendicular depth. This Author then relates, that after he had descended about 180. or a hundred fathoms, he came into a very warm Region of the Earth, which lasted to the bottom of the Mine, and is so hot both Winter and Summer, that the Laborers are wont to work in it without their clothes, and he was scarce able to indure the heat of it, although the external Air were very hot: the weather being very fair, and the moneth *July*, / He adds,<sup>†@</sup> that he having demanded of the Overseer of the Mine, whence this heat came, he was answer'd, to that and several other questions, *That* it came from the lower parts of the earth; *that* in all deep Mines, after one is past the Colder crust of the earth, one comes into a region, that is perpetually warm, and *that* where ever they dig the ground, after they are come to such a depth (which he elsewhere mentions

to be about 80. or a hundred fathom) they feel no more any cold, but a perpetual heat, how deep soever they dig, (†@ yet without observing, that after they are once into that warm region, they find the heat sensibly increase, the nearer they approach to the centre of the earth, / unless by accident they happen to dig through veins of hotter Minerals.) And these answers (subjoyns my Author) I received not in one Mine alone, or from a single overseer, but in all the Mines, and from all the Masters of them; so that if these were not mistaken, we may safely conclude, that as far as experience can inform us, the body of the earth in its lowermost parts, where 'tis presum'd to be coldest, is every where, and that considerably, hot. I said, *if these Mine-men were not mistaken*, because having been in the bottom of some Mines my self, though I find it acknowledged, that 'tis still warm in the bottom of deep ones, yet I confess, I somewhat suspect by what I have

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observ'd, that this degree of heat, which our French Physician found in the *Hungarian* Mines, might be rather in great part from the peculiar nature of those places, or of the Minerals generated there, then barely (as he and those that inform'd him suppose) from the greatness of their depth beneath the surface of the earth; for I know several mixtures, / besides those that are common, of bodies neither of them actually hot, which will produce a considerable degree of heat.

And very credible eye witnesses affirm, that in some parts of *England*, they dig up good store of a kind of Mineral, which is thought to be of a Vitriolate nature, which by the bare addition of common water, will grow hot, almost to ignition. So that the *Hungarian* Mines being deep, and as appears by our Authors Narrative, being not destitute of water enough to make a Subterranean Spring in the Mine its self, besides what water may plentifully ascend in the forms of vapours, and moisten the Oar, it may be suspected, that either the water, or some appropriated Mineral spirit or juice (of which the bowels of the earth may contain divers, that we know nothing of) may produce together with the Mineral a warm steam, which for want of sufficient vent in those narrow, and close places, may heat them considerably, which conjecture may be countenanced by these three circumstances, / that I took notice of in our Authors Narrative; *one*, That the smoak that copiously ascended out of the Mine by the perpendicular groove, was not barely hot, but consisted of stinking exhalations, which were so saline, and fretting, as oftentimes to corrode and spoil both the wooden ladders or stairs, and the iron instruments of the diggers. *The other*, that the overseers themselves of the Mines, told *Morinus* (as we lately saw) that they in some places met with veins of hot Minerals, which made it hotter, then the bare vicinity of those places to the centre of the earth would have done. And *lastly*, †@ as our Author was descending into the golden Mine at *Cremnitz*, he found in one place, the heat to increase as he descended more and more, (which seems not to agree with a passage we lately mention'd out of him) and to exceed any he had met with in any other Mine; and afterwards the overseer bringing him into a room, / that abounded with smaragdine Vitriol, (the Mineral whence this heat proceeded) though the room were spacious, he found there, besides a sharp spirit very offensive to his throat, so troublesome a heat, that he was ready to faint away with sweating, and very much wondered how the diggers were able to work there. And elsewhere the Author himself notes, that such hot Mines of Vitriol, or Sulphur, may be found even in the first region of the earth, (as he calls that which is somewhat near the surface, and which he thinks fit to name the *cold region*) and within a large sphere of activity make it perpetually hot. But this, as I was intimating, I mention but as a suspicion, or a conjecture, and notwithstanding that the

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degree of heat may be much increased in these Mines, by the concurrence of accidental causes, in case the conjecture be admitted; yet since the frequency of a sensible degree of heat in very deep places does very little favour their opinion, that will allow the earth to have no other heat, but what it receives / from the Sun beams, or by the manifest fire of burning hills, as *Ætna* and *Vesuvius*. And if it should be objected, that this Subterranean heat is adventitious to the Earth, which is supremely cold of its own nature; *Gassendus* might reply, †\* that 'tis as likely, that the coldness of it near the

superficies may be adventitious too, and that it appears at least as manifestly, that the one proceeds from the contiguous Air, as it does, that the other proceeds from some included fire; and if I misremember not, he hath this consideration, that 'tis somewhat strange, that Nature should have intended the Earth for its *summum frigidum*, and yet that a great part (and for ought we know the greatest) should be constantly kept warm, either by the Sun, as under the Torrid Zone, or by the Subterranean fires. But the objection mention'd against *Gassendus*, opposes but one of the Arguments we have alledg'd against the Earths being the *primum frigidum*, and would leave the others in their force, though it did more convincingly answer, that, / against which 'tis framed, then it seems to do.

10. And if the Patrons of the Earths coldness, to evade the Arguments I have alledged, should pretend, that when they affirm the Earth to be the *primum frigidum*, they mean not the Elementary Earth, but some Body that is mingled with it; I shall desire to know, which 'tis they mean of the many other Bodies, that make up the Terrestrial Globe, that we may examine what right it has to that Title; and in the mean time I shall conclude against them, that the Earth it self has none, since they grant a colder Body then it, and such a one as the earth must be beholding to, for the greatest degrees of coldness it chances to possess.

11. But though I presume, enough has been said to make it appear unlikely, that the Earth should be the *primum frigidum*, yet I must in this dissent from the learned *Gassendus*, that he thinks the Earth, not only not to be the *primum frigidum*, but not to be naturally cold any more then hot. For the insensible parts of the Earth, / like those of other firm Bodies, being heavy, and perhaps gross, and either having no constant motion at all, or at least a far more remiss agitation, then that of our Sensories; it seems to follow, that the Earth must seem cold to us, unless it be by the communicated heat, or motion of some extrinsick Agent, put into a degree of agitation, that belongs not to its nature; and for the like reason I think it not improbable, that pure Earth should in its own Nature be colder, then either pure Water or pure Air, since the Earth being a consistent Body, its component particles are at rest among themselves, or at least mov'd with an almost infinite slowness, whereas Water and Air being fluids, their component particles must be in a restless and various motion, and consequently be less remote from heat, which is a state wherein the various agitation of the minute particles is more vehement.

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12. And if those, that plead for the Earth, had declar'd, that they meant not the pure or Elementary / Earth, but that part of the Terrestrial Globe, that is distinct from the Sea, and other Waters, that make it up, and would have Earth in that sense not to be the *primum frigidum*, but only the *summum frigidum*, perhaps they might have a better plea for their Opinion, then they can urge for theirs, who contend for the Water or the Air, especially, if to countenance their Opinion, this memorable observation be added, which I have met with among those Navigators, that have had the greatest Experience of the Frigid Zone;†@ for the *Dutch*, that sail'd thrice to *Nova Zembla*, and once wintered there, affirm in their first voyage, that the highest degrees of Cold are not to be met with in the main Sea, where yet men are most expos'd to the Operations / of the Air, and of the Water, but either upon the Land or near it. That accurate Geometrician and Hydrographer *Fournier* tells us,†\* that in 1595. the *Hollanders* being intercepted by Icy Scholes in the strait of *Weigats*, and meeting with certain *Muscovites*, demanded of them, whether those Seas were always frozen, and were answered, that neither the Northern Sea, nor that of *Tartary* did ever freez, and that 'twas only that strait with the Sea contiguous to the shores of some Bays and Gulphs, that were frozen; and our judicious Author, not only adds, that in effect all those that sail into those parts relate, That all those Lumps of Ice are such as have been loosened, and severed from the Islands, and the Rivers of the *Samojeds* and *Tartars*, but adventures to affirm in general terms, that 'tis certain, the *main Seas never freez*, and that 'tis but the confines, and shores of some of them, that are frozen.

13. That the water is the *primum frigidum*, the Opinion of *Aristotle* has made it to be, that of the



schools, and / of the generality of Philosophers. But I can as little acquiesce in this opinion, as in the former, not finding it agreeable to what experience teaches us.

14. For not to mention, that it would be very difficult to prove, that divers very cold Bodies, as Gold and Silver, and Crystal, and several other fusible stones have in them any water at all, to which their coldness may with any degree of probability be ascribed; nor to urge the Arguments, that some Modern contenders for the supreme coldness of the Air are wont to imploy; not (I say) to insist on such things, I shall content my self to make use of this obvious *φαινόμενον* of Cold,<sup>†\*</sup> that in Rivers, Ponds, and other receptacles of water, the congelation begins at the Top where the liquor is expos'd to the immediate contact of the Air, which sufficiently argues, that the Air is colder

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then the Water, since it is able not only sensibly to refrigerate it, but to deprive it of its fluidity, and congeal it into Ice, whereas if the water it self were the *primum frigidum*, / either it ought to be, at least as to the *major* part of it, always congeal'd, or we may justly demand a reason, why, when it does freez, the glaciation should not begin in the middle, or at the bottom, as soon as at the Top, if not sooner. And our Arguments against the precedency of the water in point of coldness, may be strengthen'd by this, That frosts are wont to be hardest, when the Air is very clear, and freest from Aqueous vapors, whereas in rainy weather, wherein such vapors most abound, the cold is wont to be far more remiss: To which we may add, what we lately deliver'd from the observation of Navigators, that even in the frigid Zone the main Sea, where yet the water is in the greatest mass, and so most likely, as well as advantag'd to disclose its nature, never freezes, though the Straits, and Bays, and Gulphs be frozen over, which argues, that the greatest degrees of Cold are rather to be assign'd to the Air, or to the Earth, then to the Water, which by the practise formerly mention'd of the Masters of / the French Salt Marshes appears to be (when it is of a considerable depth) fitter to preserve Bodies from congelation, then to congeal them, which instance I the rather repeat, because it seems to argue, that the water is not so much as dispos'd to receive any very intense degree of cold at a remote distance from the Air: for though Navigators tell us of exceeding thick pieces of Ice, yet, as we have already elsewhere noted, we are not bound to believe, that the congealing cold has pierced any thing near so much as that thickness amounts to from the superficies of the Sea directly downwards; for though it were no great matter if it did, in comparison of that depth of the Sea, which, though the water be naturally cold, the sharpest Air is unable to congeal, yet we have elsewhere proved, that those thick masses of Ice, are not solid and intire pieces, but rather heaps of many flakes, and other fragments of Ice, which running upon one another, or sliding under one another, are by the congelation of the intercepted water (and / perchance half thaw'd snow) as it were, cemented together into misshapen and unweildy masses; which conjecture agrees very well with that observation of the Ingenious Captain *James*, which he delivers in these words.<sup>†\*</sup>

*It seldom rains after the middle of September, but snows, and that snow will not melt on the lands, nor sands: At low water, when it snows (which it doth very often) the sands are all covered over with it, which the half tide carries officiously (twice in twenty four hours) into the great Bay, which is the common Rendezvous of it. Every low water, are the sands left clear to gather more to the increase of it. Thus doth it dayly gather in this manner, till the latter end of Octob. and by that time hath it brought the Sea to that coldness, that as it snows, the snow will lye upon the water in flakes, without changing its colour, but with the wind is wrought together, and as the Winter goes forward, it begins to freez on the surface of it, two or three inches, or more in one night, which being carried with the half tide, meets with some obstacle (as it soon doth) and*

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*then it crumples, and so runs / upon it self, that in few hours it will be five or six foot thick; the half tide still flowing, carries it so fast away, that by December it is grown to an infinite multiplication of Ice. Thus far this Navigator, to which I shall add another passage out of one of his Countreymen (Mr. *Hudson*) (famous for the Northern Discoveries, that bare his name) by which, added to what*



has been elsewhere deliver'd to the same purpose, we may be invited to believe, that the vast Hills and Islands of Ice, that are to be met with about the Straits of *Weigats* and elsewhere, are not generated of the Sea it self.<sup>†@</sup> *Its no marvel* (says he) *that there is so much Ice in the Sea towards the Pole, so many Sounds and Rivers being in the Lands of Nova Zembla, and Newland to ingender it, besides the coasts of Pechora, Russia, and Greenland, with Lappia, as by proof I find by my Travel in these parts.*

15. But for all this, I think not fit, as does the Ingenious *Gassendus*, and some others, to make the water indifferent, as to heat and cold.<sup>†\*</sup> For, as I formerly noted concerning the / Earth; so I must now represent touching the water, that, setting aside the heat of the Sun, which is but adventitious, where it does operate, and which leaves many vast portions of that Element, which it does not constantly reach, the insensible parts of water are much less agitated, then those of our Sensories temperately dispos'd, and consequently may in regard of us be judg'd cold. For though water being a Liquor, I readily allow it a various Motion of its component Corpuscles, (that being requisite to make a Body fluid,) yet such an agitation, which is sufficient for fluidity, may be, and often is, far more remiss, then that of the spirits, Blood, and other liquors of so hot a Sanguineous animal as Man, as we see, that Urine, though after it has been long omitted, it continues a fluid Body, yet its parts are far less agitated, then they were, when it came hot, and reeking out of the Bladder.

16. And upon this occasion, I shall add, what by inquiry I have learned, that (except the parts somewhat near the superficies of the water, which / the heat of the Sun, or the warmth of the neighbouring lower Region of the Air may give some warmth to) the whole Body of the Sea is very cold; for being very well acquainted with one, that for some time got a livelihood, by going down into the Bottom of the Sea, to fetch up what could be recovered out of shipwrackt vessels, I purposely inquired of him, what cold he felt under water, and he more then once told me, that though near the Top of the water the cold were very moderate, yet when he was necessitated to descend a great depth, he found it so great, that he could not very long support it; and particularly he told me, that having occasion to descend about twelve or fourteen fathom deep (which is nothing in comparison of the depth of many Seas) to fasten ropes to the Ordinance of a great ship, that was some years since cast away, near the coast of one of the Northern Countries, though the Engine that was let down with him supplied him so well with Air, that he was not incommodated in point of Respiration, and / though he felt no

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other inconveniencies, that might dissuade his tarrying longer, yet the cold was so great, and troublesome, that he was not able to endure it above two or three hours, but was constrain'd to remount to a milder, as well as a higher Region. I wish'd several times he had had with him a seal'd Weather-glass (for *ordinary* Thermometers would on that occasion have been unserviceable) to prevent some little doubt, that might be made, whether the intense Cold he felt might not be only and chiefly in reference to his Body, which might be so alter'd, and dispos'd by this new Briny Ambient, as to make such a disturbance in the course or texture of his Blood, as that which makes Aguish persons so cold at the beginning of the fit, though the temperature of the Ambient Body continue the same. But this is not the only person, that found the Sea Exceeding cold, for I remember *Beguinus* relates from the mouth of a *Marseillian* Knight, that was overseer of the Coral-fishing in the Kingdom of *Tunis*,<sup>†@</sup> that having / upon that coast let down a young man, to feel, whether Coral were hard or soft, as it grew in the water, when this man was come about eight fathom, near the Bottom of the Sea, he felt it exceeding cold. To which we shall add the testimony of a sober Traveller, *Josephus Acosta*, who tells us,<sup>†@</sup> *That it is a thing remarkable, that in the depth of the Ocean, the water cannot be made hot by the violence of the Sun, as in Rivers: Finally* (he subjoyns) *even as Salt-Petre (though it be of the nature of Salt) hath the property to cool water, even so we see by experience, that in some parts and havens, the salt water doth refresh, the which we have observed in that of Callao, where they put the water or wine which they drink, into the Sea*

*in Flaggons to be refreshed, whereby we may undoubtedly find, that the Ocean hath this property to temper and moderate the excessive heat.* For this cause we feel greater heat at Land then at Sea, *cæteris paribus*, and commonly Countries lying near the Sea, are cooler then those that are farther off. By all these testimonies, it seems to appear, that both in very cold Regions, and / very hot, the deep parts of the Sea seem to be very Cold, the Sun beams being not able to penetrate the Sea to any great depth; for I remember, that having enquired of the Diver I lately mentioned, whether he could discern the light of the Sun at any great distance from the surface of the water, he answered me, that he could not, but as he went down deeper and deeper, so he found it darker and darker, and that to a degree, that would scarce have been expected in so Diaphanous a Body as water is.

17. But this submarine cold (if I may so call it) though it be great and considerable, is not so intense, as to intitle water to be the *primum frigidum*, since as cold as our *Divers* found it at the bottom of the Sea, they did not find it cold enough to freez the water there, as the Air often does at the Top.

18. The next Opinion we are to consider, is that of the *Stoicks* of old, and

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adopted by the generality of Modern Philosophers, that are not Peripateticks, who assert the *Air* to be the *primum frigidum*: But being *ere / long* more particularly to treat of the Temperature of the Air, we will reserve till then to examine, whether it be cold of its own nature or not; but in the mean time, we shall *here* take leave to question, whether it ought to be esteem'd the *primum frigidum*. For not to mention, that *Aristotle*, and the Schools, with many other learned men, think the Air so far from being the coldest of the Elements, that they reckon it among the hot ones, because I confess their opinion is not mine, not to represent the heat of the Air in the Torrid Zone, nor that by the generality of Philosophers, the upper Region of the Air, which is believed to make incomparably the greatest part of it, is always hot, and the lower Region is so too, in comparison of the middle, though the coldness even of this is not perhaps unquestionable, not to urge any of these things, I say, I shall in this place mention only two observations.

19. The one is that, which I lately recited, touching the great coldness of the water in the deeper parts / of the Sea, for 'tis not easie to show, how this great cold proceeds from that of the Air, whose operation seems not (as may be judg'd by that little way that frosts pierce into the moist Earth) to reach very far beneath the surface of the water, (insomuch that Captain *James*, who had very good opportunity to try, allows not, in case the Ice be not made by accumulation, that the Frost pierces above two yards perpendicularly downwards from the surface of the water, even in the coldest habitable Regions.<sup>†\*</sup>) And this will seem the more rational, if we consider, that in case the coldness of the Sea proceeded constantly from the Air, as such, the cold would be greater near the surface, where 'tis contiguous to the Air, then in the parts remoter from it, and yet the contrary may appear by the passages lately recited.

20. But if it be objected, that this at best can prove no more, then that the Air is not the *primum frigidum*, notwithstanding which, it may be the *summum frigidum*. For answer, / I must proceed to my second Argument, which will perhaps evince, that it is not that neither, for by the same way of arguing, by which those I am now dealing with endeavour to prove the Air to be the coldest Body in the World, I shall endeavour to prove, that it is not so: For their grand, and (as far as I remember) their only considerable Argument is drawn from Experience, which shows, that water begins to freez at the Top, where 'tis exposed to the Air; but to this vulgar Experiment I oppose that of mine, which I have often mentioned already to other purposes, that by an application of salt and snow, I can make water, that would else freez at the Top, begin to freez at the Bottom, or at any side I please, and that much sooner then the common Air, even in a sharp frosty night, would be able to congeal it; and when in exceeding cold weather the Ambient Nocturnal Air had reduc'd a parcel of Air purposely included in a convenient glass, to as great a degree of condensation as it could: I have

External application of other things, been able to condense it much farther, which argues, that 'tis not the Air as such, but some adventitious frigorifick Corpuscles (taking that term as I do in this Treatise in a large sense) that may sometimes be mingled with it, which produce the notablest degrees of cold, or upon whose Account the Air produces them. And if these be duly applied, water will be congealed, whether Air comes to touch the surface of it or no; nay, though Bodies, which the Air can never penetrate nor congeal any of their parts, be interpos'd, as may appear by the Experiments formerly mention'd of freezing water included in glass bubbles, and suspended in oyl of Turpentine, and other uncongealed Liquors; and it is worth taking notice of, by them that conclude the Airs being the *primum frigidum*, from the waters beginning to freez at the Top, where 'tis contiguous to the Air, that it is there also where the Ice begins to thaw.

21. Besides the three Opinions we have hitherto examin'd, there is a / fourth, that justly deserves to be seriously consider'd; for the learned and ingenious *Gassendus* is suppos'd, though I doubt how truly, to be the Author of it, and though according to his custom, he speaks warily, and not so confidently of it, yet in his last writings he much countenances it;†\* yet some eminently learned men, as well of our own, as of other Nations, have resolutely enough embraced it. According then to these, the congelation of Liquors, and the cold we meet with in the Air, Water, and other Bodies, proceeds from the admixture of Nitrous exhalations, or Corpuscles introduc'd into them: And as I have a great respect for divers of these mens persons, so I like very well in their opinion, that they do not ascribe the supreme degree of frigeffective Virtue to the Air it self, but to some adventitious thing, that is mingled with it; but whereas they pitch upon *Nitre*, as the grand Universal efficient of cold, I confess I cannot yet fully acquiesce in that Tenent. For though I am not averse from allowing Salt-Petre to be one / of those Bodies, that are endued with a refrigerating power, and to be copiously enough dispers'd through several portions of the Earth, yet for ought I know, there may be not only divers other causes of cold, but divers other Bodies qualified to be Efficients of cold, as well as Salt-Petre.

22. And first, if cold be not a positive quality, but the absence of heat, the removing of calorifick Agents will in many cases suffice to produce cold without the introduction of any Nitrous particles into the Body to be refrigerated. But because 'tis disputable, whether cold be a positive quality or no, we will urge this Argument no further, till the Controversie be decided, and till then, as it will remain not improbable, we propose it as no other, but proceed to the next.

23. In the second place, I see not as yet any proof, that the great cold, we have formerly mention'd to be met with in the depths of that vast Body the Sea, especially when it is greater elsewhere, then nearer the / Top, where the Air may better communicate its coldness to it, must be the effect of Nitrous

Atoms, which must certainly swarm in prodigious multitudes to be able to refrigerate every drop and sensible particle of so stupendiously vast a Body as the Ocean. Besides that I remember not to have found or known it observ'd, that Nitre, especially in vast quantities reaches near so deep in the Earth, as those parts of the Sea, that are found exceeding cold. And as the halituous part of Nitre is more dispos'd to fly up into the Air, then dive down into the Sea, so we find no great documents of its having its grosser and sensible parts abounding in the Sea-water, since the evaporations of that leaves not behind it Salt-petre, but common Salt. But these, though no light considerations, are not those, that most weigh with me.

24. For (in the next place) I am not satisfied with the Experiences I find alledged to prove, that 'tis



by Nitre, that the Air and the neighbouring parts of the Earth, and Water (not to repeat the objections I lately / borrowed from the Sea) receive their highest degrees of Cold. For when *Gassendus* and others tell us, that 'tis Nitre resolv'd into exhalations, that make the gelid Wind, which refrigerates all things it touches, and penetrating into the water, congeals it, this, I say, to me will seem precarious, untill *Gassendus* (or some other for him<sup>†\*</sup>) tell us, what Experiments they are (which he seems in one place to intimate) that this new Doctrine depends on; for, I, confess, that for my part, I who have perhaps had more opportunity to resolve Nitre, have seen no great feats, that the steams of it have done, more then those of other saline Bodies in the production of cold; and the spirit of Nitre, which is a liquor consisting of the volatile parts of that resolved salt, not only does not (that I have observed) appear to the touch to have considerably, if at all, a greater *actual* cold, then that of divers other Liquors, but seems to have a *potential* heat. For whether or no the Exhalations of Nitre be able to congeal water into Ice, I have formerly observ'd, / that the spirit of Nitre or *Aqua fortis* will dissolve Ice into water, very near, if not altogether as soon as the spirit of Wine it self, which inflammable Liquor is generally acknowledg'd to be in a high degree *potentially* hot. If *Gassendus* did not mean such steams of Salt-petre as these which I have been speaking of, it had not been amiss to have signified what other kind of Corpuscles of resolved Nitre he meant, without leaving his Reader to divine it; and if we may judge of other Experiments, which we lately took notice, that *Gassendus* seems to intimate, by that which he sets down a little after, compar'd with that he had mention'd a little before:<sup>†@</sup> I am not likely much to be convinc'd by them, but shall rather be tempted to suspect, that learned man might be impos'd upon by others to write that, as matter of fact, / which he never had tried, and yet own not the

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having it only by report. For whereas he seems to say, that dissolved Nitre mingling it self with water, freezes it, and that in Summer, yet I must freely profess, that although some other Learned Moderns teach the same thing (but without any mans avouching it, that I know, upon his own experience) I, who am no stranger to Nitrous Experiments, have never been able to produce, or so fortunate, as to see any such effect, and 'tis somewhat strange to me, that *Chymists*, who make such frequent solutions of Nitre, and oftentimes with less water, then is sufficient to dissolve it all, so that by consequence the proportion of the Nitre to the Water, must have run through almost all the possible measures of proportion, should never so much, as by chance (as I can hear) have observ'd any such matter: and that which makes me thus interpret *Gassendus* his meaning, (though in one of the two passages, wherein he sets down this Experiment, he mentions also snow, or ice to be added to / the Nitre) is, that in the first of those two passages, he ascribes the congelation to Nitre alone, without speaking of either ice or snow; and in the other place, not only his words seem to import,<sup>†@</sup> that notwithstanding the addition of the other ingredients, the Corpuscles of the Nitre expiring out of the mixture, and penetrating into the water, are they that make it freez, but the Exigence of his discourse seems to require such an interpretation: for to say it is the Corpuscles of the Nitre, that were harbour'd in the ice or snow, that freez the water they invade, is no better then to beg the Question. For besides that, he ought to prove, that there are multitudes of the Corpuscles of Nitre, lodg'd in snow and ice: Besides this, I say, since these two Bodies are said to be water before they were congealed, to grant what his Explication supposes about ice and snow, is to grant in effect, / that Nitre alone (without ice or snow) can turn water into ice, which is the thing that Experience warranted us lately to deny; and if this be all, that is meant by the Experiment, the mixing of Nitre with the ice, or the snow, will signifie very little, to evince what should be proved. For, if instead of Nitre you take Sea-salt, or the spirit of Salt, nay, the inflammable part of Wine, the Experiment will succeed; and yet I think *Gassendus* would not have the Corpuscles of these Bodies to be frigorifick, like those of Nitre, which yet they may be prov'd to be by the same Argument, which is employed to show, that the Corpuscles of the Nitre, which is added as a distinct ingredient to the ice, or to the snow, are the Efficients of the Congelation.

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25. Having thus examin'd *Gassendus* his *Experiments*, we will now, as our next and last Argument touching this subject, subjoyn our own, as far as we can find any of them among our notes, some of which follow in these words./

26. [As cold as they think Salt-petre to be, who teach its spirituous parts to be the Grand and Catholick efficient of cold, yet we found, that it would dissolve ice readily enough, as well as Sea-salt, &c. are wont to do, as we collected from this, That roch'd Petre mingled with ice, would freez the vapors wandring in the Air, to the outside of the single Vial, wherein we made the Experiment, which the ice alone would not have done; and having placed some grossie beaten Nitre (of the same parcel) in little heaps here and there upon plates of ice, we manifestly found them to sink into the ice, which argued their dissolving it; and having put some of it upon a thick and smooth piece of ice, we found, that it had pierc'd a hole quite through it, whilst the surrounding part of the ice remain'd of a good thickness.]<sup>†\*</sup>

27. [We took a large single Vial, almost full of water, and put it into as much roch'd Petre, as by keeping it a good while by the fires side, we could dissolve in it, of which one mark was, that there remain'd / a pretty deal of Salt intire at the Bottom of the liquor, this being expos'd to the Air, during an extremely sharp night, and a good part of the day, the solution was frozen so hard to the very Top of the liquor, that having broken the glass, we could hardly break the included mass. But at the Bottom there appear'd some liquor, with Crystals of Nitre well figur'd, that seem'd to have shot in it, and argued the Water to be sufficiently impregnated with the Salt.]

28. [As for the spirituous parts of Nitre, so far forth as their temper, as to heat or cold, can be judg'd by distillation, and by Weather-glasses, they are not *actually* more cold then some other Liquors, and appear rather to be *potentially* hot, then cold, at least they seem indispos'd to turn water into ice, since we have tri'd, that the spirit of Nitre will readily enough turn ice into water.]

29. These three foregoing notes show, that Salt-petre is no such wonderfully cold Body, but that there are others colder, as being able to / freez water, which Nitre could not congeal. Nay, they manifest, that Nitre, which is said to be the efficient of ice, does thaw and dissolve it, and so seems at least in reference to It, to be rather hot then cold.

30. I shall now add one note more, to show it does not always make water so much as equally cold with the common Air; the Experiment I find thus recorded.

31. [We took a seal'd Weather-glass,<sup>†@</sup> and by a little pulley fastned to a frame, suspended it in a solution of roch'd-Petre, as strong as we could make it, without heat, as appear'd by a pretty Quantity of Nitre, that had continued some days undissolved in the vessel, which was a Beer-glass, with a flat Bottom. After the Ball of the Weather-glass had been suspended in this

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liquor, to try, whether the Ambient Air were not at this time colder then the Liquor, (it being a cloudy and windy day, and betwixt the hours of 11. and 12.) though both the Weather-glass and it, had stood some days in the same place. I lifted up the glass out of / the water by the string it hung by, that I might not touch it with my warm hands, and found the Liquor in the glass to descend by degrees, about two divisions (which were eights of an inch) and then by the string lifting up the Weather-glass, and putting again the solution of Nitre under it, the included Liquor was impell'd up again two divisions, and sometimes two divisions and a half, for to satisfie my self the more fully, I repeated the Experiment several times, and observ'd, that the included liquor usually ascended the first division, so fast, that the eye could perceive its progress, and that the ascent upon the immersion in the dissolv'd Nitre was discernably quicker, then the descent upon the removal of the Weather-glass into the open Air, though the space both of the one and of the other were about, either two divisions, or two divisions and a half.]

32. If it be here demanded, what then I think of the frigifactive Virtue of Nitre, I must answer, that I have not yet fully satisfi'd my self concerning / it, but thus much I am not willing to deny, That among divers other Bodies, that upon several occasions exhale from the Terrestrial Globe, those Corpuscles that are of a Nitrous Nature, may be for the most part well qualified to refrigerate the Air, and I am not indispos'd to think, that there may be store of little saline Bodies of kin to Nitre, that (especially at certain times) rove in great multitudes to and fro, in some parts of the Atmosphere; but that this aerial salt, which some moderns call volatile Nitre, should be true and perfect Salt-petre is more then I am sure of, and that this Salt alone should be the *summum frigidum*, is more then as yet I am convinc'd of; especially, since, for ought I know, there may be in the bowels of the Earth, (whence I have seen many concretes digg'd out, whose very names and outsides are for the most part unknown, even to Chymists themselves) divers other Bodies besides Salt-petre, whose steams may have a power of refrigerating the Air, as great in proportion to their / Quantity, as those of Salt-petre; and since common salt in artificial glaciations, is found to cooperate as powerfully, as Salt-petre it self, and since it is undeniably a Body, of which there is a vast quantity in the Terrestrial Globe, and which by reason of the Sea, where it abounds, is exceedingly diffus'd, I see no great reason, why we may not aswel esteem that kind of Salt among the Catholick efficientes of Cold, and the rather, because that the smallest Corpuscles, our eye discerns of Sea-salt, are wont to be, (though not exactly) of a Cubical figure, which is that figure, *Philoponus* informs us, the great *Democritus* of old (justly admir'd by *Gassendus*) assign'd to the Atoms of cold,<sup>†\*</sup> whereas, according to *Gassendus* himself, the Corpuscles

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of Nitre, at least as far as sense has inform'd us, are not the most conveniently shap'd to produce cold, since he labours to show, that the figure of frigorifick Atoms is to be Tetrahedral or Pyramidal, whereas the Crystals, or Grains, great or small, into which good Salt-petre shoots, / are wont to be Prismatical having their base Sexangular; but to return to what I was saying, concerning the congealing of water, with ice, I shall subjoyn, that the same Experiment countenances my conjecturing, that oftentimes it may not be emanations of one Salt, or other Body, but a peculiar and lucky conjunction of those of two or more sorts of them, that produces the intense degree of cold, as we see, that ice and snow themselves have their coldness advanc'd (as to its effects) by the mixture either of Sea-salt or Nitre, or spirit of Wine, or any other appropriated additaments. Nay, I may elsewhere have occasion to shew, that actual Cold, may be manifestly promoted, if not generated, by the addition of a Body that is not actually Cold. But to all this I must add, that I doubt whether any of those saline or Terrestrial expirations, either single or conjoyned, be the adequate causes of cold, since, for ought I know, there may be other ways of producing it, besides the introduction of frigorifick, whether Atoms or / Corpuscles, of which we may have occasion to take some notice hereafter. In the mean time, having discours'd thus long against the admitting a *primum frigidum*, I think it not amiss to take notice once more, that my design in playing the Sceptick on this subject, is not so much to reject other mens probable opinions, of a *primum frigidum*, as absolutely *false*, as 'tis to give an account, why I look upon them, as *doubtful*. /

## Title XVIII.

### *Experiments and Observations touching the Coldness and Temperature of the Air.*

1. I have shewn in the former Section, that the Air is not the *Primum Frigidum*, but yet I cannot readily yield my assent to the Opinion of the learned *Gassendus*, and some others, (who have written before, and since him) that the Air is of it self indifferent, that is, neither cold, nor hot, but as it happens to be made, either the one or the other by external Agents. For if we take Cold in the obvious and received Acception of the word, that is, for a Quality relative to the senses of a Man, whose Organs are in a good or middle Temper, in reference to Cold and Heat, I am hitherto inclinable to think / that we may rather attribute Coldness to the Air, then either Heat, or a perfect

Neutrality as to Heat and Cold. For to make a Body cold as to sense, it seems to be sufficient, that its minute Corpuscles do less agitate the small parts of our Organs of Feeling, then they are wont to be agitated by the Blood, and other fluid parts of the Body; and consequently, if supposing the Air devoid of those calorifick and frigorifick Atoms, to which the learned

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Men, I was naming, ascribe its heat and cold, it would constitute a fluid, which either by reason of the minuteness of its parts, or their want of a sufficiently vehement motion, would less affect the sensory of Feeling, then the internal liquors, and spirits of the body are wont to do, and so it would appear actually cold. Nor is it necessary, that all liquors, much less all fluids, should be as much agitated as the blood and vital humors of a humane body, as we see (to omit what in the last Section is mention'd about newly emitted Urine, and to skip other obvious instances) / in those Fishes and other Animals, whose Blood and analogous Juices are always, and that in the state, which passes for their natural state, actually Cold to our Touch. And I see no sufficient reason, why we should not conceive the Air even in its natural state, (at least as far forth as it can be said to have a natural state) to be one of the number of cold Fluids. For as to the main, if not only, Argument of *Gassendus*, and others, namely, That, as we see the Air to be easily heated by the Action of the Sun, or the fire, so we see it as easily refrigerated by ice, and snow, and Northerly winds, and other Efficients of Cold, and that heat and cold reign in it by turns in Summer and in Winter: This only proves, what I readily grant, that the Air is easily susceptible at several times of both these contrary Qualities, but it does not shew, that one is not more connatural to it, then the other, as we see, that the water may be easily depriv'd of its fluidity by the circumposition of snow and salt, and reduc'd to be fluid again by the Sun, / or the Fire; and yet according to them, as well as others, fluidity, not Firmness, is the natural quality of water. But this is not that, which I lay most weight upon, for I considered, that it is manifest and acknowledg'd by these learned Men themselves, that the heat of the Air is adventitious to it, and communicated by the beams of the Sun, or of the Fire, or by some other Agents naturally productive of heat, as well in other Bodies as the Air: And 'tis also evident, that upon the bare absence, (for ought else that appears) of the Sun, or Extinction of the Fire, or removal of the other causes of heat, the Air will, as it were of its own accord, be reduc'd to Coldness. Whereas, that there are swarms of frigorifick Atoms diffus'd through the Air, from which all its coldness proceeds, is but an *Hypothesis* of their own, far from being manifest in it self, and not hitherto, that I know of, prov'd by any fit Experiment or cogent reason. And though in some cases I am not averse to the admitting such Corpuscles, as may in a / sense, be styl'd frigorifick, yet I see not why we should have recourse to them in cases where such a bare cessation, or lessening of former motion, as may easily be ascrib'd to manifest causes, may serve the turn, as to a *Sensible* (for I now consider not the causes of the *Intenser*) Coldness in the Air, without taking them in. And the opinion, I incline to, has at least this advantage, that the Air seems to be as rightfully term'd cold, as Iron, Marble, Mercury, Crystal, Salt-petre, and such other Bodies, which men unanimously look upon as such, there being none of these to which the Argument imploy'd against the coldness of the Air, is not applicable, save that the Air being a fluid of a looser and finer Texture does sooner receive, and lose the impressions of heat and cold. And yet if a Block

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of Marble, for instance, or an Iron Bullet were remov'd into one of those empty spaces, that *Gassendus* and some others suppos'd to be beyond the bounds of this world, I see not why it should not be rather cold, then either warm, or in a state of perfect / Neutrality: Since when the Corpuscles of Heat, and those of Cold had extricated themselves, and were flown away into the neighbouring *Vacuum*, the component Particles of the stone or metal, whose implicated Texture would hinder their Dissiliation, remaining much less agitated then our Organs of feeling are by the warm blood and spirits, that vivifie them, must, if applied to those sensories, appear Cold.

2. But I shall not upon this subject spend any farther discourse, since perhaps the dispute, either

may *be*, or at least may easily *be made* Verbal: For in case those I argue with, should so explain their opinion, as not to deny, that in its own nature the Air, left to its self, may be reputed *Cold* in reference to the sensories of men, who are warm animals: But say, that nevertheless, comparing it indefinitely to other then humane bodies here below, it is so easily susceptible of both the contrary qualities, that neither of them seems predominant in it; and that when it is considerably either cold or hot, it is made / so by adventitious agents: I shall not much contend with them, especially if it can clearly be made out, that there are great quantities of such cold spirits, as *Cabæus* and *Gassendus* suppos'd to be universally productive of cold (more or less) in all bodies, where they get admission;†\* but of these cold spirits more perhaps elsewhere. Our principal business in this Section being to deliver *Experiments* and *Observations*, and because we shall mention but few of the former sort, we will dispatch them first.

3. [*November* the 20. 1662. we took a Weather-glass fill'd to a convenient height with well rectifi'd spirit of Wine, and Hermetically seal'd, this we inclos'd in a glass Receiver of a Cylindrical form, of about two inches Diameter, and about a foot and a half high, and having cemented on the Receiver, we let it alone for some hours, that it might perfectly cool. Then drawing out the Air, and watching it narrowly, we observ'd, that the liquor in the Weather-glass descended a little, though but a very little upon the first / Exuction of the Air, and a little, though it seem'd somewhat less, upon the second, but afterwards we did not find it sensibly to descend. This subsidence of the liquor in all amounting to about the length of a Barley corn, we attributed to the stretching of the glass by the spring of the included Air, when the ambient was withdrawn, and accordingly upon our allowing a Regress to the excluded Air, we saw the spirit in the Thermometer, rise about half a Barley-corns length to the place whence it began to subside. Afterwards we suck'd out, and let in the Air of the Receiver, as before, with like success, as to the descent and remounting of the liquor.†\*

4. *N.B.* We tri'd with a very hot Handkerchief appli'd in a convenient

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place to the outside of the Receiver, whether the included Weather-glass would receive impressions from it, the Air, that was wont to be intermediate, being remov'd; but we did not find the liquor in the Weather-glass sensibly to swell, either by this way, or by casting upon it the concentrated / beams of a candle trajected through a double convex glass. But when the Air was readmitted into the Cavity of the Receiver, then the same Handkerchief, heated a fresh, and applied, made the spirit of Wine sensibly, though but little more, to ascend: Of which yet it seem'd something difficult by reason of the Nicety of the Experiment to estimate with any thing of certainty the Cause.] So that upon the whole matter, till the Experiment be repeated in Airs of differing tempers, to verifie, whether 'twas the withdrawing of the wonted pressure, or the recess of the substance of the Air, that made the liquor included in the Thermoscope subside, and till the Experiment be repeated with the further observation of other circumstances (which reiteration of the Trial we intended, but were by intervening accidents hindred) the recited Experiment will not afford much more then good hints towards the Discovery of the Temperature of the Air.

5. I have elsewhere taken notice,†@ that air included in Vessels sufficiently / strong and well clos'd, was not *sensibly*, or at least not *considerably* condens'd by Cold, but when the Air was not so included, as not to be in some part or other expos'd to the pressure of the outward Air or Atmosphere, it would then by a degree of Cold, capable to freez water, be manifestly reduc'd into a less room. But how much this Contraction or Condensation of the air may amount to, I did not there subjoyn, nor has the measuring of it been, that I know of, attempted by any man. Wherefore we thought fit to indeavour something in this kind, of which we shall annex a brief account, whereby it will appear upon the whole matter, that in the Climate, we live in, the Cold does not so considerably condense the Air, as most men seem to have hitherto imagin'd.

6. And first, it will not be amiss to intimate, that among other ways we tried to measure the



shrinking of the Air by sealing it up in glasses furnish'd with long and very slender stems, that by breaking off the tips of those glasses immers'd under water, when by / the Cold Air of a frosty night, or the Circumposition of snow and salt, the included air was highly refrigerated, the water might (by the pressure of the Atmosphere upon it) be impell'd into the Cylindrical cavity of the broken glass, and by its greater or lesser Ascent therein shew, how much the internal Air had been made to shrink upon the account of the Cold. But this way, for reasons too long to be here deduc'd, we found it troublesome and difficult to practise with any thing of certainty. Nor did we ever, that I remember, by this way bring the refrigerated air to lose above a 30. part of its former dimensions.

7. We would have tried also to measure the Condensation of the air by the ascent of water into the stem of a Bolthead, so inverted, that the orifice of

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the stem might be under the surface of the water, and the Bolthead kept erected. But this way we disapproved, because it was likely (and indeed we found it so by experience) that the external air would first freez the uppermost part of the / water contain'd in the stem, and thereby hinder its ascent, and perhaps occasion the bursting of the lower part of the said stem.

8. Wherefore though for want of a sufficient Quantity of some liquor, that would neither freez like water, and aqueous Bodies, nor congeal like common oyl, and the like unctuous Juices, we found it for a while somewhat difficult to practise the Experiment, yet bethinking our selves of the indisposition that Brine has to Congelation, we made so strong a Brine with common salt, that with it (and as I remember, with oyl of Turpentine also, of which we chanc'd to have some quantity by us) we made divers Trials, of which I had two among our Collections, which we shall here subjoyn, whereof the one informs us, that an Egg being inverted into salt water, the Cold of a frosty night made the air shrink in the Pipe near five inches; and the other (which is the accuratest I meet with among my Collections) gives me this account, That *January* the 29. the Air extended into 2057. spaces, / was by the cold of the sharp and frosty night contracted into 1965. spaces, so that in extraordinarily cold weather, the most we could make the Air lose of its former dimensions by the additional Cold of the Atmosphere, was a 22. part, and a little more then a third: And this was the greatest condensation of the Air, that we remember our selves to have observ'd, though we were so careful, as after we had placed marks, where the incongealable liquor reach'd in the pipe, that when the internal air was expos'd abroad to the cold, we caused servants to watch, and from time to time to take notice (by placing marks) of the various ascents of the liquor, especially early in the morning, least we should omit taking notice of the greatest contraction of the air, which omission (by reason that the Coldness of the ambient air does oftentimes begin to be remitted before we can feel it to be so) is not easily avoided without watchfulness.

9. But having thus observ'd the Condensation of included air by the natural / and unassisted Cold of the external air, we thought fit to prosecute the trial somewhat further, and in regard we conceiv'd the Cold of a mixture of snow and salt to be far more intense, then that of the mere ambient air alone, we endeavoured to measure, as near as we could, how much the one exceeded the other: And though we found, that by prosecuting the lately mention'd Trial in the glass-Egg by the application of ice and salt to the Elliptical part of the vessel, the liquor rise by our Estimate near four inches more (then those five which it had risen already, upon the account of the Refrigeration of the included air by the bare cold of the external:) Yet by prosecuting the other Experiment (made the 29. of *January*) at the same time, when we were making it, we did somewhat more accurately determine the matter. For by applying ice and salt to the outside of the vessel, we found, that the included air was contracted from 1965. spaces, to which the Cold of the ambient air had reduc'd it, into 1860. spaces, so that / the Circumposition of

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ice and salt did as much, nay somewhat more condense it, after the mere Cold of the external air had contracted it as far as it could, then the bare, though intense, Cold of the ambient air could condense it at first, and the greatest degree of adventitious Cold we were able to give by the help of nature or of art, did not make the air expos'd to it, lose a full tenth part of its former Dimensions: on which occasion it may not be unworthy observation, That there is no greater Disparity betwixt the proportion in which the Cold was able to condense the Air, and that wherein the Cold was able to expand water.

10. This is all that at present I think fit to say concerning the interest that Winds may have in the Temperature of the Air. And therefore I will now proceed to those other particulars, wherewith I not long since said, that I intended to close up this Section; and I might on this occasion subjoyn many things, but partly haste, and partly other considerations will confine me to those, that relate to / the effects of Cold upon the Air in a more general way.

11. And first, we will observe, that Cold may hinder in an almost incredible measure, the warming operation of the Sun upon the Air, not only in the hottest part of the Day (for that may sometimes happen, even in our Climate) but at several times of the Day, even in the heat of Summer.

12. I remember I once accidentally met with an intelligent and sober Gentleman, who had several times sail'd upon the frigid Zone, and though an intervening accident separated us so suddenly, that I had not opportunity to obtain from him the resolution of above two or three questions; yet this I learned of him belonging to our present purpose, That by the help of a Journal he kept, he call'd to mind, that upon the coast of *Greenland* he had observ'd it to snow all Midsummer night, which affirmation of so credible a person, imboldens me to add some other relations, which I should else have scrupled at.<sup>†\*</sup> /

13. Mr. *Logan* an English Merchant, that Winter'd at *Pecora*, one of the Northern Towns of *Muscovy*, relates, that being there at a great Salmon-fishing, there hapned about the close of *August* (which in many Countries is wont to be the hottest time of all the year) so strong a Frost, which lasted till the fourth day, *That the Oзера was frozen over, and the Ice driving in the River to and again, broke all the Nets, so that they got no Salmon, no not so much as for their own Victuals.*<sup>†@</sup>

14. Captain *G. Weymouth* mentions, that in *July*, though he was not near the Latitude of *Nova Zembla*, much less of *Greenland*, yet sailing in a thick fog, when by reason of the darkness, it occasioned, *he thought good to take in some of his sails, when his men came to hand them, they found their Sails, Ropes, and Tacklings so hard frozen, that it did* (says he) *seem very strange unto us, being in the chiefest time of Summer.*<sup>†@</sup>

15. In the fifth Voyage of the English to *Cherry Island*, which lies betwixt 74. and 75. degrees of Latitude, they observ'd,<sup>†@</sup> that the wind being at /

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North-east upon the 24. of *July*, *It freez'd so hard, that the Ice did hang on their clothes.* And in the seventh Voyage (which was made three years after) to the same Island, they mention, that on the 14. of *July* the wind being Northerly, *they had both snow and frost.*<sup>†@</sup>

16. The next thing that we shall take notice of, is the *degree* of Cold, which the Efficient causes of that Quality, whatever they be, are able to produce in the air; but of this we must not here treat indefinitely, the strange effects of cold upon other bodies being most of them produc'd by the intervention of the cold first diffus'd in the Air, and those are treated of in a distinct Section, wherefore we shall now give two or three instances of the *sudden* operations of the Cold harbour'd in the Air.

The formerly mention'd English Ambassador into *Russia*, Dr. *Fletcher*, gives us two instances very memorable to our present purpose.<sup>†@</sup> *When you pass* (says he) *out of a warm Room into a Cold, you will sensibly feel your breath to wax stark, and even stifling / with the cold, as you draw it in and out.* So powerfully and nimbly does the intensely refrigerated Air work upon the Organs of respiration.

[And whereas a very credible person, now chief Physician to the *Russian* Emperor,<sup>†\*</sup> being ask'd by me concerning the truth of what is reported, sometimes to happen at *Musco*, and is reputed the eminentest proof that is readily observable of the extreme coldness of the air, assur'd me, that he himself saw the water thrown up into the air, fall down actually congeal'd into ice: Dr. *Fletcher* confirms this Report. For] our Ambassador also says, That *the sharpness of the Air you may judge of by this, for that water dropped down, or cast up into the Air, congeal'd into Ice before it come to ground.*<sup>†@</sup> And I remember, that inquiring about the probability of such Relations, he answered me,<sup>†\*</sup> That being at the famous Seige of *Smolensko* in *Russia*, he observ'd it to be so extremely cold in the fields, that his Spittle would freez in falling betwixt his mouth and the ground, and that if he spit against a Tree, or a / piece of wood, it would not stick, but fall to the foot of it.<sup>†\*</sup>

17. Among the *Phænomena* of Cold, relating to the air, I endeavour'd to observe, whether upon the change of the Weather, from warm or mild, to cold and frosty, there would appear any difference of the weight of the

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Atmosphere by its being plentifully furnish'd with a new stock of such frigorifick Corpuscles as several of the modern Philosophers ascribe its coldness to, but though I several times observ'd by comparing a good Barometer (and sometimes also unseal'd Weather-glasses furnish'd one with a tinted Liquor, and the other with Quicksilver) with a good seal'd Weather-glass, furnished with pure spirit of Wine, that upon the coming in of clear and frosty weather, the Atmosphere would very early appear sensibly heavier then before, and continue so, as long as the cold and clear weather lasted; yet by reason of some considerations and Trials, that breed some scruple in me, I refer the matter to more frequent and lasting observations, then I yet have been / able to make, in which it will concern those that have a mind to prosecute such Trials, not only to consider, whether or no the increased gravity of the Atmosphere may not proceed from some other Cause, then the coming of frigorifick Atoms into the Air; but to have a special care, that their Barascopes be more carefully freed from the Air, that is wont to lurk in Quicksilver it self, as well as other Liquors, then those in the making of the Torricellian Experiment Tubes usually are, least that Air getting up into the deserted part of the Tube, do by its expansion and contraction, obtain an unsuspected interest in the rising and falling of the subjacent Mercurial Cyinder, and so impose upon them.

18. Another Effect that the Cold especially in Northern Countries has oftentimes upon the Atmosphere, is, the making the Air more or less clear then usually it is. For in the Northern Voyages, the Seamen frequently complain of thick and lasting Fogs, whose causes I shall not now consider, but some help to guess at them / may be given by what we are about to add, namely, that it very frequently happens on the contrary, That when the cold is very intense, the air grows much clearer then at other times, probably because the Cold by condensing precipitates the vapours, that thicken the air, and by freezing the surface of the earth, keeps in the steams, that would else arise to thicken the air. Not to dispute, whether it may not also somewhat repress the vapours, that would be afforded by the water it self, since some of our Navigators observe, that even when it was not cold enough to freez the surface of the Sea, it would so far chill and infrigidate it, that the snow would lye on it without melting.

19. I remember a *Swedish* extraordinary Ambassador, and a very knowing person, whom I had the honour to be particularly acquainted with, would say, when he saw a frosty day accompanied with

great clearness, that it then look'd like a *Swedish* winter, where when once the frosty weather is settled, the sky is wont for a / very long time to be very serene and pleasant, and here in *England* we usually observe the sharpest frosty nights to be the clearest. But to confirm our Observation by a very remarkable instance, I shall borrow it from a Navigator very curious of Celestial Observations, which circumstance I mention to bring the greater credit to the following observation of Captain *James*, which in his Journal is thus delivered:<sup>†@</sup> *The thirtieth and one and thirtieth*

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*of January, there appeared in the beginning of the night more Stars in the Firmament, then ever I had before seen by two thirds. I could see the Cloud in Cancer full of small Stars.*

20. To determine what effect the coldness of the air may have upon the Refractions of the Luminaries and other Stars, I look upon as a work of no small difficulty, and that would require much consideration as well as time, wherefore I shall only add two or three narratives, supplied me by Navigators, without adding at present any thing to the matters of fact.

21. The first is that famous Observation of the *Dutch* in *Nova Zembla*,<sup>†\*</sup> who / take great pains to evince by several circumstances, some of them highly probable, that they were not mistaken in their account of time, according to which they concluded, that they saw the Sun, whom they had lost sight of eleven weeks before, about fourteen days sooner then he ought to have appear'd to them, which difference has been, for ought I know to the contrary, by all that have taken notice of it, ascrib'd to the strangely great Refraction in that Gelid and Northern air.

22. And as for that other extremely cold Country, where Captain *James* wintered, it appears by his Journal, that he there made divers Celestial, and other observations, which gave him opportunity to take notice of the Refraction, and he seems to complain, that he found it very great, though among the particulars he takes notice of, there are some that seem not very strange, nor are there any that are near so wonderful, as that newly mention'd of the *Hollanders* in *Nova Zembla*, however in regard of the extreme coldness of the Winter / air in *Charleton Island*, it may be worth while to take notice of the following passages out of his Journal, since they may at least help us to conjecture what is not to be expected in reference to Refractions from the coldness of the air as such. *The 21.*

*of January* (says he)<sup>†@</sup> *I observed the Latitude with what exactness I could (it being very clear Sunshine weather) which I found to be 52. degr. 52. min. This difference is by reason that here is a great Refraction.* Which last clause is very obscure, unless it refers, as one may guess it does, to what he had elsewhere said, That his first coming to the Island, *he took the Latitude with two Quadrants, and found it to be just 52. degrees, without any minutes.*<sup>†@</sup> Elsewhere; *my observations* (says he)<sup>†@</sup> *by these glasses I compar'd to the Stars coming to the Meridian. By this means we found the Sun to rise twenty minutes before it should, and in the evening to remain above the horizon twenty minutes (or thereabouts) longer then it should. And all this by reason of the Refraction.*

And in another place, *March the / 15. This evening* (says he)<sup>†@</sup> *the moon rose in a very long oval amongst the Horizon.*

I shall add one passage more out of our Author, concerning Refractions,

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not only because it may bear Testimony to some relations of the like kind, that I have mention'd in another Treatise; but because it is concluded with an observation, that (if there be nothing of mistake in it) is odd enough. *I had often* (says he)<sup>†@</sup> *observed the difference betwixt clear weather, and misty Refractive weather in this manner. From a little Hill, which was near adjoyning to our house, in the clearest weather, when the Sun shone, with all the purity of Air that I could conceive,*



*we could not see a little Island, which bare of us south south-east some four leagues of; but if the weather were misty (as afore-said) then we could often see it from the lowest place.*

23. Hitherto I have treated of the Temperature of the Air in general, and though the past Discourse have been prolix enough, yet possibly I may have no fewer things to say, if I would at present fall upon the particular / consideration of the three Regions into which the Air is wont to be distinguished. For I confess I am not altogether without scruples, both as to the Number, and as to the Limits, and as to the Qualities assign'd to these Aerial Regions. But (as I have partly declar'd in another Tract<sup>†@</sup>) though I had time to enter upon so intricate a Disquisition, yet till I have an opportunity to consult some other papers, I know not whether what I have noted touching these difficulties, may not more properly belong to another Treatise, then this of Cold.

24. Having thus dispatch'd the few Experiments I can meet with among my papers, concerning the Coldness of the Air, I now proceed to subjoyn some observations, that have occur'd to me in the writings or verbal Relations of Navigators and Travellers about that subject. But in regard, that the greatest part of the *Phænomena* of Cold, which nature of her own accord presents us with, seem to be produc'd, either mediately or immediately by the Air, we intend not / here to treat of the coldness of the air in the largest sense, but only to take notice of some of the choicer instances, that seem to belong to our present Argument. And these we shall annex, either as Promiscuous Observations at the Close of this Section, or as Illustrations or proofs of the three following Observations.

I. The first I shall propose in these terms, *that the greater or lesser coldness of the Air in several Climates and Countries, is nothing near so regularly proportionate to their respective distances from the Pole, or their vicinity to the Equator, as men are wont to presume.*

This puts me in mind of what I have formerly, either heard from a skilful man, or observ'd my self about the difference betwixt places of the same latitude in the Northern and Southern Hemisphere; namely, That of places equally distant the one from the Northern, the other from the Southern Pole, the latter are generally much colder then the former. And as I remember, I long since noted some things to this purpose; but being not at present able to recover / them, I shall propose this only, as that which may deserve an inquiry, being not yet satisfi'd, but that in the Examples I had taken notice of, some accidental and concurrent causes may have occasion'd the greater

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coldness observ'd in the places seated on the other side of the Line; as on this side of it, the like causes may much vary the coldness of differing places of equal latitudes, as we are now going to shew by the following testimonies.

1. How excessive a Cold reigns at *Musco* and thereabouts in the Winter time, when many men lose their noses or their toes, and some their lives by the extremity of the cold, we have several times occasion to take notice of in this Treatise. And yet at *Edenburgh*, which I find some of our modern Navigators to place more Northerly by above a degree, there, I say, and in the neighbouring places, the air is known to be temperate enough, and the cold very tolerable: And 'tis affirm'd, that the snow very rarely lyes any long time on the ground after it is fallen. /

2. In the Voyage made for discoveries northward, by Mr. *Poole*, in the year 1610. I find this passage,<sup>†@</sup> *I was certifi'd, that all the Ponds and Lakes were unfrozen, they being fresh water, which putteth me in hope of a mild Summer here, after so sharp a beginning, as I have had, and my opinion is such (and I assure my self it is so) that a passage may be as soon attain'd this way by the Pole, as any unknown way whatsoever, by reason the Sun doth give a great heat in this Climate; and the Ice (near the 79. degree) I mean that that freezeth here, is nothing so huge as I have seen in 73. degrees.*

To this agrees the testimony of the *Hollanders* in their first Voyage to *Nova Zembla*, in which the writer of it, *Gerat de Veer*, speaks thus,†@ *We have assuredly found, that the only and most hinderance to our Voyage, was the Ice, that we found about Nova Zembla, under 73, 74, 75, and 76. degrees, and not so much upon the Sea, between both the lands, whereby it appeareth, that not the nearness of the North Pole, but the Ice that cometh in and out from the Tartarian Sea about Nova Zembla, caused / us to feel the greatest cold. Therefore in regard, that the nearness of the Pole was not the cause of the great cold that we felt. &c.* And a little after,—*It is true* (says he) *that in the Country lying under 80. degrees (which we esteem to be Greenland) there is both leaves and grass to be seen, wherein such beasts, as feed of leaves and grass, as Harts, Hinds, and such like beasts, live, whereas to the contrary in Nova Zembla, there groweth neither leaves nor grass, and there are no beasts there, but such as eat flesh, as Bears and Foxes, &c. although Nova Zembla lyeth 4, 5, and 6. degrees more Southerly from the Pole, then the other land aforesaid.*

And to this purpose I remember what is related by the learned *Josephus Acosta*, concerning the Heats and Colds in the Torrid Zone, and elsewhere:†@ *When I pass'd* (says he) *to the Indies, I will tell what chanc'd unto me, having read what Poets and Philosophers write of the burning Zone, I perswaded my self, that coming to the Æquinoctial, I should not indure the violent heat, but it fell out otherwise, for when I pass'd, which was when the Sun was there for Zenith, / being entered into Aries, in the moneth of March I felt so great a cold, as I was forc'd to go into the Sun to warm me: what could I else do then but laugh at Aristotles Meteors, and his Philosophy, seeing that in that place, and at that season, when as all should be scorch'd*

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*with heat, according to his rules, I and all my companions were a cold? in truth there is no Region in the world more pleasant and temperate, then under the Equinoctial, although it be not in all parts of an equal temperature, but have great diversities. The burning Zone in some parts is very temperate, as in Quitto, and on the plains of Peru, in some parts very cold, as at Potosi, and in some very hot, as in Ethiopia, Brasile, and the Molucques. And within two Chapters after, he discourses more largely of some of these Particulars. And again Chapter the 12. You may continually* (says he)†@ *see upon the tops of these mountains snow, hail, and frozen waters, and the cold so bitter, as the grass is all wither'd, so as the men and beasts, which pass that way, are benumm'd with cold. This, as I have said, is in the burning Zone, and it happens most commonly / when they have the Sun for Zenith.*

These Testimonies of a learned man, that writes upon his own knowledge, I thought it worth producing, to make it probable, that as in several Countries the heat does not always answer to the nearness of places to the Line, so in Northern Regions the cold may not always be proportionate to their vicinity to the Pole. In Mr. *Hudsons* second voyage written by himself, he mentions that above 71. degrees, though they were much pester'd with ice, about the end of *June, that day* (when this hapned) *was calm, clear, and hot weather, adding of the next day also, that it was calm, hot, and fair weather.*†@ And *Acosta* tells us, *that we see these differences, not only on the land, but also on the Sea: there are some Seas where they feel great heat, as the report of that of Mazambigus and Ormus in the east, and of the Sea of Panama in the west. There are other Seas in the same degree of height very cold, as that of Peru, in the which we were a cold, when we first sail'd it, which was in March, when the Sun was directly over us. In / truth on this continent, where the Land and Sea are of one sort, we cannot imagine any other cause of this so great a difference, but the quality of the wind that doth refresh them.*

But to multiply no more instances, we shall conclude with this one, That *Charleton Island*, where Captain *James* winter'd (and of which we so often have occasion to make mention in our History) though it seems by the effects to be a colder Region, then even the Countrey about *Musco*, and perhaps as cold as *Nova Zembla* it self; yet Captain *James*, who had several times occasion to take the latitude of it, and assigns it the same Elevation, and consequently, the same Distance from the

Pole with *Cambridge*, whose latitude he reckons to be 51. degrees besides minutes, and whose air is very well known to be very temperate.<sup>†@</sup> And it is remarkable, that though this place, whose latitude is short of 52. degrees, was found uninhabitable by reason of the cold, yet not only in Mr. *Hudsons Voyage*,<sup>†@</sup> the writers admonish the Readers to take notice, *That although / they ran along near the shore, they found no great cold, which made them think, that if they had been on shore the place is temperate*:<sup>†@</sup> And yet in this place they reckon themselves to have reach'd the 78.

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degree of latitude: And our recenter Navigations inform us, that several parts of *Greenland*, to which this newly mentioned coast belong'd, are well enough inhabited: And one of our English Navigators assures us, that the true height of *Pustozera* in *Russia* is no less then 68. degrees and a half, if not more, and yet that is a town not only well inhabited, but of great trade; but in *Hudsons voyage* I find what is more strange, That under the 81. degree of latitude, beyond which they discovered land very far off, but (beyond which none is thought to have actually sail'd toward the Pole) *they found it during the whole day clear weather, with little wind, and reasonable warm*.<sup>†@</sup> And beyond 80. degrees, they not only found a stream or two of fresh water, but *found it hot on the shore, and drank water to cool their thirst, which they also commended.* /

II. The next observable I am to propose about the coldness of the Air, is this, That the degrees both of Heat and Cold in the air may be much greater in the same climate, and the same place, at several seasons of the year, or even at several times of the same day, then most men would believe.

For the proof of this Proposition, we shall subjoyn two sorts of Testimonies, of Travellers, and Navigators, the former shewing, that in Countries, where it is very cold in Winter, it may nevertheless be hot in Summer; and the latter manifesting, that even on the same day, as well as in the same place, the heat and cold, that succeed one another, may be one of them sensible, though the other were extreme, or may perhaps be both of them considerable.

To make this good, we shall produce the following Testimonies.

1. Dr. *Giles Fletcher*, English Ambassador to the *Muscovian* Emperor, in his Treatise of *Russia*, and the adjoining Regions, has this memorable / passage to our present purpose.<sup>†@</sup> *The whole Countrey* (says he) *differeth very much from it self, by reason of the year, so that a man would marvel to see the great alteration, and difference betwixt the Winters and Summers in Russia. The whole Countrey in the Winter lyeth under snow, which falleth continually, and is sometime of a yard or two thick, but greater towards the North; the Rivers, and other waters are all frozen up, a yard or more thick, how swift or broad soever they be, and this continueth commonly for five moneths, to wit, from the beginning of November, till towards the end of March; what time the snow beginneth to melt, so that it would breed a frost in a man to look abroad at that time, and see the Winters face of that Countrey.* And a little after he adds<sup>†@</sup> *And yet in the Summer time you shall see such a new hew and face of a Countrey, the woods (which for the most part are all of Fir and Birch) so fresh, and so sweet; the Pastures and Meadows so green, and well grown (and that upon the sudden) such variety of flowers, such noise of Birds (especially of Nightingals, that seem to be more loud, and of a more variable note, then in other / Countries) that a man shall not lightly travel in a more pleasant Countrey.* And some lines after, *As the Winter exceedeth in cold, so the Summer inclineth to over much heat, especially in the moneths of June, July, and August, being much warmer then the Summer Air in England.*

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Almost like things have been much more recently affirm'd by the learned *Olearius*, Secretary to the Duke of *Holstein's* Embassy into *Russia*, and now *Bibliothecarius* to the present Prince of



*Holstein*.<sup>†@</sup> And an acquaintance of mine, who, after having liv'd in *Italy*, pass'd a Summer in *Russia*, assur'd me, that he scarce in *Italy* did ever eat better Melons, then some which he had eaten at *Musco*, of a strange bigness, which bears witness to that almost incredible Relation of *Olearius*, who (after having much prais'd their goodness at *Musco*) affirms, that he there met with Melons of 40. pound weight, of which he there teaches the Culture.<sup>†@</sup>

At the royal City of *China*,<sup>†@</sup> which scarce exceeding the 42. degrees / of latitude, one would expect, that as the Summer is very warm, so the Winter should be very mild, as it is observ'd to be in divers places of *Spain*, *Italy*, and *Greece*, that have the same, or a more Northern latitude: and yet the learned Jesuite *Martinius*, who liv'd many years in *China*, assures us,<sup>†\*</sup> that usually for four whole moneths together, all the Rivers are so hard frozen, that not only all Ships are clos'd, and kept immovable by the Ice, but that also horses, wagons, and even the heaviest carriages do securely pass over the Ice. Concerning which, he adds this strange circumstance, that 'tis usually made in one day, though to its dissolution it require many.

*Prosper Alpinus* in his learned Treatise *de medicina Ægyptiorum*,<sup>†@</sup> tells us, that at *Grand Cayro*, where he practis'd Physick, though that famous *Metropolis* of *Ægypt* be distant but six degrees from the Tropick of *Cancer*, yet the Air, which in Summer is almost insupportably hot, in Winter is sometimes very considerably cold; adding, that there is not / any sort of Diseases that proceed (as he is pleas'd to speak) from distillations from the head, to which the people are not there subject: To these instances we shall annex but two more, but those remarkable ones.

The first is mention'd by *Purchase*, as communicated to him by an eye witness, in these words.<sup>†\*</sup> This I thought good at our parting to advertise thee, That Mr. *Hebey* hath affirm'd to me, touching the diversity of weather in *Greenland*, that one day it hath been so cold (the wind blowing out of some quarter) that they could scarce handle the frozen Sails; another day so hot, that the pitch melted of the Ship, so that hardly they could keep their Clothes from pollution: yea, he hath seen at midnight Tobacco lighted or fired by the Sun beams with a glass. The other example I am to produce, is no less remarkable; namely, that in the often mention'd *Charleton Island*, where that

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winter was as sharp, perhaps as any known place of the habitable world, Captain *James* his Journal gives us this account of the / weather: *In June the sixteenth* (says he<sup>†@</sup>) *was wondrous hot, with some thunder and lightning, so that our men did go into the Ponds ashore to swim, and cool themselves, yet was the water very cold still. Here had lately appeared divers sorts of Flies, as Butterflies, Butchers-flies, Horse-flies, and such an infinite abundance of blood-thirsty Muskitoes, that we were more tormented with them, then ever we were with the cold weather. These (I think) lye dead in the old rotten wood all the Winter, and in Summer they revive again. Here be likewise infinite companies of Ants, and Frogs in the Ponds upon the land.*

Thus we see, what difference there may be in the same place, betwixt the temperature of the Air in Winter, and Summer. We shall now add what may appear more strange, that there may be very great disparities in the heat and coldness of the air, not only in the same place, but within the compass of the same day.

The lately mention'd *Alpinus*, affords me an example to this purpose, in *Ægypt* its self, where one / would expect a much more uniform heat. *Hyeme* (says he<sup>†@</sup>) *nocturnus aer admodum frigidus observatur, qui ob orto sole paulo post, parum incalescit, in meridieque plurimum: adveniente vere nocte rursum in frigidum permutatur, ita, ut aer ille valdè inæqualis sit dicendus, ab ipsiusque illa inæqualitate plurimi morbi originem ducunt atque generantur, qui eo tempore per urbem vagantur.*

The learned *Olearius* relating how he travelled with the Ambassadors, whose Secretary he was,



over a branch of mount *Taurus*, takes notice,<sup>†\*</sup> that it being after the middle of *June*, the air of that hot region of *Persia* oblig'd them only to travel by night, and yet the nocturnal cold was so great, that they were all benumbed with it, insomuch, that they were hardly able to alight from their Horses; adding, that the sudden change from an extreme cold, to the excessive heat, they were again expos'd to the next day, cast no less then 15. of their company into strong burning feavers at once. (Which brought into my mind the complaint of good *Jacob*, who, though he liv'd / in an Eastern Countrey, when he had said, *that in the day the drought consumed him*, adds, *and the frost by night*.<sup>†\*</sup>)

And the same curious traveller mentions, that in another Countrey in *Persia*, call'd *Faclu*, notwithstanding the heat of the region (at the end of *March*, at which time they pass'd that way) they saw and felt in one night, which they were forc't to pass without their tents, both lightning, and thunders, and winds, and rain, and snow, and ice.

We will conclude with a remarkable instance, afforded us by the Journal of the English that wintred at *Charleton Island*.<sup>†\*</sup> *The season here in this Climate*

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(says the often quoted Author of the Voyage) *is most unnatural; for in the day time it will be extreme hot, yea, not indurable in the Sun, which is, by reason that it is a sandy Countrey. In the night again, it will freez an inch thick in the Ponds, and in the Tubs about, and in our house, and all this towards the latter end of June.*

III. The third observable I intended to take notice of, about the Coldness of the Air, may be compriz'd in / this Proposition, That in many places the Temperature of the Air, as to Cold and Heat, seems not to depend so much upon the Elevation of the Pole, as upon the Nature and Circumstances of the winds that blow there.

It would require a very long Discourse, to treat in this place of Winds in general, and much more to examine the several causes of winds, that are assigned by several Authors, and therefore when I have once given this intimation, that divers of these opinions may be more easily reconcil'd, then the maintainers of them seem to have thought, to the Truth, if not to one another: The causes that may produce wind, being so various, that many of those propos'd, may each of them in some cases be true, though none of them in all cases be sufficient: having hinted this, I say, it may suffice on this occasion, to subjoyn three or four observations, to prove and illustrate the matter of fact delivered in the Proposition.

And first, 'tis a known Observation / in these parts of the world, that Northerly and Northeasterly winds, do at all times of the year bring cold along with them, and commonly if it be Winter, Frost. And here in *England* I have sometimes wondred at the power of the winds, to bring not only sudden Frosts, but sudden Thaws, when the frost was expected to be settled, and durable, which yet seems to hold commonly, but not without exception. For during one of the considerable Fits of Frost and Snow, that I have taken notice of in *England*, I remember, that I observed (not without some wonder) that the Wind was many days Southerly,<sup>†@</sup> unless it may be said, That this Southerly Wind was but the Return of a stream of Northerly Wind, which had blown for many days before, and might by some obstacles, and agents, not here to be inquir'd after, be made to wheel about, or recoyl hither, before it had lost the / greatest portion of the refrigerating Corpuscles it consisted of before.

The formerly mention'd *Prosper Alpinus*, attributes strange things to the Northerly wind, that blows in *Ægypt*, as to the cooling and refreshing the Air, in spite of the violent heats, that would otherwise be intollerable. (And many in *Egypt* ascribe to the *Ætesian* Winds, that almost miraculous ceasing of the Plague at *Grand Cairo*, of which we elsewhere speak.<sup>†@</sup>) *Dominatur*

*autem aer (says he<sup>†@</sup>) summè calidus, ipsius cæli, ut dictum est, ratione, quod hæc civitas à Tropico Cancræ tantum 6. gradibus distet. Quâ brevi inter-capedine dum sol ad illum accedit Tropicum, & illorum Zenith fit propinquior, aer ille valdè incalescit, & nisi Ætesiaë venti tunc à septentrione spirarent, vehementissimus, & qui vix à nostris perferri possit, caloris æstus sentiretur.*

*Advenæ nostri iis provenientibus ad subterranea loca confugiunt, in quibus / morantur quousque ille ventorum ardor residerit atque cessaverit. Conjunxit hæc incommoda Deus Optimus, cum aliis quibusdam bonis, nam ubi calidissimi illi venti contiguere, statim à Septentrione flare alii incipiunt, qui subitaneum inflammatis atque laxatis corporibus solatium præstant. Si enim illi diu perseveraverint, nemo in eâ regione vivere possit.<sup>†@</sup>*

Whence winds should have this power to change the Constitution of the Air, and especially to bring cold along with them, is not so easie to be determin'd. Indeed the other Qualities, and even the heat, that is observable in winds, may for the most part be probably enough deriv'd from the Qualities of the places, by which they pass. Of this we have already given an example or two in the passages lately mention'd. And it may be further confirm'd by what *Acosta* says, that he himself saw in some parts of the *Indies*:<sup>†@</sup> namely, *That the Iron Grates were so rusted and consumed by a peculiar wind, that pressing the mettall between your fingers, it would be dissolv'd, and crumbled, as if it had been Hay or parched Straw.* And / this Learned Traveller, who seems to have taken peculiar notice of the winds, affords us in divers places of his Book several Examples to confirm what we were saying (though he take not the nature of the regions, along which the wind blows, to be alone in all cases a sufficient Cause of their Qualities) of which yet we shall now mention but these two memorable passages.<sup>†@</sup> *In a small distance (says he) you shall see in one wind many diversities. For example, the Solanus or Eastern wind is commonly hot and troublesome in Spain; and in Murria, it is the coldest and healthfullest that is, for that it passeth by the Orchards, and that large Champiane which we see very fresh. In Carthagene, which is not far from thence, the same wind is troublesome, and unwholsome. The Meridional (which they of the Ocean call South, and those of the Mediterranean Sea, Mezo Giorno) commonly is rainy, and boisterous, and in the same City, whereof I speak, it is wholsome and pleasant.* And in his Description of *Peru*,<sup>†@</sup>

speaking of the South and South-west, he affirms, that this wind yet in this / region is marvellous pleasing.

But though, as we were saying, many other Qualities of winds may be deduc'd from the Nature and Condition of the places, by which they pass: And though the heat also, which *Prosper Alpinus* (as we lately took notice) attributes to the Southerly winds, that blow in *Egypt*, may be probably ascrib'd to the heated Exhalations and vapours they bring from the Southern and parched Regions they blow over; yet whence the great coldness of Northern and Easterly winds should come, may be scrupled at by many of the modern Philosophers, who with divers *Cartesians* will not admit, that there are any Corpuscles of Cold.<sup>†\*</sup>

And possibly I could, about these matters, propose some other difficulties, not so easie to be resolved. But not being now to discuss the *Hypothesis* about Cold, I think it will be more proper in this place, instead of entring upon disputes and Speculations, to subjoyn an Experiment that I made, to give some light about this matter. /

Considering then that I had not met with any Trial of the Nature of that I am about to mention, and that such a Trial might possibly prove Luciferous, I caused a pretty large pair of ordinary Bellows to be kept a good while in the Room, where the Experiment was to be made, that it might receive the Temperature of the Air in that Chamber, then placing upon a board, one of those flat Bottom'd

Weather-glasses, that I elsewhere describe to contain a movable drop of pendulous water, by blowing at several times with intermissions upon the bubble or lower end of the Weather-glass, though the wind blown against my hand, were, as to sense, very manifestly cold, yet it did not cool the air included in the Bubble, but rather a little warm'd it, as appear'd by a small, but sensible, ascension of the pendulous drop each time, that, after some interpos'd rest, the lower part of the glass was blown upon, which seem'd to proceed from some small alteration towards warmth, that the air received by its stay (though short) in the Bellows, / as seem'd deducible from hence, that if by closely covering the Clack, the matter were so ordered, that the Air, that should come into the Bellows, must come in all at the nose; if this nose being held very near the bubble of the Weather-glass, the Air were, by opening the Bellows, suddenly drawn in, that stream of air or wind coming from a part of the window, where the air was a little cooler, then that which was wont to come out of the Bellows, would not, as the other, make the pendulous drop rise, but rather the contrary.

This done, we proceeded to shew by Experiment, That though a wind were nothing, but a stream of Air, yet in its passage it might acquire a considerable coldness distinct from that which it has by vertue of its motion, though upon the score of that, we see that air mov'd by a fan, (or as in our newly mentioned Trial) by a pair of Bellows, might to our touch, feel Cold, nor did we forbear to expect a good event of our Trial, upon the doubt that

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may be rais'd, whether there be frigorifick / Corpuscles or no: For whatever become of that question, I thought I might expect, that whether or no Ice emit Corpuscles, that are universally frigorifick, yet the air being, either by them, or upon what account soever, highly refrigerated, the Corpuscles that compose this cold Air, being most of them driven on before it, by the wind that meets them in its way, will, in a sense, prove frigorifick, in regard of a less cold body, which they shall happen to be blown upon, and accordingly, having provided a ridge Tyle inverted, and half fill'd the Cavity, which look'd upwards, with a mixture of ice and salt, and having likewise put the Iron pipe of the Bellows upon that mixture, and then covered it with more of the same, that so the Pipe being surrounded, as far as conveniently it could be, with ice and salt, the air contain'd in it, might thereby be highly refrigerated, I found, that blowing wind out of the Bellows upon my hand, that wind felt much more cold, then that which had been before blown upon my hand, / out of the same Bellows, before the frigefactive mixture was appli'd to it. But for fear my sense of feeling should deceive me, I caus'd a Weather-glass, made after the common manner, but with a more slender pipe, to be so plac'd, that the nose of the Bellows (which together with the Tyle and Ice, was upheld with a frame) lay in a level with the bubble of the Thermometer, and then blowing the refrigerated air of the Bellows upon the globular part of the glass, I saw the water in the Cylindrical part and shank, manifestly ascend, as it was wont to do upon the refrigeration of the included air: And as this Ascension of the liquor continued, during three or four blasts of the Bellows, so upon the cessation of the artificial wind, the water subsided by degrees again, till by fresh blasts it was made to ascend. Lastly, having repeated this Experiment, we thought fit to trye, how much the air, refrigerated immediately by the frigorifick mixture, would produce a colder wind then the former, and accordingly, drawing back the nose / of the Bellows, that the air, that should be blown out, might pass along the Cavity left in the frigorifick mixture by the Iron pipe (of the Bellows) which we had withdrawn, the wind was manifestly more cold, then before, and had a greater operation on the Weather-glass, it was blown upon.

This Experiment, if carried on, and prosecuted, may possibly prove more Luciferous; but I will not take upon me here to determine, whether all cold winds must be necessarily made so, by frigorifick Corpuscles properly so call'd, since I have sometimes suspected, that some winds may be cold, only by consisting of, or driving before them, those higher parts of the Air, that, by reason of the languid Reflection of the Sun beams, in that upper (or perhaps Arctick) region of the Air, are for the most part very cold. For it may be observ'd, that Rains oftentimes very much and suddenly refrigerate the lower Air, when no wind, but what the clouds and rain make, accompanies them, as if they /

brought down store of cold air with them from that upper Region; which *Acosta*, and one I conversed with, that visited far higher mountains, then the *Alps*, affirm to be in some places (for I am not satisfi'd, that 'tis so every where) exceedingly cold, both in hot Climates, and in hot

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seasons of the year. And I observe, that the *Hollanders* do, in more places then one or two, mention the Northerly and North-easterly winds, to be those, that brought them the prodigious colds they met with, though *Nova Zembla*, where they were expos'd to them, be so Northwards, that it lies within 16. or 17. degrees of the Pole it self.<sup>†\*</sup> This being a bare suspicion, it may suffice to have touch'd it. But I shall subjoyn two or three instances on the occasion of our proposition, concerning the influence of the winds upon the air, and to show more particularly, That even cold winds receive not always their Qualities, so much from the Quarter whence they blow, as from the Regions over which they blow: I shall therefore begin with what is / delivered by Mr. *Wood*, in his *New Englands* prospect.<sup>†@</sup> *Whereas in England (says he) most of the cold winds and weathers come from the Sea; and those situations, are counted most unwholsome, that are near the Sea-coast, in that Countrey it is not so, but otherwise. And having added, as his reason, that the North-east wind, coming from the Sea, produces warm weather, melting the snow, and thawing the ground; he subjoyns, only the North-west wind coming over the Land, is the cause of extreme cold weather, being always accompanied with deep snows, and bitter frosts, &c. To which passages we shall add only one out of Captain James, as being considerable to our present purpose. The winds (says he<sup>†@</sup>) since we came hither, have been very variable and unconstant; and till within this fortnight, the Southerly wind was coldest. The reason I conceive to be, for that it did blow from the main Land, which was all covered with snow, and for that the North winds came out of the great Bay, which hitherto was open.*

## Title XIX.

### *Of the strange Effects of Cold.*

1. To enumerate and prosecute all the several Effects of Cold, being the chief work of the whole Book, it is not to be expected, that they should be particularly treated of in this one Section of it, wherein I shall therefore confine my self to mention only those Effects of Cold, that are not familiar, but seem to have in them something of wonderful; nor must I take notice of All them neither, least I should be guilty of useless Repetitions, but only of them, which either are not at all, or are but incidentally or transiently delivered in the foregoing Sections. Nor is it to be expected, that I should pawn my credit for the truth of every one of the Relations I am about to subjoyn. For if they had not something / of extraordinary, and consequently, that may beget some Diffidence in wary men, they would not be proper for the title of this Section, and most of them, that they may be fit to be plac'd here, must be the

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Effects of such extreme degrees of Cold, that I cannot in this temperate Climate of ours, examine the truth of them by my own Trials, so that all I can do, is, to make choice of such Relations, as are almost all of them delivered by the Relators, as upon their own Knowledge. And even this may perchance, not only gratifie and excite the Curiosity of some, who are pleas'd with no things so much, as with those, that have somewhat in them of Prodigy, and (which is more considerable) their Narratives may afford the Ingenious such strange *Phænomena*, that the Explication of them may serve, both to exercise their wits, and try their *Hypothesis*.

2. It seems not necessary, in the marshalling these observations, to be scrupulous about method, but yet to avoid confusion, we shall first mention / the Effects of Cold, as to those four great Bodies, of that part of the Sublunary World we live in, that are commonly reputed Elements, and thence we



will proceed to take notice of the Effects of Cold upon some other inanimate Bodies, and, for an instance of its operation on living Creatures, upon men.

3. Of the power of Cold, either to straiten the sphere of activity of fire, or to hinder its wonted effects, the chief examples I have met with are recorded, partly by the *Dutch* in *Nova Zembla*, and partly by Captain *James*, when he winter'd in *Charleton Island*. These *Hollanders* in one place speak thus;†@ *The twentieth it was fair and still weather, the wind Easterly, then we wash'd our Sheets, but it was so cold, that when we had wash'd and wrung them, they presently froze so stiff, that although we laid them by a great fire, the side that laid next the fire thaw'd, but the other side was hard frozen, &c.* Elsewhere thus, *We were in great fear, that if the extremity of the Cold grew to be more and more, we should all dye there, with cold, for what fire soever we made, it would not / warm us.* And because it were tedious to transcribe all that their Journal afford us to our present purpose, we will conclude with this passage, *Hereby we were so fast shut up into the House, as if we had been prisoners, and it was so extreme Cold, that the fire almost cast no heat, for as we put our feet to the fire, we burnt our hose before we could feel the heat, so that we had work enough to do to patch our hose, and which is more, if we had not sooner smelt then felt them, we should have burnt them ere we had known it.* Though Captain *James* wintred in a Countrey many degrees remoter from the Pole, then *Nova Zembla*, yet in one place he gives us this account of the colds power to restrain or oppose the action of fire.†@ *The Cooks Tubs, wherein he did water his meat, standing about a yard from the fire, and which he did all day ply with melted snow water, yet in the night season, while he slept but one watch, would they be firm frozen to the very Bottom. And therefore was he fain to water his meat in a Brass Kettle, close adjoyning to the fire; and I have many times both seen and felt, by putting my hand into it; / that side which was next the fire was very warm, and the other an inch frozen. I leave the rest to our Cook, who will almost speak miracles of the Cold.*

3. Thus far our English Navigator, whose relation compar'd with those of the *Hollanders*, make me not so much wonder, as I once did, that men should

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relate to *Marcus Polus*, that there is a certain Plain in *Tartary*, situated between some of the highest mountains in the World, *where if fire be kindled, it is not so bright, nor so effectual to boil any thing, as in other places.*†@ For so *Purchase* renders that passage; whence occasion has been taken to impute to *Marcus Polus*, a writer not always half so fabulous, as many think him, that he affirm'd, that there was a Countrey in *Tartary*, where fire could not be kindled.

4. And as for the other newly mention'd relations of Seamen and Travellers, though to us, that live in *England*, they cannot but seem very strange; yet I am kept from rejecting them as utterly incredible, by considering, that ice and snow having before their Congelation been water, / must in probability owe their Coldness, to that which reign'd in the Air: So that if in any place Nature has, either so plentifully stock'd the Air it self with frigorifick exspirations, or other Corpuscles (if we will admit any such) or have upon any other account rendred it as cold as it can make ice and snow to be even here amongst us, I know not why the Northerness of the climate, and perhaps some saline exspirations from the Earth and Sea, may not there diffuse through the air a cold superior to that, which by small Quantities of ice (or snow) and salt, can at a small distance be produc'd here. And this cold is so intense, that by pouring some water on a Joynt-stool, and placing on it a silver Tankard, or other convenient vessel, we may, as experience has assur'd me, with beaten ice (or snow) and salt, and a little water (which is added to hasten the solution of the other) nimbly stirr'd together in the pot, make the mixture freez the external water quite through the Tankard; and they may be by this way so hard frozen together, / as that by lifting up the pot, you may lift up the Joynt-stool too, and that (which is the circumstance, for which I mention this) just by the fire, which in this case is unable to hinder so difficult an operation of the Cold.

5. Thus much of the effects of cold, in reference to fire. What the same quality may perform upon Air, we shall say but little of in this place, because we treat of those *Phænomena*, partly in the foregoing Section of the coldness of the Air, and partly in other places. Only we shall not here pretermitt a testimony of the learned *Olearius*, who, as an eye witness, confirms what we elsewhere deliver of the high degree of cold, to which the Air may be brought. For he tells us,†@ *That in Muscovy he experimentally found, that which others left recorded in their writings, That ones spittle would be congeal'd before it reach'd the ground, and that water would freez as it was dropping down.*

6. Of the effects of cold upon water, we shall not need to say much in this place, since the two notablest / of them being, the power cold has to congeal water suddenly, and the force it has to turn vast quantities of it into sollid ice. Of the former I have newly given, out of *Olearius*, an example as eminent as almost any that is to be met with, and of the latter also, I have given several

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instances in the Section, that treats of ice: Yet two or three notable instances, which we do not elsewhere mention, 'twill not be improper to deliver in this place.

7. The first declares, that notwithstanding the warmth of the inside of a mans mouth, his spittle may be frozen even there. *The 27. of September* (they are the words of *Gerat de Veer*†@) *it blew hard Northeast, and it froze so hard, that as we put a nail into our mouthes (as when men work Carpenters work they use to do) there would Ice hang thereon, when we took it out again, and make the blood follow.* The like relation (if I misremember not) I have met with in a modern English Navigator, and it is very little, if at all more strange, then what is affirm'd by Queen *Elizabeths* Ambassador to / the *Russian* Emperor: *In the extremity of winter* (says Doctor *Fletcher*, speaking of *Muscovia*) *if you hold a pewter Dish, or Pot in your hand, or any other mettall, except in some chamber, where their warm Stoves be, your fingers will stick fast to it, and draw off the skin at the parting.*†\*

8. The other instance I intended to mention, is this, that though *Macrobius*, and other learned men, both ancient and modern, will not allow salt water to be congealable; yet the *Dutch* at *Nova Zembla* relate, that even in the midst of *September* (and as the Marginal note says, in a night) *It froze two inches thick in the salt water.*†@

9. As to the effects of violent colds upon the Earth, what they would prove upon pure and Elementary Earth (if any such there be) I can but conjecture; but as for that impure or mingled Earth, which we commonly tread on, the effects of extreme cold upon that, may be very notable. For *Olearius* relates,†\* *that in the year 1634. the cold was so bitter at Musco, that in the great market-place, he saw the ground open'd by it so, that / there was made a cleft of many yards long, and a foot broad.* [And the present great Duke of *Muscovies* Physician being asked by me concerning the truth of such relations, answered me, that he himself had in those parts seen the ground reduc'd by the cold, to gape so wide, that a childs head might well have been put into the cleft.]†\*

10. 'Tis somewhat strange, that the violent heat of Summer, and the extreme cold of Winter should both of them be able to produce in the ground the like effects; but whether to make these gaping chinks, that we have been speaking of, the surface of the ground expos'd to the air, being first frozen, is afterwards broken by the expansive force of the moist earth underneath, to which the cold at length pierces, and congealing it, makes it swell, and heave, and so burst or cleave the hard and frozen crust of the ground, which cannot sufficiently yield to it, whether this (I say) may produce the clefts we were speaking of, or whether they must be deriv'd from some other

cause, not having yet made the experiments, / I thought upon, to clear the matter one way or other, I do not as yet pretend to determine, but will rather subjoyn the second observation I purpos'd to mention of a strange operation of Cold upon the ground, and it is afforded us by the *Dutch* in their often quoted third voyage to *Nova Zembla*: In one place of which they tell us,<sup>†@</sup> *That when they had built them a wooden house, and were going to shut themselves up in it, for the winter, they made a great fire, without the house, therewith the thaw the ground, that they might so lay it, viz. the wood about the house, that it might be the closer, but it was all lost labour, for the earth was so hard, and frozen so deep into the ground, that they could not thaw it, and it would have cost them too much wood, and therefore they were forced to leave off that labour.*

11. After what we have said about the strange effects of cold, in reference to fire, air, water, and earth, we will now proceed to take notice of its effects upon confessedly compounded Bodies, whether inanimate or living; but of the former / sort of mix'd Bodies (I mean those that have not Life) it will not be necessary to say much in this Section, in regard that we have in many other places, upon several occasions had opportunities to mention already most of the particulars that belong to that head. For we elsewhere take notice, that violent Colds would freez Beer, Ale, Vinegre, Oyl, common Wine, and even Sack and Alegant themselves. We have likewise noted, that the Cold may have a notable operation, upon Wood, Bricks, Stone, vessels of Glass, Earth, and even Pewter, and Iron themselves, to which *Bartholinus* out of *Janus Muncks Voyage to Greenland*,<sup>†\*</sup> allows us to add vessels of Brass (though these are not immediately broken by the Cold, but by the included Liquors which it dilates) and divers strange effects of Cold upon inanimate Bodies, which 'twere here troublesome to recapitulate, may be met with dispers'd in several places of the present History. Wherefore having only intimated in general, that, though many plants are preserv'd / by a moderate cold, yet it has been observ'd, that most Garden-plants are destroy'd by excessive degrees of it, we will pass on to consider the effects of Cold upon animals, and of the many observations, that we have met with among Travellers, concerning this subject, we shall, to avoid prolixity, deliver only the considerablest, and those that we find attested by very credible Writers.

12. Captain *James* speaking of the last of the three differences he makes of Cold (namely, that which he and his company felt in the woods) gives this account of it;<sup>†@</sup> *As for the last, it would be so extreme, that it was not endurable; no clothes were proof against it, no motion could resist it. It would moreover so freez the hair of our Eye-lids, that we could not see; and I verily believe, that it would have stifled a man in a very few hours.*

13. *Olearius* giving an account of the Air of *Muscovy*, and especially the

Capital City of it,<sup>†@</sup> *The Cold* (says he) *is there so violent, that no Furs can hinder it, but sometimes mens Noses, and Ears, Feet and Hands will / be frozen, and all fall off.* He adds, that in the year 1634. when he was there, *they could not go 50. paces without being benumm'd with cold, and in danger of losing some of their Limbs.* And yet to add, that remarkable observation upon the by, the same Author, near the same place, speaking of *Musco*, and the neighbouring Provinces distinguished from the rest of that vast Empire, says,<sup>†@</sup> *That the Air is good and healthy, so that there one scarce ever hears of the Plague, or any other Epidemical diseases.* And he adds, that for that reason, when in the year 1654. *the Plague made havock in that great City, the thing was very surprizing, nothing like it having been seen there in the memory of men.*

14. Our already divers times mention'd English Ambassador Dr. *Fletcher*, speaking of the cold that sometimes happens in *Russia*, witnesseth thus much of it. *Divers* (says he<sup>†@</sup>) *not only that travel abroad, but in the very markets and streets of their Towns are mortally pinch'd, and kill'd with all;*



so that you shall see many drop down in the streets, many Travellers brought into the / Towns sitting dead, and stiff in their sleds. Divers lose their Noses, the Tips of their Ears, and the Balls of their Cheeks, their Toes, Feet, &c. Many times when the winter is very hard and extreme, the Bears and Wolves issue by troops out of the woods, driven by hunger, and enter the Villages, tearing and ravening all they can find, so that the inhabitants are fain to flee for the safeguard of their Lives.

15. To descend now to observations, that do somewhat more punctually set forth the more particular *Phænomena* of Cold, in reference to mens Bodies, take the following Observation.<sup>†\*</sup> *The 15. of March some of their men, that had been abroad to kill Deer, returned so disabled with cold, which did rise up in blisters under the soles of their feet, and upon their legs, to the bigness of Walnuts, that they could not recover their former estate (which was not very well) in a fortnight after. This may be confirmed by that passage of the Hollanders, where speaking of their preparing springes to take Foxes, they add,<sup>†@</sup> that they did it with no small trouble; for that if they stay'd long without doors, / there arose Blisters upon their Faces and Ears. We did dayly find by experience (says Captain James<sup>†@</sup>) that the cold in the woods would freez our faces, or any part of our flesh, that was bare; but it was not so mortifying, &c.*

16. The *Dutch* speaking of the pains they were fain to take to dig away the snow, that cover'd the house, and choaked up their door, adds, that in that laborious work, *they were forc'd to use great speed, for they could not long endure without the house, because of the extreme cold, although they wore Foxes skins about their heads, and double apparel upon their backs.*<sup>†@\*</sup>

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17. The lately mention'd Captain *James* relates, that in *Charleton Island* he was fain to cut the hair of his head short, and shave away all the hair of his face, because the Isicles, that would be fastned to it, made it, as he speaks, *become intolerable.*<sup>†@</sup>

18. And he elsewhere relates, that once he and his Companions, having been for a little while parted into two companies, *had their faces, hair and clothes so frozen over, that they could not / know each other by their habits, nor* (which is a considerable circumstance, for whose sake chiefly I mention this passage) *by their voices.*<sup>†@</sup>

19. And the same Author gives this account of the death of the Gunner of his Ship, whom he calls a strong hearted Man, and who died before the end of *November*. *He had* (says our Author) *a close boarded Cabbin in the Gun-room, which was very close indeed, and as many clothes on him as was convenient, (for we wanted no clothes) and a pan with coals of fire continually in his Cabbin, for all which warmth his plaister would freez at his wound, and his bottle of Sack at his head.*

20. *The 11. of December* (says *Gerard de Veer*<sup>†@</sup>) *it was fair weather, and a clear Air, but very cold, which he that felt not would not believe, for our shooes froze as hard as horns upon our feet, and within they were white, so that we could not wear our shooes, but were forc'd to make great pattents, the upper part being sheep skins, which we put on over three or four pair of socks, and so went in them to keep our feet warm, yea, and the clothes upon our backs were white over with frost.* /

21. Which may be somewhat confirmed by this passage of Captain *James*.<sup>†@</sup> *The clothes on our Beds would be covered with hoar frost, which in this little habitacle was not far from the fire. We might adde to all these, this other passage of the often mentioned Gerard de Veer.*<sup>†@</sup> *The 26. of December, it was foul weather, the wind North-west, and it was so cold, that we could not warm us, although we used all the means we could with great fires, good store of clothes, and with hot stones and Billets laid upon our feet, and upon our Bodies, as we lay in our Cabbins, but notwithstanding all this, in the morning our Cabbins were frozen, &c.* But we shall not insist on such passages, as



this last recited, because that of the force of cold to repress and withstand the fire, we have already deliver'd as remarkable things, as will be easily met with, in approved Writers, in the former part of this present Section.

22. I have my self met with a knowing and very credible person, that related to me of the cold of *Russia*, where he travelled, little less strange things, then those I have mentioned / of it out of Books; and if I did not want the Historians name, I should make small difficulty to add, That since I made a good progress in this present Section, a very learned Traveller (though not into cold Countries) related to me, upon the occasion of what I was treating, what he affirm'd to have met with in an approv'd History of the

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strange operation of the inclemency of the Air upon multitudes of men at once, namely, that about the year (if he rightly remember it) 1498. an Army of the Turks making an incursion into *Poland*, upon their return was surprized with such an extremity of Cold and of Snow, that though it were but (if he mistake not) in *November*, forty thousand of them (the whole Army consisting of seventy thousand) perish'd through the extremity upon the place.<sup>†\*</sup>

23. Amongst the many Relations I have met with of the fatal Effects of Cold in the Northern Countries, I took notice not without a little wonder, as well as trouble, that I could not find, that any of the Relators / had the curiosity to see what change was made in the internal parts of the Bodies so destroy'd, which yet were an inquiry very proper to have been made, but at length the other day an ingenious Person having shew'd me a Book newly publish'd in French, containing the Description of a *Polonian* Province he calls *Ukraine*, as I was skimming it over, with hope to find some observations about Cold, I lighted on a relation, which though not such as I desir'd, is more then I have any where else found, and I take the more notice of it, because, that though the very name of this Province is scarce hitherto known to us in *England*, yet having a while after by good chance met with an intelligent *Polonian* Lord, and having inquired of him, whether he had ever been in that Country, he both told me, that he had been quarter'd there, and by his Answers and Relations did countenance divers particularities of it, mention'd by this French officer (named *Monsieur de Beauplan*) who liv'd long there.<sup>†\*</sup> This Author then / after having taken notice, that this fertile Province, though but situated in the same height of the Pole with *Normandy*, is oftentimes subject to excessive colds (which circumstance I mention as a further confirmation of something of the same nature delivered in the former Section) gives an Account of two differing Effects of this Cold upon the Bodies of men: The one being a peculiar kind of sickness, the other Death.

24. The first which I remember not to have elsewhere met with, is, that sometimes when the natural heat proves strong enough to protect the Toes, and Cheeks, and Ears, and other parts, that are either more remote from the heart, or more tender from a sudden mortification; yet unless nature be assisted, either by good Precautions, or Remedies, she cannot hinder the cold from producing in these parts Cancers, as painful as those which are caus'd by a scalding and malignant humour, and which let me see (says my Author) when I was in those Countreys, that cold was not less cutting nor powerful / to destroy things, then the fire to consume them: He adds, that the beginning of these Cancerous sores is so small, that what produces the pain scarce equals the bigness of a Pea, and yet in few days, nay sometimes in few hours

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it spreads so, as to destroy the whole part it invades, which he confirms by the Example of two persons of his acquaintance, who in a trice lost by Congelation the badges of their Sex.

25. As to those that are kill'd with Cold, our Author informs us, that they perish'd by two differing kinds of death. For some being not sufficiently fortifi'd against the cold by their own internal heat,

nor competently arm'd against it by Furs, Inunctions, and other external means, after having had their hands and feet first seized by the cold, till they grow past feeling it, there the rest of their Bodies are so invaded, that they are taken with a (kind of Lethargick) Drowziness, that gives them extreme Propensity to sleep, which if indulg'd to, they can no more awake out of, but dye insensibly. And / from this kind of Death our Author adds, that he was several times snatch'd by his servants, who were more accustom'd to the cold, and seasonably forc'd him to awake out of those drowzinesses, which they knew to be most dangerous. And that sometimes the death by cold is indolent enough, the Relations of some intelligent acquaintances of mind, who have been in exceeding cold Countries, do confirm.

26. But the other way whereby cold destroys men, is that, which is the most remarkable in our Author, and though less sudden is more cruel. For he tells us, that sometimes the cold seizes mens Bodies in the reins, and all about the Wast (and especially horse-men underneath the Armor of the Back and Breast) and straitens, as he speaks, those parts so forcibly, that it freezes all the parts of the Belly, especially the Guts, so that though they have keen appetites, they cannot digest, or so much as retain the lightest and easiest Aliments, without excepting Broths themselves, but presently reject them by vomit, / with unspeakable gripings and pains, and so continually complaining of their condition, and sometimes crying out, as if some body were tearing out their bowels, they end their miserable lives, being often brought by the violence of their torments to the brink of madness and despair, before they come to that of the grave. And our Author having seen some of these departed wretches open'd, says, that they found the greatest part of their guts black, burn'd up, and as it were glew'd together, whence he thinks it probable, that, as their bowels came to be spoil'd and gangrenated, they were forc'd to those complaints and exclamations; and we may add, That probably upon the same cause depended those continual vomits of what they eat or drunk; the Gangrene of the guts hindering the descent of Excrements downwards, as it often falls out, in the true *Iliaca Passio*, and the Peristaltick, or the usual motion of the parts being inverted, as it also frequently happens in the same disease. There is no doubt but Anatomists and Physicians will / think this account very imperfect, but yet I think my self beholden to the Author for it, because 'tis not the best, but the only, that I have hitherto yet met with of this matter, though I could wish it had been much more full and particular, and that he had also open'd those Animals, and especially their brains, that he mentions to have been kill'd suddenly, and without pain, by cold. For such informations (whose want, is far as our Climate will permit, I have had

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thoughts of supplying by Experiments upon other animals) would perhaps satisfie me one way or other about a conjecture I have had, and been able to countenance by several trials upon Vegetables and dead Animals, about the cause of mortifications produc'd by excessive cold.

27. What effects a violent Cold may have upon the bodies of other animals then men, I scarce find at all taken notice of by the Writers I have met with, and what I remember upon that subject amounts to but few particulars: The French Author lately quoted, takes notice in general, / that the cold in *Ukrain*, as the *Polanders* call it, is sometimes so great, as to be scarce supportable by horses, and some other tame beasts.

28. This same Author also mentions a certain fourfooted Animal called *Bohack*, which is said to be peculiar to those parts, and hides himself under ground in the Winter; and having inquir'd of the lately mention'd *Polish* Nobleman concerning this beast, he told me, that being in that Province he had one presented him as a rarity, upon an occasion proper enough to be mention'd here: For some of the *Poles* chancing to dig (for some purpose that I remember not) in a certain retir'd place, were surpriz'd to find under ground, an Animal not familiar to them, and though this creature was so frozen and stiff, that they thought it to be stark dead, yet when they came to flea it for its skin, being awaken'd by pain, it recovered life again, and was brought as a rarity to the Commander, from whom I have the relation.

29. That some other animals may / be frozen till they are stiff, and yet recover, I shall (ere long) have occasion to observe at the close of the 21. Section.<sup>†\*</sup> And therefore I shall now add but this, That whereas 'tis a Tradition among Travellers into Northern Climates, that both Birds and wild Beasts are in icy and snowy Countries ordinarily turn'd white, if not at all times, yet at least in the Winter by the coldness of those gelid Climates, I dare neither admit the position as a thing that is true universally, nor reject it as a thing that is never so. For not now to enquire, whether whiteness proceeds from the coldness of the Countrey, or from some settled seminary impression, or from the imagination of the females affected by the vivid whiteness of the snow, that almost all the year long is the constant object of their sight: I find by the Voyages I have perus'd, that Navigators often mention their meeting with store of white Bears and Foxes in *Nova Zembla*, and other very Northern Regions, as also their meeting sometimes with herds of white Deer: And in the *Alps*, always / covered with snow, good Authors mention their having met with white Partridges; to which purpose I remember, that when I was in *Savoy*, and the neighbouring Countries, which have mountains almost perpetually cap'd with snow, I heard them often talk of a certain white kind of Pheasants to be met with in the upper parts of the mountains, which for the excellency of their taste were accounted very great delicacies.<sup>†\*</sup> But on the other side, the same Navigators treating even of the coldest Climates, seem to

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distinguish the white Bears from others of those parts.<sup>†@</sup> And as for a herd of white Deer, their colour may proceed from seminal impressions, since here in *England* I have seen several Deer of that colour, and though *Greenland* be by some degrees nearer to the Pole than *Nova Zembla*, yet I have seen a live Deer brought thence somewhat differing in shap'd from ours, whose skin was not white, but rather a kind of dun: And to add / That upon the by, I took notice, that provident Nature to arm them against the cold, had afforded him a Coat, that might have pass'd for a Fur.

30. Yet these two things seem remarkable in favour of the efficacy of cold, the one, that in several cold Countries, as particularly *Greenland*, and *Livonia*, even Modern describers of them affirm, that Hares will grow white in Winter, and return to their native colour in Summer.<sup>†@</sup> And the other, that though *Charleton Island* differ not one degree in Latitude from *London*; yet (as the cold is there prodigious, so) I remember, that Captain *James* some where takes notice of his having seen there, both divers Foxes, that were pied black and white, and white Partridges, though he could not catch them.<sup>†@</sup> But of the whiteness of Animals I elsewhere treat among other subjects, that belong to the History of Colours. And having already been more prolix than I intended / in setting down the observations of others, I think it now time for me to resume the mention of my own Experiments, divers of which, though made before others, that have been already mention'd, X or XII. Sections of, I thought fit for to reserve for this place, both for other reasons, and because, this place seems proper for Experiments, that have a nearer tendency to the hinting or the examining the more general *Hypothesis* about Cold. /

## Title XX.

### *Experiments touching the weight of Bodies frozen and unfrozen.*

1. Since divers of those ingenious men, that have of late revived, and embraced the Doctrine of the old *Atomists*, teach us, that water is turned into ice by the introduction of frigorifick Corpuscles, which *Democritus* of old is said to have believed to be cubical (and to which other Philosophers of late have assigned other shapes indeed, but yet determinate ones<sup>†\*</sup>) we thought fit

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not so much for our own satisfaction, as for that of others, to try, whether or no a Liquor by its increase of weight, when frozen, would betray any substantial accession of the Corpuscles of Cold,

which according to the *Epicurean* / Principles, may, by reason of their smallness, pass in freely, and in vast multitudes, at the pores of other Bodies, and even of glass, and which by reason of the same smallness, must be supposed exceedingly numerous to be able to arrest the motions of such multitudes of minute Corpuscles, as must go to the making up of any considerable quantity of water.

2. And first we made a trial with Eggs, of which our Notes give us the following account.

3. [We took a good pair of Scales and placing them upon a frame (purposely made for such Experiments, as required, that the things to be weighed should remain long in the balance) we put into one of these a couple of Eggs, and having counterpoised them with brass weights, we suffered them to continue all night in a Turret (built as it had been made for an observatory) that the breaking of the Eggs, or any such other accidents might not hinder the success of our endeavours (which were to try, whether the Corpuscles of Cold, / which divers Philosophers suppose to be the Efficients of Congelation, would make them any whit heavier,) but we were somewhat surprized, when the next morning, after a very sharp night, going up to the Turret, we found (the scales and frame being in good plight) the Eggs to be grown lighter by very near four grains.]<sup>†\*</sup>

Thus far the Note.

4. But though we afterwards repeated the Experiment once or twice (if not oftner) yet having been by intervening avocations diverted from registering the circumstances of the events; I dare not now trust my memory for any more, then that some of the circumstances seemed odd enough, but uncertain, and that I desisted from prosecuting the Experiment, chiefly for this reason, that an increase of weight in exposed Eggs was scarcely to be hoped for, because it seemed probable, that part of the more subtile and spirituous Corpuscles contained in the Egg do continually, by little and little, get away through the pores of the skin and shell; that, seeming to be the reason / why Eggs long kept have usually within the shell, a manifest, and sometimes very considerable cavity unfilled with either yelk or white, which Cavity seems to have been left by the recess of the subtile parts we have been mentioning, so that although the frigorifick Atoms should by their ingress add some, not altogether insensible weight to the Egg, yet that would not, unless perhaps in the very nick of time, when the Congelation is first actually made, be taken notice of, by reason of the greater decrement of weight, that proceeds from the Avolation of the more subtile parts of the Egg it self.

5. And to satisfie our selves about this matter, we took four hen Eggs, and counterpoised them carefully in a good pair of Scales, which were suspended at a frame, that the balance might be kept unstirr'd in a quiet room, wherein

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we had placed it, and suffering it to continue there for a pretty while, we observ'd, that though it were Winter, and though the room wherein it stood were destitute of a Chimney, yet that Scale / wherein the Eggs lay, did almost daily grow manifestly lighter, so that it was requisite, from time to time, to take a grain out of the opposite scale, to reduce the ballance to an *Equilibrium*. And by this means we found the Eggs after some time to have lost eight grains of their former weight, but how much more they would have lost, if we had continued the Experiment, the need we had of the Scales kept us from discovering.

6. Upon this occasion I will add, that I us'd some endeavours to satisfie my self about this inquiry, viz. whether Eggs being once actually frozen (for those mention'd in the former Note, might lose their weight before they were so) and kept in a pair of good Scales fasten'd to a frame in some quiet place, well fenc'd from the Sun, would by the cold of the Air in freezing weather, be kept for any considerable time, without a sensible diminution of weight, but an unexpected thaw hindered us from seeing the success of what we design'd of this nature, both as to Eggs, and / also some other



Bodies: For if the Experiment were very carefully tri'd upon a competent variety of them, it might possibly assist us to guess, especially in Camphire, and some other easily exhalible bodies, what interest Cold may have in suppressing or diminishing the expiration of their *Effluvia*.

7. But to return to the weight of Bodies frozen and unfrozen, we attempted to discover somewhat about it by several ways, according as the differing accommodations, we were furnish'd with, permitted. And of these trials I will mention four or five, as well of the less, as of the more accurate, as my memory or Notes supply me with them.

8. One of the less Accurate ways we imployed to try, whether ice, in which according to Atomists, great store of these frigorifick Corpuscles must be wedged, would not upon their expulsion or recess, leave the water lighter then was the ice, was that which follows, wherein to hasten the Experiment, we mingled a little salt. And though we foresaw, / there would be a difficulty from the Adhæsion of the vapors of the external Air, to the outside of the glass we were to employ, we thought, that inconvenience might be remedied by well wiping off the frost, or dew from the outside of the glass, till it were clean and dry: The event of the trial we find succinctly set down among our Notes as follows. [A single vial sealed up with ice and salt, being wiped dry, and weighed, was found to weigh four ounces four drachms and a half, when it was quite thawed, it was found to weigh somewhat more then a grain less then its former counterpoise.]<sup>†\*</sup>

But more accurate and satisfactory Trials about this matter, I find thus set down in one of my papers.

9. [We took a vial more thin then those that are commonly us'd, that, of the Aggregate of that and the Liquor, the glass might make so much the lesser

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part: This vial was furnished with a somewhat long neck, which at the flame of a Lamp was drawn by degrees slenderer and slenderer, that being very narrow at the / Top, it might the more readily and conveniently be seal'd, notwithstanding the waters being in it; then we *almost* fill'd it with that Liquor, I say almost, because a competent space ought to be left unfill'd, to allow the water, swell'd by glaciation, room to expand it self: This vial with the liquor in it, was plac'd in a mixture of snow and salt after our usual manner, and when the glass appear'd almost full of ice, it was taken out, and nimbly clos'd with *Hermes's* seal,<sup>†\*</sup> presently after this was weigh'd in a pair of very good Scales, and the vial together with the contain'd liquor, amounted to  $\text{℥v. 38. gr. } \beta$  [5 ounces, 8 drachms,  $\frac{1}{2}$  grain,] which yet was not all ice, because these things could not be done so nimbly, but that some of the ice began to thaw, before we were able to dispatch them quite, the vial thus seal'd being remov'd, and suffered for two or three hours to thaw, when the ice was vanish'd, we weigh'd again the seal'd glass in the same Scales, and found, that it weigh'd, as before, at least, if there were any difference, it seem'd to weigh a little more.] But / this Increment that amounted not quite to  $\frac{1}{2}$  a grain, might easily be attributed to some difference in the weights and grains themselves, wherein 'tis not easie to find a perfect exactness, or to some little unheeded moisture, that might adhere to some part of the vial.

10. And because it may be wished, that as this Experiment shews the weight of Ice resolv'd into water, to be the same with that of the solid ice, so we had tri'd, whether the weight of water congeal'd into ice, would be the same with that of the former fluid water, we will subjoyn what immediately follows in the same paper in these words.

11. [We took a seal'd vial, very thin, that it might be lighter, but not so large as the other, by about a third, as amounting in the lately mention'd Scales but to  $\text{℥iiij. 3ij. gr. 41.}$  [4 ounces, 2 drachms, 41 grains] when we had seal'd it up with the water in it. This vial we plac'd as we had done the other, in a mixture of snow and salt, freezing it warily, lest being seal'd, it should break, then we remov'd it

into the same / Scales, to try, whether it had got any weight by the suppos'd subingression of the Atoms of Cold, which many learned men take to be the efficients of Congelation; but it either weighed just as before, or if there were any difference, it seem'd to have lost  $\frac{1}{4}$  of a grain. But suffer'd to thaw, and put into the same Scales again, it weigh'd just as much as it did, when frozen, though the weights were numerically the same, and about  $\frac{1}{8}$  would sway the Scales, or at least be sensible upon them. But note, that I was careful this last time to wipe the outside of the glass with a linen cloth, because I had observ'd, according to what I elsewhere deliver, that, in case ice be any thing hastily thaw'd, it may produce a dew on the outside of the glass, as I suspected, that even the warm Air might in some measure do in this, and if it had not been for this suspition, some adhering dew,

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that I was thereby enabled to detect and wipe off, before I put the vial into the Scales, might easily have impos'd upon us.<sup>†\*</sup>

12. These Trials I presume may / give some satisfaction about the inquiry, for the resolving whereof, I thought fit to make them.

13. But I was also desirous to see, whether any difference, as to weight, would be produc'd by freezing and thawing (if I may use those expressions in this case) Iron, Stone, Wood, or the like solid and permanent Bodies, which I intended to have exactly weigh'd, before and after their being expos'd to the Air, and also after the frost was gone, (and all this against Counterpoizes not expos'd to so great a Cold) would discover any sensible alteration, as to weight, that might safely be ascrib'd to the Cold. And though Avocations, and the negligence of one that we imploy'd, kept us from bringing the matter to such an issue as was desired, yet the Trials seem'd not altogether irrational, since we have formerly made it probable (and have since met with fresh instances to confirm it) that even Stones, and Metals, may resent some change of Texture by the operation of some degrees of Cold. And indeed induc'd by such considerations / of that kind, as seem'd the least doubtful, I remember I sometime made several experiments of the weight of some metals, and stones, both before and after they had been much expos'd to a more vehement Cold, then would have suffic'd to turn water into ice, and also after they had been, if I may so speak, thaw'd in a warm Air. But the paper in which we registred the events of these trials having been mislay'd, I dare not charge my memory with the particulars. Only, if I mistake not, one or two of the stones seem'd to have increased in weight, after having been buried in our frigorifick mixture, which I was apt to impute to some particles of the ice resolv'd into water by the salt, that was mingled with it, and (being perhaps made more piercing by the saline particles associated with them) imbib'd into the pores of the stone. For I remember, that having procur'd an Experiment, that I then wanted conveniency to try my self, to be made by an ingenious person, upon a stone hard enough to bear a good polish, / I was by him inform'd, that the stone by having been kept a while in water, did, though it were afterwards wip't dry, discover a manifest increase of weight: and in confirmation of my conjecture, I shall add, that from a sort of stones, that are of a texture close enough to be usually polisht; I did, as I expected, obtain by distillation (and that without a naked fire) a considerable quantity of an almost insipid liquor, which I suspected to be in good part but water soaked into the stone, for reasons, that 'tis not worth while here to discourse of; the cause of my mentioning these particulars being, that (I hope) they may make those, that shall hereafter try such Experiments, cautious how they draw inferences from them, and may invite them to expose the bodies, they would make trial of, rather to the cold of the free Air in very sharp weather, (for want of which, we our selves could not do what we advise) then to artificial glaciations at least, unless they be so ordered, that nothing that's moist come to touch the bodies to be wrought upon. /

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14. But such Trials as these newly mention'd, and others of the like kind, we must leave to be

prosecuted by those, that are furnish'd with accurate Scales, and leisure; for want of the latter of which, and sometimes too of the former, we were fain to give over the pursuit of them, which troubled us the less, because those made with the seal'd Vials were diligently made; and as for divers others, we made them, as we were saying, more to be able to gratifie others, then to satisfy our selves, because though in case there should unquestionably appear some sensible increase or decrement of weight, upon that which the Atomists would call *the Accession or Expiration* of frigorifick Corpuscles; it would afford a plausible Argument in favour of the *Epicurean* Doctrine, about the generation of ice; yet if no such change of weight should be found upon the freezing or the thawing of water, or any other Body, I doubt whether it may, on the contrary, be safely concluded, that the Atomists *Theory* of Cold is false. For / possibly they may pretend, that the Atoms of Cold may not have either gravity or levity, any more then the steams of Electrical Bodies, or the *Effluvia* of the Loadstone. Nay, though we should admit the frigorifick Corpuscles not to be altogether devoid of gravity, it may yet be said, that when they invade the Body, they freez, they expel thence some other preexistent Atomes, that may also have some little weight, and that the frigorifick Corpuscles, that flie, or are driven away, may be succeeded by some such, when bodies come to be thaw'd. But of this no more at present.

### *Appendix to the XX. Title.*

The Experiments recorded in the foregoing Section, may perchance in this regard prove more useful then I was aware of, that they may keep men from being misled by the contrary accounts, that I find to / have been given of the weight of ice, and water, by no obscure writers. For (to spare one of the famousest of the Ancients) *Helmont* in the Treatise he calls *Gas Aquæ*, where he gives an account of the congelation of water, which I confess to be unintelligible enough to me, and where he is pleased to ascribe to I know not what extenuation of part of the sulphur he supposes to be in water, that levity of ice, which the bubbles, it contains, afford us an intelligible and ready account of, delivers very positively this Experiment.<sup>†@</sup> *Imple* (says he) *lagenam vitream & magnam frustis Glaciei, collum verò claudatur sigillo Hermetis, id est, per vitri ibidem liquationem: ponatur hæc tum lagena in balance adjecto pondere in oppositum, & videbis quod propemodum octava sui parte aqua post resolutam glaciem erit ponderosior seipsa glacie. Quod cum millesies ex eadem aqua fieri possit, &c.* Thus far *Helmont*, who in case he take *lagena vitrea* in the ordinary acception of the word, would have made us some amends for this erroneous account, if he

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had taught us the way how he could seal such a / broad vessel, as a glass flagon, *Hermetically*. But what has been deliver'd in the foregoing Section, will sufficiently shew, what is to be thought of this Experiment of *Helmonts*. And for further confirmation, we have several times weigh'd ice frozen, and reduc'd to water, without finding any cause to doubt, but that *Helmont* was mistaken. And particularly upon the last Trial I made of this kind, having fill'd a wide mouth'd glass with solid fragments of ice, together with it amounting to a pound (of which the glass alone weigh'd somewhat above five ounces) I whelm'd over the mouth of it another flat bottom glass, that if any vapours should ascend, they might be condens'd into drops, as in the like case I had formerly observ'd them to do. And this ice being thaw'd in a warm room, as no drops were seen to stick to the inside of the inverted glass, so the other glass being again put into the same Scales, appear'd almost exactly the same weight as formerly, whereas the ice alone, that had been resolv'd, amounting / to much above eight ounces, according to *Helmonts* proportion, the weights should have been augmented by a whole ounce at least: And I make little doubt, but that if the Experiment had been tri'd in greater quantities of ice, the event would have been very little, if at all, different. But I purposely chose in the *Statical* Experiments about cold, to make my Trials in no greater quantities of matter then I have done, because 'tis very difficult to get scales strong enough to weigh, without being injur'd, much greater weights, and yet be accurate enough to discover truly such small differences, as are fit to be taken notice of in such Experiments. But to return to *Helmont*, notwithstanding all that we have said



against what he delivers about the weight of ice, yet because I take this inquisitive *Chymist* to have been, in spite of all his extravagancies, a Benefactor to experimental learning, I am willing to suggest on his behalf, that possibly much of the additional weight he ascribes to the resolv'd ice, may have proceeded / from that which would not have been taken notice of by an ordinary Experimenter. For (as I not long since intimated) I have (sometimes purposely, and sometimes by chance) by thawing ice in clos'd vessels somewhat hastily, produc'd a copious dew on the outside of the vessels, which dew, as being made by the condens'd vapours of the ambient Air, ought to be wip'd off, before the vessel be put into the scales to weigh the melted ice: And 'tis possible also, that *Helmont* may have err'd in the manner of weighing his *Lagena*, whatever he mean by it, it being usual even for learned men, that are not vers'd in *Statick's*, to mistake in Experiments, which require, that things be skilfully and nicely weigh'd: How far this excuse may be appli'd to a late Commentator upon *Aristotles* Meteors,<sup>†@</sup> who says, he tri'd, that water frozen is heavier then unfrozen, being a stranger to that Authors writings, I shall not consider: only whereas *Helmont* / and He

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seem to agree very little in their Affirmations, it will be perhaps more difficult to accord them, then to determine, by the help of our formerly register'd Experiments, what may be thought of both their Relations.

Yet I shall add on this occasion, That if I had not devis'd the above mention'd way of freezing water by Art in *Hermetically* seal'd glasses, I should have found it difficult to reduce, what is affirm'd by *Manelphus*, which I then dreamt not of, to an accurate Experiment; for though I had imploy'd a seal'd glass, (which I have not heard, that he or any other has yet made use of to that purpose) yet if I had in that vessel expos'd the water to be frozen the common way, 'tis odds (though it be not absolutely certain) that the water beginning, as 'tis wont to congeal at the Top, the Expansion of the subsequently freezing water would break the glass, and so spoil the Experiment: And for the same reason I have sometimes in vain attempted, to examine the weight of water frozen, by nature, according / to her wonted method in open vials. And if instead of glasses, you make use of strong earthen vessels, there is danger, that something may be imbib'd, or adhere to the porous vessel, and increase the weight, and by some such way, or by some mistake in weighing, 'tis very probable *Manelphus* may have been deceiv'd, which I am the more inclin'd to think, if we suppose him a sincere writer, not only because of some things I have taken notice of about congelations made in earthen vessels, but because, when I have instead of an earthen, made use of a metalline pottinger (both which sorts of vessels have in common this inconvenience, that their ponderousness makes them less fit for accurate Scales) there appear'd cause to suspect, either that our Author did not use metalline vessels, or, which I rather suspect, that he wanted skill or diligence in weighing. For as I find no intimation of his having imploy'd any peculiar or artificial sort of vessels, so, if he us'd such as we have newly been speaking of, and had weigh'd them carefully, I / cannot but think, that instead of finding the ice heavier then the water 'twas made of, he would have rather found it lighter. For I remember, that having once expos'd all night a pottinger almost full of common water, to an exceeding sharp Air, and having caus'd it the next morning to be brought me, when the liquor was throughly frozen, I found it to have lost about 50. grains (if I misremember not) of its former weight, and though this event were consonant enough to my conjectures, yet for greater certainty I repeated the Experiments another frosty night with this new caution; that the pottinger and water, together with the counterpoise, were kept suspended in the Scales, to be sure that no effusion of any part of the water in carrying it abroad to the open Air, should be made without being taken notice of; but the next morning (somewhat late) the vessel with the contain'd water now congeal'd, appear'd to have lost about 60. grains: and with the like success the Trial was reiterated once more, and that in weather / so sharp, that I am not apt to think, the water expos'd by *Manelphus*, began to freez sooner then ours. But the event was not unexpected, for besides that I consider'd, that in these kind of Experiments, part of the water, notwithstanding the exceeding coldness of the Air, must in all likelihood fly



away before the surface of it began to be congeal'd, I judge it not improbable, that not only the fluid part, but even that, which was already congeal'd, might continually lose some of its Corpuscles, and by their recess lose also somewhat of its weight. And least these conjectures should seem too unlikely, 'twill not be amiss to add in favour of the first of them, that having purposely provided a large Pewter Box, with a cover to screw on it, and having fill'd it almost full of water, (I say almost, because if the vessel had been quite full, the congealing cold might have burst it) and carefully weigh'd the Aggregate of both (which amounted to [!ounce!]v. [!drachm!]ij. gr. 11 [5 ounces, 2 drachms, 11 grains] whereof the vessel weigh'd 3ij. 3vß. and gr. 8.) [2 ounces, 5½ drachms, 8 grains] we expos'd the water after the Top / of the pot was screw'd on, to hinder the Avolation of it, to the freezing Air all night, and the next morning found it frozen from the top to the bottom, though not uniformly and perfectly, but found not one grain difference betwixt its present and its former weight: And as for the second conjecture newly propos'd, though it may seem somewhat strange, yet it is confirmable by this Experiment; that having plac'd divers lumps of solid ice in a Pottinger, which together with them weigh'd a pound, consisting of 16[!ounce!] [16 ounces], and having exposed these things in the same scales, wherein they were weigh'd, to the free Air on a very frosty night, we found the ice to have lost the next morning 24. grains of its weight, and the weather continuing so cold, that it froze hard all day long in the shade, I gave order to have it kept out of the Sun in the same scales, during all that time, and a good part of the following night, and then weighing it the second time, found, that the whole decrement of weight, did now amount to five grains above two / drachms, though the weight of the ice without the pottinger were but about seven ounces; and when we had kept about 13. ounces of ice in a very frosty night expos'd to the cold Air, it had lost as early as the next morning a good deal above two drachms of its former weight: But these *Statical* observations have perhaps already but too much swell'd this *Appendix*. /

## Title XXI.

### *Promiscuous Experiments and Observations concerning Cold.*

1. I hope it will not be imagined, that I have such narrow thoughts of the subject I treat of, Cold, as to believe, that I have compriz'd under those few Titles, prefix'd to the Sections of this Historical Treatise, all the Particulars that I knew to belong to so comprehensive a Theme, as would readily appear, if I thought it convenient to insert here the Scheme of Articles of inquiry, that I drew up to direct my self, what inquiries and Experiments to make. But though there were divers of those Heads, to which I could say so little, that I judg'd it improper to assign them distinct Titles, *because* as to some of /

them, I had not time and opportunity to make those Trials, which if I had not wanted those Requisites, might have been made even here in *England*: and *because* also, as to more of them, I conceiv'd my self unable to produce in this temperate Climate, so strong and durable a Cold, as seem'd necessary to make the trials, that might be referr'd to them, succeed so far, as to satisfie my doubts, either affirmatively, or negatively: Though, I say, these, and some other Considerations kept me from increasing the Number of the Titles, among which I have distributed the Experiments and Observations, that make up the foregoing part of this Treatise, yet since divers particulars have occur'd to me, which though they seem not properly reducible to the foregoing Titles, do yet belong to the subject and design of this Treatise, I think it fit to annex them in this place, and without any other order then that, wherein they shall happen to occur to me, throw them into this one Section, together with some loose Experiments, and divers / Relations, that I have met with among Navigators and Authors, that have travell'd into the Northern Climates, touching Cold, not forbearing to insert promiscuously among them, some few *Paralipomena*, which if they had seasonably come to my hands, or into my mind, might have had a more proper place among the foregoing Sections, or have composed a Title by themselves. Wherefore though the Observations

will not be altogether unaccompanied with Experiments, yet for the reasons above intimated, much the greater part of what is to be deliver'd under this Title, will consist of Collections out of Voyages, in which the strange things mention'd, being such as we cannot examine by our own Trials, I can equitably be thought answerable for the Truth of nothing, but the Citations.

2. I remember I tri'd at several times divers Experiments, to discover, whether or no congelation would by constriction of the pores of Bodies, or vitiating their Texture, or arresting the motion of their parts, / hinder them from emitting those *Effluvia*, that we call odors, but the Register of these Observations, being unhappily lost in one of my late removes, I dare add but these few, wherein I have no cause to distrust my memory.

3. I did in the Moneths of *December* and *January*, at several times gather differing sorts of flowers in frosty weather, but in most when they were freshly gather'd, and hastily smelt to, I could scarce perceive any sensible smell, whether it were, that the causes above hinted, hinder'd the expiration of the odoriferous steams, or that the cold had some undiscerned influence upon the Organ of smelling, which made the sense more dull, or that the same cold kept the Alimental juice of the flowers from rising in such plenty, and abounding so much with spirituous parts, as was usual at the more friendly seasons of the year: and this seem'd the more likely to be one reason of the *Phaenomenon*, because most of the flowers were flaggy, and as it were ready to wither, and because also a Primrose, / that was vigorous and fresh in its kind, had an odor, that was manifestly (and 'twill easily be believ'd, that it was not strongly) sweet, and genuine.

4. I took also about an ounce by guess of Rose-water, and putting it into a

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small vial, after I had smelt to it, it was expos'd to freez in the open Air, and when it began to have ice in it, I then smelt to it again, but found not the perfume considerably, if so much as manifestly abated, and lastly, having suffer'd it to continue in the Air, that was then very sharp, till 'twas quite frozen, and discover'd no liquor, when the vial was turn'd upside down, the ice notwithstanding was not destitute of a graceful and genuine sent, though it seem'd somewhat faint; but after the ice was reduc'd to water again, the fragrancy appear'd considerable. But on this occasion 'twill not be improper to subjoyn this Caution, That care must be had in Trials of this Nature, to make ones estimate betimes, for if a man should stay too long about it, there is danger, that the warmth of / ones breath and face may relax the pores, or thaw the surface of the ice, that is held near his Nose, and both free and excite the Corpuscles of smell, that are imprison'd there, that so instead of ice he may smell a liquor. The reasonableness of which advertisment may be justifi'd by an Experiment that I am about to annex. For being pretty well confirmed by the casual and unwilling Observations of one of my friends, curious in making sweet water, That even Liquors, more easie to be spoilt then Rose-water, would not have their fragrancy destroy'd, though perhaps impair'd, nor so much as their odors for the time quite imprison'd and suppress'd by congelation, and this appearing congruous to what I formerly noted of the *Effluvia*, that may by the Decrement of weight be gathered to issue from ice it self, I thought it worth while to try, whether stinking Liquors would not be more alter'd by congelation, then odoriferous ones: and accordingly having procur'd some rain water, that had been kept in a Tub, till it / stunk so strongly, that I could hardly endure it near my nose, I caus'd a pottinger full of it to be expos'd all night to a very sharp Air, and examining it the next morning, when it was all turn'd into ice, neither I nor some others, to whom it was offer'd, could perceive any stinck at all in it: and having in another place, but with as stinking water, repeated the Experiment, when the pottinger was the next morning brought to my beds side, I found it to smell abominably, whereupon guessing, that this difference proceeded from some thaw made by the warmth of the room in the superficial parts of the ice, I found it to be so indeed, partly by the help of the light, which discovered a little liquor upon the ice, and partly by exposing the vessel with that liquor in it to the cold Air again, by whose operations an ice was produc'd, that was perfectly inodorous; and I remember, that one of these parcels of ice being thaw'd, seem'd to be less

stinking then before / it had been frozen,<sup>†@</sup> and if I had not been diverted, I should have tried, whether this ice, that did not emit odors, would emit like other ice, *Effluvia*, discoverable by the Scales: for whether the ice would lose of its weight, which seem'd the more probable, or would not, the event may afford a not inconsiderable hint.

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5. It is a thing not only remarkable, but scarce credible, that though the Cold has such strange and Tragical effects at *Musco*, and elsewhere in Cold Countries, as we have formerly mention'd, especially a little after the beginning of this 18. and somewhere in the 19. Section, yet this happens to the *Russians* and *Livonians* themselves, who not only by living in such a Countrey, must be accustomed to bitter Colds, but, who to harden themselves to the Cold, have us'd themselves, and thereby brought themselves to be able / to pass to a great degree of Cold, from no less a degree of heat, without any visible prejudice to their healths. For I remember, that having inquired of a *Virtuoso* of unquestionable credit, whether the report of our Merchants, concerning this strange custom of the *Muscovites* and *Livonians* were certainly true, he assur'd me, that it was so, at least as to the *Livonians*, among whom being in their Countrey, he had known it practis'd. And the same was affirmed to me by an ingenious person, a Doctor of Divinity, that had occasion some years since to make a journey to *Musco*. And the Tradition is abundantly confirm'd by *Olearius*, whose Testimony we shall subjoyn, because this seems one of the eminentest, and least credible instances, that we have yet met with of the strange power that custom may have, even upon the Bodies of men. 'Tis a wonderful thing, says he,<sup>†@</sup> to see how far those Bodies (speaking of the *Russians*, that are accustomed and hardned to the Cold) can endure heat, and how when it makes them ready to faint, they / go out of their Stoves stark naked, both men and women, and cast themselves into cold water, or cause it to be pour'd upon their Bodies, and even in Winter wallow in the Snow. To which passage our Author adds from his own observation particular Examples of the Truth of what he delivers.

6. I had several years since, the curiosity to try, whether there were any truth in that tradition, which is confidently affirm'd, (and experience by some is pretended for it) that the Beams of the Moon are cold, but though I were not able to find any such matter, either by the ununit'd beams of the Moon, or by the same beams concentr'd by such Burning-glasses as I then had; yet having some years after furnish'd my self with a large and extraordinary good mettalline Concave, I resolv'd to try, whether those beams were not only devoid of cold, but also somewhat warmish, since they are the Sun-beams, though reflected from the Moon. And we see, that his beams, though reflected from glasses not shap'd for Burning, may yet produce / some not insensible degree of warmth. But notwithstanding my care to make my Trials in clear weather, when the Moon was about the full, and, if I misremember not, with a Weather-glass, I could not perceive by any concentration of the Lunar beams, no not upon a black object, that her light did produce any sensible degree, either of cold or heat; but perhaps others with very large glasses may be more succesful in their Trials.

7. On this occasion I shall add, that meeting the other day in a Book-sellers shop, with the works of the Learned Physician *Sanctorius* (whom I look

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upon as an inquisitive man, considering when and where he liv'd)<sup>†\*</sup> a Picture drew my eyes to take off an Experiment, whereby he thinks to evince the light of the Moon to be considerably hot, which he says, he tri'd by a Burning-glass, through which the Moons light being cast upon the Ball of a common Weather-glass, the water was thereby depressed a good way, as appear'd to many of his disciples, amidst whom the / observation was made. But though this may invite me, when opportunity shall serve, to repeat my Trials, yet I must till then suspend my assent to his Conclusion. For my Burning-glass was much better, then by the Narrative his seems to have been, and my Trials were perhaps at least as carefully and impartially made, as his Experiment in which

this may probably have impos'd upon him; That performing the Experiment, a company of his Scholars, whilst they stood round about his Thermoscope, and stoop'd (as in likelihood their curiosity made them to do) to see by so dim a light the event of the Experiment, the unheeded warmth of their breath and bodies might, unawares to *Sanctorius*, somewhat affect the Air included in the Weather-glass, and by rarifying it, cause that depression of the water, which he ascrib'd to the Moon beams. But because this is a conjecture, I intend, if God permit, to repeat the Experiment, when I shall have opportunity to do with a more tender Weather-glass, then I had by me, when I made my former Observations. /

### *To the XI. Title.*

By the unsuccessfulness of the former attempts made with an Iron instrument, I was invited, especially being at another place, where I was unfurnish'd with such hollow Iron balls, as are mention'd *Num.* the 10. to substitute the following Experiment.<sup>†\*</sup> I caus'd a skilful Smith to take a Pistol barrel, guess'd to be of about two foot in length, and of a proportionable bore, and when he had by riveting in a piece of Iron, exactly stopp'd the touch-hole, I caus'd him to fit to the nose of the barrel a screw, to go as close as well he could make it, and then having fill'd it to the very top with water, I caus'd the screw to be thrust in (which could not be done without the Effusion of some of the water) as forcibly as the Party I imploy'd was able to do it, that the water, dilated by Congelation, might not either drive out the screw, or get between it and the top of the Barrel, / and having then suspended this barrel in a perpendicular posture in the free Air, in a very cold night, which then unexpectedly happen'd, and gave me the opportunity of making the trial, I found the next morning, that the intumescent water had thrust out a great part of the screw, notwithstanding, that to fill up intervals, I had oyl'd it before, and was got out betwixt the remaining part of it, and the barrel, as

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appear'd by some ice, that was got out, and stuck round about the screw; wherefore the bitter cold continuing one day longer, I did the next night cause the intervals, that might be left betwixt the male and female screws, to be fill'd up with melted Bees wax, which I presum'd would keep the screw from being turn'd by the water: and having in other points proceeded as formerly, I found the next morning, that the screw held, as I desir'd, and the preceding night having been exceeding bitter, the cold had so forcibly congeal'd and expanded the water, that it burst the Iron barrel somewhat near the top, and made a considerable / and oblique crack in it, about which a pretty quantity of ice appear'd to stick, besides that there were three or four other flaws, at some of which smaller quantities of water appear'd to have got out. At the same time, that I bespoke this Iron Barrel of the Smith, I order'd him to get me a brass one fill'd up after the same manner, to make the Experiment the more satisfactory. But though he could not procure it, yet the success was not unwelcome, because it was manifest, that there were cracks in the Iron in one place conspicuous, and in others easily discoverable, by blowing into the barrel, and putting on the outside of the suspected parts, either spittle, or some fit liquor, whose agitation plainly disclos'd the egress of the wind, and there appear'd small cause to doubt, but that these cracks were produc'd by the operation of the cold, since not only the Smith was a skilful man in his trade, and one that I us'd to imploy about Instruments, and also the barrel had been sometimes kept many hours fill'd with water, without / appearing other then very stanch: but which is the considerablest circumstance the night before, the frost as I lately noted, was not able to make the water break out at any of these clefts, though it were able to force it self a way out at the screw, in spite of all the care we had taken to make it go close. I have only this circumstance to add about this matter, that when by thawing one part of the ice, some pieces of the rest were got out of the barrel, all I took notice of appear'd to be full enough of Bubbles, but yet such as seem'd lesser then ordinary, whether they were so by chance, or were determin'd to be so, by the resistance or compression, which the freezing water found upon its endeavouring to expand it self in the barrel. /



Long since the writing of the foregoing Section, meeting with a passage in *Bartholinus*,<sup>†\*</sup> where he vouches *Cabæus* for the Experiment of congealing water (without limiting it to any season of the year) by putting Salt petre into it, and shaking it strongly, I was thereby confirmed, that I was not mistaken, in supposing, that *Gassendus* (mention'd in the former Section) did not exclude that corporal and visible Nitre out of the number of the grand efficients of congelation. For *Cabæus* having publish'd his comment upon *Aristotles* Meteors (whence this experiment is taken by *Bartholinus*) before

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*Gassendus* publisht his Book, 'tis probable, that he as well as others borrowed the Experiment from him, and *Cabæus*, as *Bartholinus* quotes him, prescribes the putting the Salt-petre its self into water, which being a while put into a brisk motion, will / after some agitation, not only refrigerate that water, but bring it to a true and proper congelation.

Wherefore suspecting, that this relation, wherein *Bartholinus* says, he will believe him without an oath, may have given rise to the opinions and affirmations of those ingenious writers, that have since ascrib'd such wonderful coldness to Nitre, and finding in *Bartholinus*, that *Cabæus*'s proportion betwixt the Nitre and the water, was that of 35. to a 100. that is almost as *one* to *three*, I thought it very well worth while to make Trial of an Experiment, which seem'd to me little less unlikely then considerable.

I took then a pound of good Salt-petre, and near 3. pound of common water (to observe the more narrowly *Cabæus*'s proportion) these being put into a large new Pipkin, were kept constantly and nimbly stirr'd about, sometimes by me, sometimes by one or other of my Domesticks relieving one another, when they were weary, but though the mixture was with a kind of broad glass spattle / kept in a brisk motion, that for the most part was after the manner of a whirle-pool, and sometimes a more confus'd agitation, and though we kept it thus stirring for almost an hour and a half, till we saw no likelihood of effecting any thing by trying our selves any further, yet not only we could not perceive, that any Atom of true ice was produc'd, whereas according to our Authors we might have expected a true and perfect congelation of all or the greatest part of the water, but we did not find, that there was so much as any freezing of the vapours on the outside of the vessel; and for this reason we thought fit, about the same time, to try the Experiments by another kind of Agitation, and mixing two ounces of Salt-petre with about six of water, in a conveniently siz'd vial, we did several of us successively vehemently shake the vial too and fro, till we were almost tyr'd; but neither this way was there produced the least ice within the glass, or the least congelation of the vapours of the Air on the outside of it. 'Tis / true, that when so great a proportion of Salt-petre began to be dissolv'd in the Pipkin, the water had a sensible increase of coldness, which afterwards seem'd to diminish, when once the Nitre was dissolv'd; but not to mention, that (if I much mistake not) we have observ'd the water to be refrigerated, when upon the dissolution of common salt, multitudes of actually cold and solid Corpuscles came to be every way dispers'd through it; this coldness produc'd by the Nitre, was very far short of the degree requisite to congelation: for to satisfie my self, that my sense did not misinform me, I took a good seal'd Weather-glass of about ten or twelve inches long, and immersing it into the cold mixture of Nitre and Water, I observ'd the tinted spirit of Wine in the stem to descend not inconsiderably, and when I perceived that degree of cold to have wrought its effect, I remov'd the Thermoscope into a vial fill'd with common water, about which I had caus'd to be plac'd a mixture of beaten ice and salt, to refrigerate the / contained water, in which the ball of the Instrument

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being plac'd, the spirit of Wine hastily descended two or three inches below that place at which it stood, when 'twas remov'd out of the Nitrous solution: And for further satisfaction removing the

Thermoscope once again into that solution, the spirit of Wine in the stem was hastily impell'd up, as if the bubble had been put into warm water. And once more the Weather-glass being remov'd into the formerly mention'd refrigerated water, the tinted liquor began to fall down hastily again, and within a while subsided almost into the bubble, whereupon to avoid injuring the instrument, we thought fit to take it out; so that upon the whole matter, if the learned *Cabæus* were not deluded by mistaking some Crystals of Nitre (which I have observ'd easily to shoot again in water, that has been glutted with it) for true and proper ice, I cannot but wonder at his assertion, and must take the liberty to think my self warranted by so many Harmonious Trials, as I have found / unfavourable to the suppos'd supremeness of Cold in Salt-petre, to retain my former opinion about it, till more succesful Experiments withdraw me from it.

'Tis a receiv'd Tradition among the Water-men and many others, that the Rivers, if not Ponds also, are frozen first at the bottom, and begin to thaw there. But though I find this opinion to be in request, not only among English Water-men, but among the French too, yet I think it may be very warrantably question'd: For 'tis evident in waters we expose to freez in large vessels, that the congelations begin at the surface, where the liquor is contiguous to the Air, and thence as the cold continues to prevail, the ice increases and thickens downwards, and therefore we see, that Frogs retire themselves in frosty weather to the bottom of ditches, whence I have had many of them taken out very brisk and vigorous, from under the thick ice that cover'd the water. And I have been informed by an observing person, that at least in some places, 'tis usual in / Winter for shoals of Fishes to retire to those depths of the Sea, if not of Rivers also, where they are not to be found in Summer. Besides if Rivers were frozen at the bottom, we must very frequently meet in the emergent pieces of ice, the shapes of those irregular Cavities and Protuberances, that are often to be found in the uneven soils, over which Rivers take their course, whereas generally those emergent pieces of ice are flat, as those flakes, that are generated on the surface of the water. Moreover if even deep rivers freez first at the bottom, why should not very many Springs and Wells freez first at the bottom too, the contrary of which nevertheless is obvious to be observ'd. In confirmation of all which we may make use of what we formerly noted (in the Section of the *Primum Frigidum*<sup>†\*</sup>) about the practise of the Masters of the French Salt-works, who by overflowing the Banks and Causeways all the winter, keep them from being spoil'd by the frost, which could not be done, if the waters they stand under froze as well at the bottom, as at the Top. /

But I find, that that, which deceives our Water-men, is, that they often observe flakes of ice to ascend from the bottom of Rivers, to the Top, and

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indeed it often happens, that after the hard frost has continued a while, these emergent pieces of ice, do very much contribute to the freezing over of Rivers. For, coming, in some of the narrower parts of them, to be stopp'd by the superficial ice, that reaches on each side of the River a good way from the Banks towards the middle, those flat icy bodies are easily cemented by the violence of the cold, and by the help of the contiguous water, to one another, and by degrees straitning, and at length choaking up the passage, they give a stop to the other flakes of ice, that either emerging from the bottom, or loosened from the banks of the River, or carried down the stream towards them, and these being also by the same Cold cemented to the rest, the River is at length quite frozen over. And the reason why so many flakes of ice come from the bottom of the River, seems to be, / that after the water has been frozen all along near the banks, either the warmth of the Sun by day, or some of those many casualties, that may perform such a thing, does by thawing the ground, or otherwise loosen many pieces of that ice together with the earth, stones, &c. that they adher'd to, from the more stable parts of the banks, and these heavy bodies do by their weight carry down with them the ice they are fastned to; but then the water at the bottom of the river being warm in comparison of the Air in frosty weather (since that even common water is so, we have manifested by experience, where we show how much sooner ice will be dissolv'd in water, then thaw'd in Air<sup>†@</sup>) the dispers'd ice is by degrees so wrought upon, that those parts by which it held to the stones, earth, or other

heavy bodies being resolv'd, the remaining ice being much lighter bulk for bulk, then water, gets loose, and straightway emerges, and may perhaps carry up with it divers stones and clods of earth, that may yet happen to stick to it, or be inclos'd in it, / the sight of which perswades the Water-man, that the flakes of ice were generated at the bottom of the river, whereas a large piece of ice may carry up and support bodies of that kind of a great bigness, in case the ice it self be proportionably great, so that the Aggregate of the ice, and heavy bodies, exceed not the weight of an equal bulk of water. On which occasion I remember, that Captain *James Hall* in a voyage, extant in *Purchas*, relates,<sup>†\*</sup> that upon a large piece of ice in the Sea they found a great stone, which they judg'd to be three hundred pound weight. But of the Tradition of the Water-men we shall say no more, then that this hath been discours'd, but upon no great information, though the best we could procure; so that for further satisfaction, it were to be desir'd, that either by sending down a Diver, or by letting down some instrument fit to feel (if I may so speak) the bottom of Rivers with, and to try, whether ice, if it met with any, be loose from, or uniformly coherent to the ground, and also bring up parcels of / whatever stuff it meets with there, the matter were by Competent Experiments put out of doubt.

We took a seal'd Weather-glass furnish'd with spirit of Wine, and though

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not above 10. inches long in all, yet sensible enough, and having caus'd a hole to be made in the Cover of a Box, just wide enough for the smaller end of the Glass to be thrust in at, we inverted the Thermometer, so that the ball of it rested upon the cover of a Box, and the pipe pointed directly downwards, then we placed about the ball a little beaten ice and salt, and observ'd, whether, according to our expectation, the tinted spirit that reach'd to the middle of the pipe, or thereabouts, would be retracted upon the refrigeration of the liquor in the ball, and accordingly the spirit did in very few minutes ascend in that short pipe above an inch higher, then a mark whereby we took notice of its former station, and would perhaps have ascended much more, if the application of the frigorifick mixture had been continued, by which, and another succeeding / Experiment to the same purpose, it seems, that the condensation of liquors by cold, is not always effected by their proper gravity only, which ordinarily may be sufficient to make the parts fall closer together: but whether in our case the contraction be assisted by some little tenacity in the liquor, or by the spring of some little aerial, or other spirituous and Elastick particles, from which the instrument was not perfectly freed, when it was seal'd up, or which happened to be generated within it afterwards, will be among other things more properly inquir'd into in another place, where we may have occasion to make use of this Experiment.

There is a famous Tradition, that in *Muscovy*, and some other cold Countries, 'tis usual out of Ponds and Rivers to take up good numbers of Swallows inclos'd in pieces of ice, and that the benumm'd birds upon the thawing of the ice in a warm room, will come to themselves again, and fly about amazedly for a while, but not long survive so great / and sudden a change. I have in another Treatise already said somewhat about this Tradition, and therefore shall now say no more of it, then these two things.<sup>†\*</sup> First, that I since was assur'd by a person of honour, that is very curious, and was commanded by (a many ways) great Prince to inquire out the truth of it, when he was in some of those Countries, where the thing is said to be familiar enough, and that the eminentest and soberest persons he could ask affirm'd the thing to be true: But (secondly) having lately inquired about this matter of a knowing person of quality, that was born and bred in *Poland*, he answered me, That in the parts where he liv'd, it was a very general and unquestion'd opinion, that Swallows often hid themselves all the Winter under water in Ponds and Lakes, and Seggy places, and that the Fishermen, when having broken the ice, they cast their Nets for Fish, do draw them up benumbed, but not dead, so that they quickly in Stoves recover their wings, but seldom after that prolong their / lives: But as for their being taken up in ice, he told me, he had not heard of it, though I see not why in case they commit themselves to shallow waters, as those of Ponds and Seggy places,

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often are a sharp lasting frost may not sometimes reach them. And therefore that which left me the greatest scruple about this Tradition, is, That this Gentleman, notwithstanding his curiosity, could not affirm, that ever he himself had seen any example of the thing he related.

But I will take this occasion to add, that having a mind in frosty weather to try some Anatomical Experiments about Frogs, one that I imploy'd breaking in a Ditch some ice that was very thick, and of which he was to bring me a quantity, found in the water, that was under the ice, good store of Frogs (besides some Toads) which I found to be very lively, and divers of which I kept for certain uses a good while after.

To confirm, and to add some *Paralipomena* unto what I have deliver'd in the Second, and in the Twentieth Titles, about the frosts getting into / hard and solid bodies, I shall here subjoyn some particulars there omitted, which I have learned partly from Experiments, and partly from persons worthy of credit, whom I purposely consulted about this matter.

And first as to the freezing of Wood, we have sometimes tri'd it by purposely exposing partly other Wood, and partly branches cut off from growing Trees, to an intense degree of Cold, by which the wood seem'd in one night to be for some little depth manifestly enough invaded by the frost. But a domestick of mine having a little while since had occasion to fell an old Apple-tree, on a day that had been preceded by a fortnights bitter frost, came and informed me, That he found, that the frost had evidently pierc'd into the very middle of it, though it were about a foot in Diameter. And an Experienc'd Artificer, whose head and hand were much imploy'd about the building of great mens houses, told me, that he had often seen here in *England* pieces of Timber it self manifestly / frozen, and rendred exceeding difficult to be saw'd, the frost also appearing by evident signs to continue in the saw-dust. And therefore it will be the less strange, if in *Poland* the effects of Cold upon wood be more conspicuous. For a learned native assur'd me, that in his Countrey 'twas usual to have wood frozen so hard, that the Hatchets would not cut it, but rebound from it, and that 'twas very usual to hear in the night a great many loud cracks, almost like the reports of Pistols, of the shingles or wooden tyles, wherewith in many places they cover their houses instead of Slate, and this (as I purposely ask'd) when the weather was dry, and excessively cold. When I likewise inquir'd about the thawing of wood, he told me, he had several times seen pieces of Timber, which having been throughly frozen in the Air, did, when brought into rooms made warm by Stoves, become cover'd with a kind of hoar frost, and made them look white, and that though his Bow (which he shew'd me) were very strong and / tough, as being made not of wood, but horn, and other close materials, it would be so chang'd by the frost, that unless special care were had in the thawing of it, it would break.

That Marle and Chalk, and other less solid terrestrial Concretions will be shatter'd by strong and durable frosts, is observ'd by Husbandmen, who thereby find it the better fitted to manure their land, the Texture of those

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bodies, during whose intireness, the parts most proper to feed grass and corn, are more lock'd up, being by congelation in great part dissolv'd, but that true and solid stones wont to be imploy'd in noble and durable Buildings, should be spoil'd by the frost, will perhaps to most readers seem very improbable. And therefore I shall here add what I have learn'd by inquiry of the ingeniourest and most experienc'd Mason I have met with, because it may not only surprize most readers, but prove an useful observation to him. Having then inquir'd of this Tradesman, whether he did not find, that some free stone, a name vulgarly known, / would not be spoil'd by the frost, he told me, that he had often observ'd both free stone and harder stones then that, to be exceedingly spoil'd by the frost, and reduc'd to crack or scale off, to the blemishing and prejudice of the houses, that are built of them. But because it may be objected against this, that experience shews us, that divers of the stateliest Fabricks in *England* have these stones for their chief materials, and yet indure very well the



inclemencies of the Air, the reply may be, that the difference may not consist in the peculiar natures of the stones imploy'd, but in the several seasons in which the same kind of stones are digg'd out of the Quarry. For if they be digg'd up, when the cold weather is already come in, and imploy'd in building the same Winter, they will, upon very hard frosts, be apt to be shatter'd or scale, but if they be digg'd early in the Summer, and suffer'd to lye expos'd to the Sun and Air, during all the heat of the Summer, these season'd stones, if I may so call them, may outlast many / sharp Winters unimpair'd. It seems to me worth trying, whether during their insolation, if that term may be allow'd me, there do not by the operation of the heat and air upon them, exhale a certain unripe mineral, sap, or moisture (whose recess may perhaps be discover'd by weight) which if it remain in the stone, may be very piercing frosts be congeal'd almost like the sap in Timber-trees, and shatter the Texture of the stone, which agrees well with what was told me by an understanding person, that is Master of a great Glass-house, of whom having purposely inquir'd, whether he did not find, that his great earthen pots, which are made up with as little water as is possible, & are deservedly famous for their durable Texture, had not that Texture alter'd and impair'd by very piercing frosts; he assur'd me, that if he did not take care to keep the frost (as they speak) from getting into them, those great and solid vessels, wherein he us'd to keep his glass in fusion, would in the fire scale or crack (and perhaps fly) and become unserviceable / no less then some weeks sooner, then if they had never been impair'd by the frost. And when I inquired, whether also glass it self would not be much prejudiced thereby, he affirmed to me, that oftentimes in very hard frosts many glasses, that had continued intire for many weeks (for that circumstance I was sollicitous to ask about) would as it were of their own own accord crack with loud noises. But whatever prove to be the issue of such Trials, it will not be amiss to confirm the *Phænomenon* it self, by the testimony of an illiterate, but very experienc'd French Author, who on a certain occasion tells us, (as I also take notice in

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another Treatise<sup>†@</sup>) *That he knows the stones of the mountains of Ardenne (famous enough in France) are harder then Marble, and yet the inhabitants of that Countrey do not draw them out of the Quarry in Winter, because they are subject to the frost. And it has been divers times seen, that upon thaws, the rocks without being cut, have fallen down, and kill'd many.*

But it may yet seem far more unlikely, / that frosts should get into mettals themselves, and yet having ask'd the newly mention'd *Polonian*, whether he had observ'd any thing of that kind, he answer'd, that he had often by drawing out his sword and pulling out his pistols, when he had been long in the field, and came into a hot room, found them quickly almost whitened over, by a kind of small hoar frost. But whether this were, as he conceiv'd any thing, that was drawn out of the Steel, and settled on the surface of it, I want circumstances enough to make me willing to determine. But if we will credit *Olaus Magnus*, it must be confess'd, that considerably thick pieces of Iron and Steel it self, will in the Northern Regions be render'd so brittle by the extreme frost, that they are fain to temper their instruments after a peculiar manner: his words, which being remarkable, I forbear to alter, are these,<sup>†@</sup> *Videntur præterea ferrei ligones certa ratione fabricati, quia his spissa atque indurata glacies cæteris instrumentis ferreis non cedens faciliùs infringitur dum aliæ secures chalybe / permixtæ, in vehementi frigore ad solum glaciei vel virentis arboris ictum instar vitri rumpuntur, ubi ligones prædicti sive ferreæ hastæ fortissimi manent.* Which testimony, notwithstanding what some have written to this Authors disparagement, does not seem to me at all incredible. For I remember, that even here in *England* I have had the curiosity to cause trials to be made in very frosty weather, whereby, if an expert Smith I then us'd to imploy, did not *gratis* deceive me in the Irons I imploy'd, that mettal may by such degrees of cold, as even our Climate is capable of, be rendered exceeding brittle, as he several times affirm'd to me, that there are some kinds of iron which he could hammer, and turn, as they phrase it, cold in open weather, which yet in very hard frosts would become so brittle, as by the same way of working easily to break, if not to flye asunder. And this he affirm'd both of Iron and Steel, of which latter mettal another very skilful workman, whom I also consulted, certifi'd the like: but though this disagreed not / with trials purposely made on Iron rods had inform'd me, yet presuming, that in such a nice piece of work as a

spring, some further satisfaction about this matter might be obtain'd, I inquired of a very dexterous Artificer, that was skill'd in making springs for others, whether or no he found a necessity of giving springs

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another temper in very frosty weather, then at other seasons, and he answered me, that in such weather, if he gave his springs the same temper, that he did in mild and open weather, they would be very apt to break. And therefore in very sharp seasons he us'd to take them down lower, as they speak, that is, give them a softer temper then at other times, which as it makes it probable, that the cold may have a considerable operation upon bodies, upon which most men would not suspect it to have one, so that discovery may afford a hint, that may possibly reach further then we are yet aware of, touching the interest that cold may have in many of the *Phænomena* of nature. /

I should here subjoyn, that in prosecution of what is deliver'd in the XX. Section about the weight of solid bodies, that I there wish'd might be expos'd to a congealing Air, I did cause some Trials of that kind to be made in a very frosty night, especially with Bricks, but something that happened to the only Scales I then had fit for such an Experiment, made me doubt, whether some little increase of weight, that seem'd to be gain'd by congelation, were to be reli'd upon, though there did not appear any hoar frost, or other thing outwardly adhering, to which the effect could be ascrib'd.

It is a Tradition, which the Schools and others have receiv'd with great veneration from their Master *Aristotle*, that hot water will sooner freez then cold; but I do not much wonder, that the learned *Cabæus*, as I find him quoted by *Bartholinus*, should contradict this Tradition, though he be himself a commentator upon that Book of *Aristotle*, wherein 'tis deliver'd.<sup>†\*</sup> For I could never satisfie my self, that there is (at least / with our water, and in our Climate) any truth in the Assertion, though I have made trial of it more ways then one, but it may very well suffice to mention a few of the plainest and easiest Trials, with whose success I am well satisfi'd as to the main, as the Reader also will, I doubt not, be; though not having, for want of health, been able to have so immediate an inspection of these, as of the rest of my Experiments, I was sometimes fain to trust the watchfulness of my servants (whom I was careful to send out often) to bring me word how long after the first freezing of the cold water, it was before the other began to be congeal'd.

We took then three pottingers, as near of a size as we could, and the one we fill'd almost to the top with cold water, the other with water, that had been boil'd before, and was moderately cool'd again, and the third with hot water; these three vessels were expos'd together in the same place to the freezing Air.

In the Entry of one of the Trials, I find, that being all three put out at / half an hour after eight of the clock. That the pottinger that contain'd the cold liquor began to freez at  $\frac{1}{4}$  after ten.

That which contain'd the water heated and cool'd again, began to freez  $\frac{3}{4}$  past ten.

And that which contain'd the hot water, at half an hour after eleven, and somewhat better. So that though all froze within the compass of two hours, yet the cold water began this time to freez an hour and a  $\frac{1}{4}$  sooner then the hot.

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These pottingers were earthen, but I elsewhere made the Trial in others of mettal, and there also the cold water began to freez, both before that which had been heated and cooled again, and long before the hot.

Another time I measured out the water by spoonfuls into pottingers (not having then by me any fit Scales to weigh it) to be the more sure, that the quantities of water should not be considerably

unequal, and then also the cold water froze a considerable while before the hot.

But my usual jealousy in the making / nice Experiments, tempting me to inquire, whether the water in some of the former Trials had not been heated in a stone Bottle, not a Skillet, it was confess'd, that it was so, but that the bottle us'd to contain nothing but Beer, and had been wash'd before-hand: And though I did not think, that the bottle could have any considerable influence on the Experiment; yet least it should be suspected, that the scalding water, might have imbib'd some spirituous parts remaining yet among the minute dregs of Beer in the pores of the bottle, for the greater security I caus'd the water to be heated in a Skillet, and because in one of the Trials made in a Village, where we had not choice of pottingers, the cold water chanc'd to be put into one, that afterwards seem'd less, then that wherein the hot was expos'd, I did this very day repeat the Experiment, by putting cold water into a somewhat larger pottinger, heating the other water in a Skillet, and the event of the Trials is this,

That the cold water being put out / with the rest at  $\frac{3}{4}$  after 6. began to freez somewhat before  $\frac{1}{2}$  after 7.

The water heated and cool'd again, began to freez  $\frac{3}{4}$  after 7. And having these frozen waters a pretty while by me, I sent in for my own further satisfaction, for the hot water, and found it not to be, in the least, frozen at half a quarter after 8. So that supposing it to continue half a quarter of an hour longer before the beginning of its congelation,†@ it was twice as long ere it began to freez, as the cold water had been.

By which we may see how well bestow'd their labour has been, that have puzzled themselves and others, to give the reason of a *Phænomenon*, which perhaps with half the pains they might have found to be but *Chymical*.

I have been the more circumstantial in setting down these Trials, that I may express a civility to so famous a Philosopher as *Aristotle*, and also because Artificial Congelations, which we can commonly best command, and which we have the oftenest us'd about our other Experiments, / are not so proper for this. For having formerly had the curiosity to take two pipes of glass made of the same Cylinder, that they might be of equal bore, and having seal'd each of them at one end, and having fill'd both to the same height, and then stirr'd them too and fro together in a mixture of beaten ice, water and salt, (which mixture I make use of for the effecting sudden Congelations) I found both waters to freez too quickly to make a notable disparity in the length of times, that they remain'd uncongeal'd: And we will not on this occasion omit

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one *Phænomenon* afforded us by these Trials, because it may admonish men, how cautious they ought to be in making nice Experiments. For having once made the formerly mention'd Trial, with glass pipes, that were but slender (as not exceeding the bigness of a mans fore-finger) and having for greater caution put the hot water first into one glass, and then into another, we found one time, that the hot water froze first, and wondering at it, we examin'd the glasses, and / perceiving one of them to be more Conical or acuminated, where it had been seal'd up then the other, it seem'd probable, and afterwards appear'd true, that the water in this acuminated part, being suddenly frozen by reason of the slenderness of the glass there, promoted and accelerated the Congelation of the rest, so that whether it were the cold or the hot water, that was put into that pipe, it would thereby gain a manifest advantage.

In the foregoing Experiments (made in pottingers) I made use not only of cold and hot water, but of water that had been heated and cool'd again, though not reduc'd to its full pristine coldness, to prevent the Objections of some, that might pretend, that such water would have frozen sooner then Cold, which yet would not salve the common opinion which specifies not such water. /



And it seems, that such Cautions as I have been mentioning, are not altogether useless. For accidentally casting my eye upon the *Circulus Pisanus* of *Berigardus* upon *Aristotles* Meteors,<sup>†\*</sup> I somewhat wonder'd to find, that an Author, who is look'd upon to be a great adversary of *Aristotle*, except in his dangerous and ill-grounded conceit of the eternity of the world, and some other erroneous opinions, does yet indeavour to justifie *Aristotle* by affirming, that his Experiment will succeed, if by heated water we understand, that which having been heated, is suffered to cool again, till it be reduc'd to the temper of other water which was not heated. For this refrigerated water he says, he has found to congeal much sooner then the other water, but this I confess I am very unapt to believe. For having divers times caus'd cold water to be expos'd to the / Air in frosty weather, with that which had been heated and cool'd again, and having set sometimes one of my Domesticks, sometimes another, to watch them, the events did very much disfavour the assertion of our Author, though care was had of the circumstances most considerable in such an Experiment, as the matter, size and shape of the vessels; the equal degree of cold in the two several parcels of water (into both which I sometimes dipp'd my finger to judge of them before they were expos'd) and the place, in which they were put both together to be frozen. But for further satisfaction, we *elsewhere* took two pottingers, bought purposely for the making of Experiments, of the same size and shape, and in the

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same shop; one of these we almost fill'd with cold water out of a glass, wherein we mark'd how high that water reach'd, that by filling the same glass to the same height with the refrigerated water, we might be able to measure out the same quantity into the other pottinger. This done, I appointed one, / whose care I had no reason to distrust, to examine the tempers of the several waters, with a more then ordinarily sensible Weather-glass, as a far safer Criterion then the bare touch, to judge of the coldness of liquors; these being reduc'd to the same temper, were expos'd to a very sharp Air, and there watch'd by the person, whom (being not well, and unable to support such weather my self) I appointed to attend the Experiment, and he according to direction finding them begin to freez, as 'twere at the very same time, brought me in the two pottingers, in each of which I saw the beginnings, and but the beginnings of congelation, where the upper surfaces of the waters were contiguous to the containing vessels: so that having made this Experiment with much greater exactness then probably *Berigardus* did, or, for want of such instruments as I us'd, *could* make it, I cannot but suspect, supposing the common waters, he and I us'd, to be of the same nature, that he was either negligent or over-seen in affirming, that heated and refrigerated / water, will cool *so much sooner*, as he would perswade us, then other.<sup>†@</sup> And as I am not convinc'd by experience, that it will freez sooner *at all*, so till he have better made out the reason he seems to give of the *Phænomenon*, I must question whether he rightly ascribe after *Cabæus* (if I much misremember not) the congelation of water to a certain *Coagulum*, distinct from the *cold* spirits, that plentifully mingle with the water, which *Coagulum* it seems (for his style is not wont to be very perspicuous) that he would have to consist of certain *dry* Corpuscles, no less necessary to conglaciate water, then Runnet to curdle Milk: And for what this Author says,<sup>†@</sup> that he must have imploy'd boiling or scalding water, who affirms it to be less congealable then other, that mistake may be sufficiently disprov'd by the several above recited Trials, wherein we / found water, moderately refrigerated, to freez much later then cold, and whereas *Berigardus* intimates, that the person whoever he be, that he dissents from, does unskilfully suppose warm salt-water to be the less dispos'd to congelation for being salt, our Author is therein also mistaken; for though it be true what he alledges, that salt *outwardly appli'd* promotes the congelation of water, yet, that *dissolv'd* in water, it has a contrary effect, may appear by the familiar observation, that Sea-water is much more difficult to be congeal'd then fresh water: and to show, that 'tis not a property of Sea-water, but a water impregnated with common Salt, I have several times tri'd, that a strong

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solution of such salt in ordinary water, will not at all be congeal'd by the being expos'd to the Air, even in very sharp frosts, as may be easily collected from some of the Experiments mention'd in the former part of this Book. Another particular there is (about the use of Allume in reference to freezing) in this often cited passage of *Berigardus*, / which I might here examine, if my hast and my indisposedness to ingage in a controversie of small moment, did not injoyn me to defer it till a fitter occasion.<sup>†@</sup>

To confirm the power ascrib'd in the VI. Section to cold, as to the long preservation of bodies from corruption, 'twill not be amiss to add these two remarkable passages, the latter of which affords a good instance of the improvement, that may be made of some degrees of cold to the uses of humane life.

The first observation is afforded us by some of our Countrey-men, in a Voyage extant in *Purchas*, where the writer of it speaks thus: *Of the Samojeds, whose Countrey he visited,<sup>†@</sup> Their Dead they bury on the side of the hills, where they live (which is commonly on some small Islands) making a pile of stones over them, yet not so close, but that we might see the dead Body, the Air being so piercing, that it keepeth them from much stincking savour: so likewise I have seen their Dogs buried in the same manner. /*

The other observation is given us in the description of *Iceland* (made by one that visited it) to be met with in the same *Purchas*'s Collections, where among other things he gives us this Account, which if I mistake not, I have had confirm'd by others, of their strange way of ordering and preserving their Fish.<sup>†@</sup> *Having taken them, they pluck out the bones, and lay up their bowels, and make Fat or Oyl of them: They heap up their Fish in the open Air, and the purity of the Air is such there, that they are hardned only with the Wind and Sun, without Salt, better surely then if they were corned with Salt. And if they kill any Beast, they preserve the flesh without stinck or putrefaction, without Salt, hardned only with the Wind.*

I know not whether 'twill be worth while to add to the fifth and sixth Numbers of the VII. Title, that, for further confirmation of our opinion, that 'tis not Natures abhorrencie of a *Vacuum*, but the distension of the water, that breaks glasses, when the contain'd liquors come to be congeal'd, I did on set purpose fill several vials (some at one time, and / some at another) to the lower parts of their necks (most of which were purposely made long) with common water, and though they were all left unstopp'd, that the external Air might come in freely to them; yet not only one of them, that I stirr'd up and down in a mixture of beaten ice, salt, and water, was hastily broken upon the congelation of the contain'd water, but several others, that were expos'd to be frozen more leisurely by the cold Air only, were likewise broken to pieces,

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by the expansion of the freezing water, as appear'd both by the gaping cracks, and also by this, that the ice was considerably risen in the necks above the waters former stations, which had been noted by marks before; and if it had been more easie for the included water to make it self room, either by stretching the glass, or (rather) heaving up<sup>†\*</sup> the superficial ice congeal'd at first in the neck, or by both those ways together, then to break the vessel, the vial would *probably* have remained intire.

I say *probably*, because I am not / sure, that there may not sometimes intervene in these Experiments somewhat that may need further observation and inquiring. For *as* it seems, that what I have been lately saying may be confirmed by an unstopp'd vial, which was expos'd at the same time to congelation, with this success, that without breaking the vial the water was frozen, and the ice in the neck impell'd up a good way above the height, at which the liquor rested before it began to congeal; *so* on the other side I remember, that I have sometimes had a good store of liquor frozen in a vial, without breaking the glass, though a vial were stopp'd: as if the difference, that I have on other occasions observed betwixt glasses, whereof some are very brittle, and others more apt to

yield, might have an influence on such Experiments, or that some peculiar softness, or other property of the ice, that afforded me my observation, or else some other thing not yet taken notice of, were able to vary their success.

In confirmation of what is delivered / in the VII. Section, about the expansion of water by freezing, I shall add, that having caus'd some strong glass-Bottles of a not inconsiderable bigness to be fill'd with a congealable liquor, excepting the necks, which were fill'd with Sallet oyl, I observ'd, that in a somewhat long, and very sharp frost the contained water was so far expanded by congelation, that it not only thrust up the corks, but the cold having taken away the defluency of the oyl, that liquor together with the water, that could no longer be contain'd in the Cavities of the glasses, being as it seem'd, frozen as fast as it was thrust out of the neck, there appear'd quite above the upper part of the Bottles, Cylinders of divers inches in height, consisting partly of concreted oyl, and partly of congeal'd water, having on their tops the corks that had been rais'd by them.

It is a Tradition very currant among us, that when Ponds or Rivers are frozen over, unless the ice be seasonably broken in several places, the Fishes will dye for want of Air. / And I find this Tradition to be more general, then, before I made particular inquiry into it, I knew of. For *Olaus Magnus* mentions it more then once,<sup>†@</sup> without at all questioning the truth of it, but rather,

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as if the general practise of the Northern Nations to break in divers places their frozen Ponds and Rivers, were grounded upon the certainty of it. In the twentieth Book (which treats of Fishes) after having spoke of the reasons, why the Northern Fishermen imploy so much pains and industry to fish under the ice, and having said among other things, that the nature of the Fish exacts it, he adds this reason, that, *Nisi glacie perforata respiracula susciperent, quotquot in flumine vel stagno versantur, subito morerentur.*<sup>†\*</sup> Another passage of the same Author, and taken likewise out of the same / (20.) Book you may meet with in the Margent,<sup>†@</sup> though in another place he seems to intimate another, and not an absurd, reason of the death of Fishes in Winter, where advertising the Reader, that Ponds and Lakes did generally begin to freez in *October*, he adds, that Fishes are usually found suffocated, when the Thaw comes, *where veins (or springs) of living water do not enter.*<sup>†@</sup> by which passage he seems to make the want of shifted water cooperate to the suffocation of the Fishes. And to the same purpose I shall now add, that having inquir'd of a learned Native, that had had about *Cracovia*, (whose Territory is said to abound much in Ponds) whether the *Polanders* also us'd the same custome, he answered me, that they did, and that sometimes in larger Ponds they were careful to break the ice in eight / or ten several places, to make so many, either vents or Air-holes, for the preservation (as they suppos'd) of the Fish. And when I inquir'd of the often mention'd *Russian* Emperors Physician,<sup>†\*</sup> whether in *Muscovy* the frost kill'd the Fishes in the Ponds, in case the ice were not broken to give them Air, he answered, that in ordinary Ponds it were not to be doubted, but that in great Lakes he could not tell, because the Fishermen use to break many great holes in the ice for the taking of the Fish. For at each of these holes they thrust in a Net, and all these Nets are drawn up together in one great breach made in some convenient place near the middle of the rest.

It appears then, that the Tradition is general enough, but whether it be well grounded, I dare not determine, either affirmatively or negatively, till trial have been made in Ponds with more of design or of curiosity, and watchfulness, then I have known hitherto done, men seeming to have acquiesc'd in the Tradition without examining it, and / to have been more careful,

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not to omit what is generally believ'd necessary to the preservation of their Fish, then to try, whether they would escape without it: Wherefore, though for ought I know the Tradition may prove true, yet

to induce men not to think it certain, till experience has duly convinc'd them of it, I shall represent, That as much as I have in other Treatises manifested, how necessary Air is to Animals; yet whether Fishes may not live, either without Air, or without any more of it, then they may find interspers'd in the water they swim in, has not yet, that I know of, been sufficiently prov'd. For what we have attempted of that nature in our Pneumatical Engine, whether it be satisfactory or not, is not yet divulged. And I remember not to have hitherto met with any writer, (except *Olaus* be construed to intimate so much) that affirms *upon his own observation*, that the want of breaking ice in Ponds has destroy'd all the Fish. Besides, that possibly in frozen Ponds, there may be other reasons / of the death of the Fishes, that are kill'd (if any store of them be so) by very sharp frosts. For who knows what the locking up of some kinds of subterranean steams, that are wont freely to ascend through water unfrozen, may do to vitiate and infect the unventulated water, and make it noxious to the Fishes, that live in it: perhaps also the excrementitious steams, that insensibly issue out of the bodies of the Fishes themselves, may by being penn'd up by the ice, contribute in some cases to the vitiating of the water, at least in reference to some sort of Fishes. For being desirous to learn from a person curious of the ways of preserving and transporting Fish, whether some Fishes would not quickly languish, grow sick, and sometimes dy out-right, if the water they swam in were not often shifted, he assur'd me, that some kinds of them would: and it has not yet, that I hear of, been tri'd, whether or no, though Ponds seldom freez to the bottom, yet the water that remains under the ice (in which it self some Fishes may be now / and then intercepted) may not, even whilst it continues uncongeal'd, admit a degree of cold, that though not great enough to turn water into ice, may yet be great enough, when it continues very long, to destroy Fishes, though not immediately, yet within a less space of time, then that, during which the surface of the Pond continues frozen. But 'tis not worth while to be solicitous about conjectures of causes, till we are sure of the Truth of the *Phænomenon*; and these things are propos'd not so much to confute the Tradition, we have been speaking of, as to bring it to a Trial, which, having no opportunity to make in Ponds, I endeavour'd as well this Winter as formerly, to obtain what information I could from Trials made in small vessels, with the few Fishes I was able to procure. And I shall subjoyn most of these Trials, not because I think them very considerable, but because they are, for ought I know, the only attempts of the kind, that have yet been made.

To satisfie my self, whether the / ices denying access to the Air, was that which destroy'd Fishes in frozen Ponds, I thought upon this Expedient, I procur'd a glass vessel with a large belly, and a long neck, but so slender, that it was only wide enough for the body of the Fishes to pass through, and then having fill'd the vessel with some live Gudgeons, and a good Quantity of

water, the neck of it was made to pass through a hole that was left, or made for it in the midst of a metalline plate, or wooden Trencher, which could descend no lower then the neck, because of the inferior part of the glass that would not suffer it, and which serv'd to support a mixture of Ice (or Snow) and Salt, which was appli'd round about the extant neck of the glass. By this contrivance I propos'd to my self a double advantage: the *first*, that, whereas in broad vessels 'tis not always so easie, as one would think to be sure, that the surface of the water is quite frozen over in every part, by this way I could easily satisfie my self, by inverting the glass, and observing, that the ice had so exactly / choak'd up and stopt the neck, that no drop of water could get out, nor any bubble of Air get in, and yet the Fishes had liberty enough to play in the subjacent water. The *other* conveniency was, that, the frigorifick mixture being appli'd to the neck, no water was congeal'd, or extremely refrigerated, but that which was contain'd in the neck, so that there seem'd no cause to suspect, that in case the Fishes, thus debarr'd of Air, should not be able to live in the water, it was rather Cold, then want of Air that kill'd them. But though not having then been able, by reason of a remove, to prosecute these Trials to the utmost, nor to register all the circumstances, I shall not lay much weight upon it, yet I remember, that the included Fishes continued long enough alive, to make me shrowdly suspect the Truth of the vulgar Tradition.

Another time being destitute of the conveniency of such glasses, I caus'd some of the same kind of Fishes to be put into a broad and flat earthen vessel, with not much more water, then / suffic'd perfectly to cover them, and having expos'd them all night to a very intense degree of cold, I found the next morning, that some hours after day, they were alive, and seem'd not to have been much prejudiced by the cold, or exclusion of Air. 'Tis true, that there was a very large moveable bubble under the ice, but that seem'd to have been generated by the Air, or some Analogous substance, emitted out of the Gills or bodies of the Fishes themselves: for, that the surface of the water was exactly frozen over (which does not in such Trials happen so often, as one would think) I found, by being able to hold the vessel quite inverted, without losing one drop of water. And that this large bubble might possibly proceed from the Fishes themselves, I was induc'd to suspect, because having at different seasons of the year, for divers purposes kept several sorts of Fishes, and particularly Gudgeons, for many days in glass vessels, to satisfie my self about some *Phænomena* I had a mind to observe, I have often by / watching them, seen them lift up their mouthes above the surface of the water, and seem to gape and take in Air, and afterwards let go under water out of their mouthes and gills divers bubbles, which seem'd to be portions of the Air they had taken in, perhaps a little alter'd in their bodies. And particularly in Lampries (of which odd sort of Fishes I elsewhere make mention) I have with pleasure, both observ'd and show'd to ingenious men, that being taken out of the water into the Air, and then held under water again, they very manifestly appear'd to squeez out, and that not without some force, at those several little

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holes, which are commonly mistaken for their eyes, numerous and conspicuous bubbles of Air, which they seem'd to have taken in at their mouthes, if not also at those holes. But of these matters a fitter occasion may perhaps invite me to say more. To return now to our Gudgeons, I shall add, that to satisfie my self further, what cold and want of Air they may be brought to support, I expos'd a couple of / them in a bason, to an exceeding bitter night, and though the next day I found the ice frozen in the vessel to a great thickness, and one of the Fishes frozen up in it, there remaining a little water unfrozen, the other Fish appear'd through the ice to move to and fro, and the ice being afterwards partly thaw'd, and partly broken, not only that Fish was found lively enough, but the other, which I alone judg'd not to be quite dead, though, when the ice was broke, it lay moveless, did in a few minutes so far recover, as to tow after it (if I may so speak) a good piece, into which his tail remain'd yet inserted; and though one of these, and some other Gudgeons, that had been already weakned by long keeping, were once more expos'd in the Bason to the frost, and suffer'd to lye there, till they were frozen up, yet the ice being broken, in which they were inclos'd, though their bodies were stiff and crooked, and seem'd to be stark dead, lying in the water with their bellies upwards, yet one of them quickly recovered, and the other / not very long after began to show manifest signs of life, though he could not in many hours after so far recover, as to swim with his back upwards. 'Tis true, that these Fishes did not long survive, but of that, two or three, not improbable reasons, might be given, if it were worth while to name here any other then this, that the ice, they had been frozen up in, or the violence that was offer'd them by the fragments of it, when it was broken, had wounded them, as was manifest enough by some hurts, that appear'd upon their bodies; yet some other Gudgeons were irrecoverably frozen to death, by being kept inclos'd in ice, during (if I misremember not the time) three days. And as for other Animals, I caus'd a couple of Frogs to be artificially frozen in a wide mouth'd glass, furnish'd with a convenient quantity of water, but though they seem'd at first inclos'd in ice, yet looking nearer, I found, that about each of them there remain'd a little turbid liquor unfrozen, as if it had been kept so by some expirations from their bodies. / Wherefore causing either the same, or two others, (for I do not punctually remember that circumstance) to be carefully frozen, and for a considerable while, I found, that notwithstanding the ice, into which most part of the water was reduc'd, not only one of them before the ice was broken appear'd to be perfectly alive, but the other that was moveless and stiff, and lying with the belly upwards in a Bason of cold water, whereinto it was cast, did in a very few minutes begin to swim about in it. I should have made more Trials at least, if not also more satisfactory ones, if I could have had Fishes and vessels, and cold weather at command: But upon the whole matter, though the



Tradition, we have been examining, may perhaps have some thing of truth in it, yet it seems to deserve to be further inquired into, both in reference to the truth of the matter of fact, *the death of Fishes in frozen Ponds and Rivers*, and in reference to the cause, whereto that effect is imputed.

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I met with an odd passage in Captain / *James's* voyage, which if it had been circumstantially enough set down, might prove of moment in reference to the weight of bodies frozen and unfrozen, and therefore though I would not build any thing on it, yet I shall not omit it. *The ninth* (says he<sup>†@</sup>) *we hoisted out our Beer and Cydar, and made a Raft of it, fastning it to our shore-Anchor. The Beer and Cydar sunck presently to the ground, which was nothing strange to us, for that any wood or pipe-staves, that had layen under the ice all Winter, would also sinck down so soon, as ever it was heav'd over board.*

About the duration of ice I forgot, through hast, to add a relation of Capt. *James*, whereby it may appear, That though Wine abounds with very spirituous and nimble parts, whence it resists congelation far more then water, yet if even this liquor came once to be congeal'd, the ice made of it may be very durable. For he sets down in his Journal, that when he came to his Ship again, he found a But of Wine, that had been all the Winter in the upper deck, / *to continue as yet all firm frozen, though it were then the moneth of May.*<sup>†@</sup>

When I treated of the great proportion in some pieces of ice, that were aground, instead of taking notice of the great piece of ice mention'd by *Gerard de Veer*, to be 52. fathom deep, the passage that was to be transcrib'd, was this other, hard by, which contains two examples of towers of ice, where the extant part reach'd upwards more then half as much as the immersed part reach'd downwards.

*We saw* (says he<sup>†@</sup>) *another great piece of ice not far from us, lying fast in the Sea, that was as sharp above, as if it had been a Tower, whereunto we rowed, and casting out our lead, we found that it lay 20. fathom fast on the ground under the water, and 12. fathom above the water. -- We rowed to another piece of ice, and cast out our Lead, and found that it lay 18. fathom deep, fast on the ground under the water, and 10. fathom above the water.*

That snow lying long, and too long on the ground, does much conduce to the fertilizing of it, is a common observation of our Husbandmen. / And *Bartholinus* in his Treatise of the *use of Snow*,<sup>†\*</sup> brings several passages out of Authors to make it good: to which I shall add the testimony of our learned English Ambassador, Dr. *Fletcher*, who speaking of the fruitfulness of the soil, and hasty growth of many things in the great Empire of *Russia*, gives this account of it.<sup>†@</sup>

*This fresh and speedy growth of the Spring there, seemeth to proceed from the benefit of the snow, which all the Winter time being spread over the whole Country, as a white robe, and keeping it warm from the rigour of the frost, in the Spring time (when the Sun waxeth warm, and dissolveth it into water) doth so thoroughly drench and soak the ground, that it is somewhat of a slight and sandy mold, and then shineth so hotly upon*

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*it again, that it draweth the herbs and plants forth in great plenty, and variety, in a very short time.*

As we made some Trials to discover, whether congelation would destroy or considerably alter the *odors* of bodies, so we had the like curiosity in reference to divers other qualities, / not only those that are reputed manifest, as colours and tastes, the latter of which we sometimes found to be notably chang'd for the worse in flesh congeal'd, but also those that are wont to be call'd *occult*, and among the qualities of this sort, I had particularly a mind to try, whether the purging faculty of Catharticks would be advanc'd or impair'd, or destroy'd by congelation, and for this purpose I caus'd to be expos'd thereunto divers purging liquors, some of a more benigne, and some of a brisker

nature, and that in differing forms, as of syrup, decoction, infusion, &c. But for want of opportunity, to try upon the bodies of animals, what change the cold had made in the purging liquors, it had congeal'd, I was unable to give my self an account of the success of such Experiments; only since, in some of these Trials I had a care to make use of Cathartick liquors prepar'd by fermentation, (which way of preparing them, is it self a thing, I elsewhere take notice of, as not unworthy to be prosecuted.) I shall add on / this occasion, that fermentation is so noble and important a subject, that the influence of cold upon it may deserve a particular inquiry. And I am invited to think, that the influence may be very considerable, *partly* by my having observ'd (upon a Trial purposely made) both that Raisins and water, (with which I was us'd to make Artificial Wines) did not in many days, whilst the weather was very frosty, so much as manifestly begin to ferment, though the water were kept fluid; and *partly* by my having observ'd, that Beer will continue as it were new, and be kept from being, as they call it, ready to drink much longer then one would readily suspect, if very frosty weather supervene, before it have quite finished its fermentation, insomuch, that an experienc'd person, of whom I afterwards inquir'd about this matter, assur'd me, that Beer not duly ripe, would not sometimes in five or six weeks of very frosty weather, be brought to be as ripe as in one week of warm and friendly weather. But we have a nobler instance to our present / purpose, if that be true which I learn'd from an intelligent French-man, whom I consulted about this matter. For according to this experienc'd person, the way to keep Wine in the Must (in which state its sweetness makes it desir'd by many) is to take newly express'd juice of Grapes, and having turn'd it up before it begins to work, to let down the vessels (which ought to be very carefully clos'd) to the bottom of some deep Well or River, for six or eight weeks, during which time the liquor will be so well settled (if I may so speak) in the constitution, it has so long obtain'd, that afterwards it may be kept in almost the same state, and for divers moneths continue a sweet, and not yet fermented liquor, which some, in imitation of the French and Latins, call in one word, *Must*. And how by the help of Cold well appli'd, some other juices, that are wont to work early, and to be thereby soon spoil'd, may be long kept from working, the Reader may perchance learn in another Treatise, to which such matters more properly belong. /

'Tis known, that the Schools define cold by the property, they ascribe to it, of congregating both Heterogeneous and Homogeneous things. I thought it not amiss to attempt the making some separations in bodies by the force of Cold. For if that hold true in this climate, which has been observ'd by Travellers and Navigators in Northern Regions; that men may obtain from Beer and Wine a very strong spirit, and a phlegme by congelation, it seems probable, that in *divers* other liquors the waterish part will begin to freez before the more spirituous and saline, and if so, we may be assisted to make divers separations, as well by cold, as by heat, and dephlegme, if I may so speak, some liquors, as well by congelation as by distillation: but I doubt, whether the ordinary frosts of this Countrey can produce a degree of cold great enough to make such divisions and separations in bodies, as have been observ'd in the more Northern Climates. For though having purposely hung out a glass-bottle with a quart of Beer in it, / in an extraordinarily sharp night, I found the next morning, that much the greatest part of the Beer being turn'd into ice, there remain'd somewhat nearer the middle, but nearer the bottom, an uncongeal'd liquor, which to me and others seem'd stronger then the Beer, and was at least manifestly stronger then the thaw'd ice, which made but a spiritless, and, as it were, but a dead drink; yet in some other Trials my success was not so considerable as some would have expected. For having put one part of high rectifi'd spirit of Wine, to about five or six parts, if I misremember not, of common water, and having put them into a round glass, and plac'd that in beaten ice and salt, though the mixture were in great part turn'd into ice; yet I could not perceive, that even two liquors so slightly mingled, were any thing accurately severed from one another, although once, to enable my self the better to judge of it, the spirit of Wine I imploy'd was beforehand deeply tinted with Cochinele, and therefore I the less wonder, / that in Claret Wine I could not make any exact separation of the red and the colourless parts: However I thought it not amiss to try, how far in some other liquors this way of separating the waterish, and more easily congealable part from the rest, would or would not succeed. And I remember, that a

large glass vessel, wherein spirit of Vinegre was exposed to the cold, a considerable part was turned into ice, whose swimming argued it to be lighter then the rest of the liquor: but though I put some of this ice in a glass by it self, to examine by its weight and taste, when thaw'd, how much it differ'd from the uncongeal'd part of the spirit, my hopes were disappointed by a misfortune, which was not repaired by my exposing afterwards a smaller quantity of spirit of Vinegre to the Nocturnal Air, for that proved so cold, that the whole was turned into ice, wherefore I must reserve for another opportunity the prosecuting that Experiment, as also the trying, whether a separation of the Serous or the Oleaginous parts of Milk / may be effected. For though once the frost seem'd to have promoted a separation of Creme, notwithstanding that heat also may do it, and though another time there seem'd to be another kind of divulsion of parts made by congelation; yet for want of leisure to prosecute such Trials, they prov'd not satisfactory, no more then did some attempts of

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the like nature, that I made upon blood by freezing it.

But notwithstanding these discouragements, I resolv'd to try, what I could do upon Brine. For calling to mind the Relations mentioned in the XV. Title, and elsewhere, which seem to argue, that in some cases the ice of the Sea-water may, being thaw'd, yield fresh water, and being the more inclin'd to think it worth Trial, by a Physician, I since happened to discourse with about this matter, who affirm'd to me, that sailing along the coast of *Germany*, he had taken out of the Sea ice, that being thaw'd, he found to afford good fresh water,<sup>†\*</sup> I began to consider, whether we might not by cold, free salt water at some seasons of the year, / from a great deal of the phlegme, which 'tis wont to cost much to free them from by fire, and other means. For a little help towards the diminution of the fresh water, is look'd upon as so useful an Experiment, by many that boil salt out of the salt springs, that in some Countries, that are thought the skilfullest in that trade, they make their salt-water fall upon great bundles of small brush-wood, that being thereby divided, and reduc'd to a far greater superficies, there may, in falling through, some of the purely Aqueous parts exhale away; wherefore dissolving one part of common salt in 44. times its weight of common water, that it might be reduc'd, either exactly, or near, to the degree of saltness, that has been by several writers observed in the water of our neighbouring Seas, and having likewise caus'd another and much stronger Brine to be made, by putting in to the water a far greater proportion of salt, (for so there is in many of our salt springs) we expos'd these several solutions to the congealing cold of the Air in frosty / weather, where the last mention'd solution being too strongly impregnated with the salt, continued some days and nights altogether uncongeal'd; but that weaker solution, which emulated Sea water, being expos'd in a shallow and wide mouth'd vessel (that shape being judg'd the most proper we could procure for our design) the large superficies, that was expos'd to the Air, did, as we expected, afford us a cake of ice, which being taken off, and the rest of the liquor expos'd again to the Air in the same vessel, we obtain'd a second cake of ice, and taking the remaining, which seem'd to be indispos'd enough to congelation, we found, that by comparing it with that, which was afforded us by the first cake of ice permitted to thaw, there appear'd a very manifest difference betwixt the water, whereinto the ice was resolv'd, scarce tasting so much as brackish, whereas the liquor, that had continued uncongeal'd, was considerably salt in taste. And if I had had the conveniency of examining my self these two liquors *Hydrostatically*, as I / was fain to have them examin'd by another, I doubt not but by their weight, I should have discovered precisely enough the difference between them (which the person I employ'd found to be *considerable*) and consequently should have been assisted to make an estimate of the advantage, that might be afforded by the operation of the cold towards the freezing of the Brine from its superfluous water. But though I had not a quantity of ice great enough to satisfie me, whether that little brackishness of

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taste, I have mention'd, proceeded from some saline Corpuscles, that concurr'd to the constituting of the ice it self, or did only adhere to the lower part of it, among other particles of the liquor, that remain'd uncongeal'd, yet perhaps 'twere not amiss to try, whether in very large, though not deep

vessels, this Experiment, especially promoted by some expedients, that practise may suggest, may not in some seasons and places, be brought to be of some advantage.

Whilst I was endeavouring by / some of the above recited Experiments, to make some separations in liquors by congelation, I thought fit to try by the same means, what separations I could make in some bodies, betwixt liquors, and those more stable parts among which they were ingag'd, hoping, upon considerations, which 'twere too long to enumerate, that, if such attempts should succeed, they might afford hints of a *Luciferous* nature. I took then divers vegetable substances of differing kinds, as Turnips, Carrets, Beets, Apples, and tender wood, freshly cut off from growing trees, as also divers Animal substances, as Musculous flesh, Livers, Brains, Eyes, Tongues, and other parts, and expos'd them to a very sharp cold, that they might be thoroughly frozen. Now one of the chief things, that I propos'd to my self in this attempt, was, to try how far I could by congelation make discovery of any thing about the Texture of Animals and Plants, that had not been taken notice of by *Anatomists* themselves, and would scarce otherwise be render'd visible. And / I easily found, that I had not groundlessly imagin'd, that in divers Succulent bodies, both vegetable and animal, the sap or the juice, that was so dispers'd among the other parts, and divided into such minute portions, as not to be manifestly enough discriminated, might by congelation be both discern'd and separated from the rest. For in divers Plants, I found the Alimental juice to be congeal'd into vast multitudes of distinct Corpuscles of ice; some of which, when the bodies were transversly cut with a sharp knife, and left a while in the Air, might be wip'd or scrap'd off from the superficies of the body, upon which 'twould after a while appear in the form of an Efflorescence, almost like meal: but in others I took a better and quicker course, for by warily compressing the frozen bodies, I could presently make the icy Corpuscles start in vast numbers out of their little holes, and though some of these were so minute as to invite me to use a *Microscope*, that magnifi'd a little, (not having then any of my best at hand) yet in some bodies, / and especially in Carrets and Beets, the icy Corpuscles were big enough to be distinctly or apart conspicuous, insomuch, that I was not mistaken in hoping, that the *figures*, as well as *sizes* (for as to the *Colour* it was scarce discernible in the ice, produc'd in so deeply crimson a Root, as the Beet it self) of these little pieces of ice, might be guess'd at by the bigness and shape of the Pores, that were left in the more stable part, or (if I may so call it) the *Parenchyma* of the root, though in making an estimate of these Cavities, as well as in discovering the order, wherein they are rang'd, I found it useful to cut the frozen roots, sometimes according to their length, and sometimes quite cross. For by that means there would appear in Carrets, for example, of

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the larger sort, a great disparity in the order of the Pores, which, when the root was divided by a plain parallel to the *Basis*, appear'd plac'd in lines almost streight, tending almost like the spoaks of a wheel, from the middle to the circumference. But if the Carret were slit from one end towards / the other, the icy Corpuscles and pores would seem rang'd in an order, that would appear very differing, but which I have not now the leisure to describe, no more then what I observed with a Microscope, about the ice and pores of Apples, the Tongues of Animals, Chips of green and sappy wood, &c. expos'd to congelation: only this I shall not pretermit, That as I many years since made (and, as I now find, too freely communicated) an Experiment, (mention'd long after in other papers<sup>†@</sup>) of freezing the eyes of Oxen, and other Animals, whereby the soft and the fluid humors of that admirable organ may be so hardned, as to become tractable, even to unskilful Dissectors: So I did on this occasion apply that Experiment to the brains of Animals, which though too soft to be easily dissected, especially by those that are not dexterous, may by congelation be made very manageable by them: And besides, that in dissecting the hardned brain, it sometimes seem'd, that the knife did cut through multitudes of icy Corpuscles / (as when one cuts a frozen Apple) the substance of the brain seem'd also to the eye to be stuffed with them, and the Ventricles of it did at least conspicuously *harbour* pieces of ice, if it were not *fill'd* up with them; and the manifest difference of Texture, that there is between the white and yelk of a throughly frozen Egg, and also betwixt the Crystalline and the Aqueous, and the Vitreous humors of the eye, wherein by



congelation the Crystalline alone loses its transparency, but acquires no conspicuous ice, whilst the others are full of ice, and that diaphanous, these and such like disparities, I say, may invite one to hope, that some things may by congealing of bodies, be discovered about their Texture, that may afford sagacious *Anatomists* improvable hints.

I know not, whether it will be thought worth while to take notice, That neither an Eye, nor a Liver, nor a lean piece of flesh, nor a live Fish, nor a living Frog, being frozen and put into cold water, was observ'd to be upon its thawing cas'd with ice, / as frozen Eggs and Apples are wont to be: because having forgot to make the Experiment above once, I dare not much rely on it; but whereas we have formerly observ'd, that congelation does most commonly spoil, or at least impair Eggs, and Apples, and Flesh, and many other bodies, I think it may not be unworthy to be consider'd, how far, and in what cases we may give a Mechanical account of this *Phænomenon*. For though the immersion of frozen bodies in cold water be allowed to thaw them, with less prejudice, then if they were thaw'd hastily by the fire, or suffer'd to thaw themselves in the Air: yet there have been complaints made; That notwithstanding this expedient, several bodies have been much the worse for having been throughly frozen, now since I have lately shown, that in many stable bodies, the Alimental juice is by congelation turn'd into ice, and have formerly evinc'd, that water and aqueous liquors are expanded by congelation, I

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see not why we may not suspect, that the innumerable icy / Corpuscles, into which the Alimental juice is turn'd by the frost, being each of them expanded proportionably to their respective bignesses, may not only prejudice the whole, by having their own constitution impair'd, as has been formerly observ'd in Aligant, and other Vinous liquors, but may upon their expansion crush in some places, and distend in others, the more stable parts, in whose Cavities they were harbour'd, and thereby so vitiate their Texture, as to impair some of their qualities, and dispose the *Compositum* to corruption. How much Contusion may prejudice tender bodies, and accelerate putrefaction, is evident in many fruits, especially the more tender ones, which having been bruised, quickly begin to rot in those parts, that have been injur'd. And 'tis agreeable to what has been formerly shown, to conceive, that in congelation there seems to happen an almost innumerable multitude of little contusions, made by the fluid parts harden'd and expanded by frost, of the formerly more stable parts every where intercepted / between them: And though these icy Corpuscles be but small, yet the sides of that stable matter, that separates them, and which they endeavour to stretch or crush, are oftentimes proportionably thin.

And we have formerly noted, That, besides that Eggs will be burst by having their Alimental juice frozen, both shingles and stones themselves may have their Texture spoil'd by the congelation of the Mineral sap, that is in exceeding minute and insensible particles dispers'd through those bodies: and the violation of the Texture of Plants, Herbs, and Animals, by the expansion of the aqueous and juicy particles, which, though they be not congregated, do abound in them, will be the less wonder'd at, if it be remember'd, that our former Trials manifest, that a few ounces of water congeal'd, did not only burst Glass and Pewter vessels, but even the Iron barrel of a Gun.

Whilst I was upon these Trials, I had also a curiosity to know, whether by freezing Animals to death, I could discover any such change in the / qualities or structure of their parts, as might help us to discover, by what means it is, that excessive colds kill men in Northern Countries, since such a discovery might probably be of good use to the People that live in those gelid Regions: But having taken a young Rabbet, as the tenderest and fittest beast, I could then procure for such a Trial, and having expos'd him all night to an extraordinarily bitter frost, without finding him otherwise mischiefed by it, then that one of his legs was swell'd and grown stiff, I was more inclin'd to resign over to others, then to repeat my self what seem'd to be an ill natur'd Experiment, though perhaps it may have much less of cruelty, then one would think, since some of our former observations have made it probable, that oftentimes the extinction of life by cold is a more indolent kind of death, then

almost any other. But in a Rabbet purposely strangled, and presently expos'd intire to a bitter cold, we found ice produc'd in such parts, as would have made us prosecute the Trial, had the want of / such Animals and of leisure not hinder'd us.

It is affirm'd by divers eminent writers, and those modern ones too, that

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water impregnated with the saline parts of Plants, and afterwards frozen, will exhibite in the ice, the shape of the same Plant: And the learned, but I fear, too credulous, *Gaffarel* tells us, that this is no Rarity, being dayly shewn by one *Monsieur de la Clave*.<sup>†\*</sup> But to what we have already publish'd in another Treatise,<sup>†@</sup> to shew, that this Experiment as it is wont to be deliver'd, is either *untrue*, or very *contingent*; we shall need but to add, that, since the Experiments there mention'd, we did again lately try, what could be done with Decoctions, that were richly imbu'd, and highly ting'd with the spirituous parts of the Vegetables; but this ice was by no means so figur'd as the Patrons of the Tradition promise: And I remember, that having also made, for curiosity sake, a *Lixivium* with 16. parts of water, and but one of salt of Potashes, that the mixture might be sure to freez, / and having expos'd the liquor in a thin glass vial to an exceeding cold Air, we found the copious ice produc'd, to lye on the top in little sticks, not unlike those Prismatical bodies, wherein Salt-petre is wont to roch, and those parts of this ice, that were beneath the water, were shot in thin parallel plates, exceeding numerous, but (as one of our notes expressly informs us) no way in the shape of Trees, by whose Incineration nevertheless *Polonian* Potashes, (as eye witnesses, that deal in them, inform me) are made.

Long after the making of the newly recited Experiment, I chanc'd to find, that the learned *Bartholinus* in the Treatise, we have often had occasion to take notice of, says, That the water, wherein Cabbage has been decocted, will, when frozen, represent a Cabbage, the vegetable spirits being, as he supposes, concentrated by the cold.<sup>†@</sup> How well this Experiment may succeed, / when made in a cold Countrey like his, I do not know: but not having my self, when I first took notice of it, the opportunity to try it satisfactorily by help of a frosty night, all I could do, was, to take a good decoction of Cabbage, and filtrate it through Cap paper, that it might be, though yellow, yet clear, and then by the circumposition of our frigorifick mixture, we froze this liquor in a thin glass vial, but the ice did not, either to me or others, appear to have any thing in it like a Cabbage, or remarkably differing from other ice. And being afterwards befriended with two or three frosty nights, we expos'd a decoction of Cabbage, to be congeal'd by the Nocturnal Air alone, without the help of Art; but neither this way did the Experiment succeed well. And though once a few ounces of the decoction being lightly frozen in a vial, there appeared in the thin ice, that adher'd to the inside of the glass, a figure

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not so very unlike that of a Cabbage leaf, but that some such accident may have invited our learned Author to think, / that the representations of Cabbages would constantly appear in their frozen decoctions, yet I was inclin'd to think this figuration rather casual, by the curiosity I have had to freez the decoctions of several Herbs, some of them spirituous enough, as Rosemary, and Penny-royal, without being able to find in the ice, I obtain'd from them, any conviction of the truth of the Tradition we are examining. And I have lately had more then once, by freezing fair water alone, after a certain manner, ice, that seem'd much more to exhibite the shapes of vegetables, then any decoctions of them, that I have made. And particularly I found more then once, that by putting hot water into a somewhat slender Cylinder of glass, and agitating it in a frigorifick mixture, consisting of beaten ice, salt, and water, so that it was very speedily frozen thereby, it was congeal'd into an ice much more regularly and prettily figur'd, then I have seen it in divers of the waters impregnated with the fix'd salts of Plants, though of *these* we are told such wonders. /

Such particulars as these joyn'd with what I have elsewhere observ'd to the same purpose, make me, I confess, somewhat surpriz'd to meet in *Berigardus*'s forecited discourse upon *Aristotles* Meteors, such a passage as this;†@ *Paucis notum est, cur intra glaciem cernuntur interdum multiformes stirpium imagines in Ampullis vitreis, aquæ superficie tenuis congelatæ plenæ. Hoc autem fit injecto in Phialas sale diversarum stirpium, nam ubi erit sal alicujus plantæ & Artemisiæ, in suo Lixivio glacies adhærens vitro, refert ejus folia laciniosa: similiter in alia Phiala videbuntur folia Plantæ, cujus sal in suam aquam fuerit injectus. Et nè quis hoc fortuito cadere putet, in aquâ sæpius solutâ, & congelatâ eadem imagines semper occurrent, ut vel ex eo dixeris multiplicem spiritum salis in principiis naturalibus esse ponendum.* Thus far this Author, who would have done well, if he had been so much more lucky, than other men, as to have performed these things, to assure expressly of his having done so, those many ingenious men, that much distrust the relations of those Chymists, that are not of / the best sort: and 'tis of such suspicious Authors, that I here declare once for all, that I would have the Reader understand all the passages of this Book, wherein I may seem to say any thing (for avoiding of tediousness) indefinitely to the disparagement of Chymists. And in case he had not tri'd them, he should, in gratitude to the Authors of them, have told us, he had, what he delivers of them, but from others, and not have authoriz'd the untri'd reports of writers, not always too veracious by his building Theories upon them. And as for what he immediately subjoyns, and seems to rely on, out of *Quercitan*, (and other *Spagyrical* writers, who possibly had it themselves from *him*) about the seminal virtues

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surviving in the Ashes of burnt plants;†\* though I will not here examine, or absolutely reject the opinion, because the discussion of it belongs to another place, yet as to the Experiment whereon *Berigardus* and others relye, namely, that the *Lixiviums* made of the Ashes of Plants, will exhibite, being congealed, the / figures of the pristine vegetables; besides that a general conclusion, as to other Plants, seems to be inferred from what happened in *Netles* only, I much doubt, whether that famous Experiment it self of the frozen *Lixivium* of *Netles*, were more then casual, if it were not also assisted by an indulgent phansie. For having, after divers Experiments made with other fixed salts, purposely repair'd, for greater security, to the notedst Chymist in *England*, to obtain from him some fixed salts, very faithfully prepared, and intimating withal, that 'twas to try such an Experiment (which he was a favourer of) I did by mingling these salts each in a distinct vial, sometimes with one, and sometimes with another proportion of water, and afterwards exposing them to the cold Air, obtain indeed divers portions of ice, but without any such figurations, as the learned *Berigardus* would have expected; though some of these Trials were made more then once, as well with the *Lixivium* of *Netles*, as with the Lees of other Plants: so that I / doubt this Author is more scrupulous in admitting some important truths, in which the best Philosophers, as well Heathen as Christian, agree, then in examining the uncertain Traditions of the Chymists, whose unsatisfactory way of setting down matters of fact, I am induc'd to take notice of his imitating, by finding, that in the very same page (that I have newly cited) he relates another Chymical Experiment in these terms.†\* *Velim porro ostendere mirabili Experimentiâ, quàm sint penetrabiles aliqui spiritus Corporei: Exarentur in charta literæ, aceto albo, quarum nullum vestigium deprehendatur, claudaturque primis foliis Crassissimi alicujus Libri. Paretur alia Charta, quæ inficiatur aqua illa fætida, ubi dilutum fuerit Auripigmentum, & exsiccata claudatur postremis foliis ejusdem Libri leviter compressi, statim videbis in priori charta literas conspicuas, perinde ac si atramento ductæ fuissent.* Now, though some thing like what is here propos'd to be done, may be perform'd, and other *Phænomena* of the Experiment, such as he seems not to have been acquainted / with, may be also exhibited, after the manner I have elsewhere†@ particularly set down, yet he must have good luck, that performs it only by the directions here given by our Author, who by omitting one of

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the chief ingredients, and some requisite circumstances, appears indeed manifestly enough to have *heard* of such an Experiment, but without seeming to have sufficiently *known*, what he pretends to teach (at least as far as his bringing this Experiment as a proof, and the obscure style, he is wont to employ in the little I have yet read of his Book, permits me to judge.)

But to return to the figurations of ice, notwithstanding such unsuccessful Trials about them, as I have been mentioning, I will not deny it to be possible, that a prepossessed and favourable spectator may think himself to have discern'd in the ice, the figures he look'd for there. For since the writing of the Essay not long since quoted,<sup>†@</sup> we have found, that several Bodies, and even Sea-salt, and Allume, to whom Nature has / given their own determinate figures, have, when dissolv'd in water, concurred with it to exhibite an ice very oddly, as well as prettily figur'd (nor will I presume to determine the utmost, that a lucky observer may *sometimes* meet with in this kind) but to name at present no other Arguments, the figures this way produc'd, were too various and extravagant not to be referred to chance, and not to afford instances how much That can perform in the exhibiting of such Apparitions.<sup>†\*</sup>

On which occasion I shall add, that I remember, I once show'd at the *Royal Society*, a glass head, whose inside was lin'd with a certain substance, that passed for saline, fashioned into the figures of Trees, as curious, as if they had been drawn by a Limner;<sup>†\*</sup> and yet as I produc'd these figures only by rectifying common oyl of Turpentine, from Sea-salt (which salt I elsewhere shew not to be *necessary*) in a certain degree of heat: so by varying that degree of heat, I could make the ascending steams settle in other figures; and I / can easily produce very pretty shapes of Trees, by distillation of that, which belongs not to the *vegetable*, but the *animal* Kingdom. And to these I could add divers other instances of the like tendency, to make it still the more probable, that though oftentimes one may happen to find pretty *Idæas*'s, or Apparitions in ice; yet the like, or as fine, may be produc'd by chance. And I

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have sometimes obtained by freezing Infusions, Decoctions, Spirits, Solutions, and other Liquors, as Vinegre (and particularly) Milk, and even common water, figures, that were so pretty, but withal so unconstantly produc'd, and so easily variable by circumstances, that as it would fill a Book particularly to describe them (which for that reason I hope to be excus'd for declining) so they would much delude him, that should expect to find them *every time* the same, that he had found them *once*.

And to intimate That by the by, to make several Trials in a short time, and thereby produce variety of figures, 'tis not an ill expedient to expose / the liquor, one would have congeal'd, in very shallow vessels, or if it be put into other vessels, to leave it, but of very little depth. And if the vessel it self be highly refrigerated, either by the cold Air, or by having salt and ice applied to the outside of it, the congelation may succeed much the more nimbly; so that within a short while the same liquor, being divers times thaw'd and frozen again, may possibly exhibit variety of figures. And the production of ice may be also much accelerated, by dipping into the liquor, one would have congealed, the convex surface of some glass or other smooth body, that will not imbibe water; for thereby the depth of the liquor will be exceedingly extenuated, and how much such a thinness or want of depth, may dispose a liquor to be speedily penetrated and congealed by the cold, may be guessed, by what is above delivered in the Section out of *Olearius*,<sup>†\*</sup> of the way of multiplying ice in *Persia*, by making water thinly diffuse it self over a plate of ice, or some other aptly figured, / and very cold body: In confirmation whereof I will add on this occasion, that I have seen a pair of Stairs, on which, though they were situated near to three Chimneys, commonly furnished with fire, almost all the day long, the water that was employed to wash them, being thinly spread with a Mop, would presently congeal (though they assur'd me it was hot, when 'twas begun to be laid on) and cover the Stairs with glossy filmes of ice. And I have likewise observed in a very sharp night, that



the water which dropp'd down from the nose of a Pump, was so well congealed, as 'twas sliding away, that the ice thus arrested in its passage (in which 'twill easily be granted that it spreads it self very thinly) had rais'd a kind of icy pyramid of a considerable bigness and height.

I forgot to mention in due places (and therefore think fit to take notice of it here) that when I was considering of the ways, whereby it might be manifested, to those that want nice scales, or distrust their skill to use / them, whence that ice comes, that appears on the outside of frozen Eggs put to thaw in cold water, I found it somewhat difficult to pitch upon such a liquor as I desir'd. For if common water be the liquor imploy'd, it may be said, that it affords the matter, whereof the ice in question is made: and if I imploy'd liquors, that were spirituous or saline, it might be pretended, that the frost (as they speak) did indeed come out of the frozen Egg, though the shell did not appear cas'd with ice, because as fast as the frost came to the outside of the

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Egg, it was resolv'd by the spirituous or saline Corpuscles of the liquor: wherefore as an expedient, I resolved to make use of oyl of Turpentine, as a liquor, which I had found incongealable by the greatest cold, I had observed in our Climate, and which yet (as may appear by the third Paragraph of the XVI. Title<sup>†\*</sup>) was more indispos'd, then common water it self, to thaw any icy Efflorescence, that might be emitted by the Egg. But the Experiment was tri'd, without uniformity in the successes. / For the first time I put a frozen Egg into oyl of Turpentine, I did not observe, that any ice was produced on the outside: neither was the event differing, when another time I put two frozen Eggs together into a small vessel full of that oyl, though to refrigerate the liquor, the vessel was for a while placed upon a mixture of salt and ice, and though also the Egg-shells at their gaping cracks (produced by congelation) discovered, that the contained liquor was well frozen. I intended to prosecute the Experiment another time (wanting ice to do it then) because that once, when during the Trial I was hindred from watching it, one of my Domesticks, whom I ordered to look after it, assured me, that the Egg, that was put to thaw in the oyl of Turpentine, had there obtained ice on the outside of it, which I should readily have believed, upon the score of a like observation, I had made my self, in two Eggs that were frozen to the bottom of the vessel, wherein they had been put to thaw, were it not, that one or both of them had been, by a mistake, dipt / in water, before they were put into the above mentioned oyl.

Some Readers may have expected to find among the examples recited of the influence of cold upon the Air, that strange story, which is related by the learned *Josephus Acosta*, of the mountains of *Pariacaca* (which he several times traversed)<sup>†@</sup> but besides that I have delivered a great part of it already in another Treatise, I was loath to say more, till I had leisure (which I have not now) to discuss the scruples, that I have, not so much about the matter of fact, as about the cause, which perhaps may be something besides cold. But since I have mention'd this XVIII. Section, I will here take notice of what I then intended, but forgot to set down, namely, That to the instances alledged to show, the coldness of regions not to be always proportionate to their greater and less vicinity to the Pole, we may add a memorable one afforded us by a Countrey so well / known to many of us, as *New England*, where, though the Winters are so long and bitter, as we have formerly related out of Mr. *Woods's* Prospect of that Countrey<sup>†\*</sup> (which has been confirm'd to me by an *American* Physician, that liv'd there) yet that Region, which is so very much colder then ours, is in many places no less then a 10. or 11. degrees remoter from the Pole.

I shall add to the same XVIII. Section, that as to the Experiment I there

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mention'd concerning Winds, and which I associate with the testimony of the newly named Mr. *Wood*; I find that the season of the year, and some other circumstances may vary it more, then one would easily have suspected. For though I faithfully recited the *Phænomena*, as I then (and that

sometimes with witness) took notice of them, yet some moneths after, and in other weather, having occasion to repeat the former part of that Experiment, I was somewhat surpriz'd at the success. For coming to blow upon the Ball of a seal'd Weather-glass, which though in its / kind very tender, might be probably presumed to be less so, then a Thermoscope made with a pendulous drop of water (such as that, mention'd in the forecited Paragraph) I found, that if I continued to blow any thing long and briskly, the highly rectified spirit of Wine (which circumstance I therefore name, because possibly the nature of That may somewhat alter the case) would *sometimes* manifestly enough subside. And in that Paragraph of the 18. Title, where I recited the Experiment of the infrigidating Winds, I should more expressly have taken notice of this circumstance, that, to satisfie my self, that 'twas not the bare Wind, as such, whose operation upon the Air included in the Ball of a Weather-glass, made the liquor to ascend, we put a mark upon the height it stood at, when we had a pretty while blown upon it, and then without removing the Bellows, put ice and salt about the Iron pipe of it. By which mixture the Air, that was afterwards blown through that pipe, was so cool'd in its passage, as to / make the liquor very manifestly to ascend, even in a Weather-glass, where I did imploy (as I have elsewhere declared, that I often do) Quicksilver instead of water, or spirit of Wine. And least the vicinity of the frigorifick mixture should be suspected to have caus'd this contraction of the included Air, we did sometimes purposely intermit the moving of the Bellows, without removing the Weather-glass; and though notwithstanding that vicinity, the liquor would begin a little to subside; yet when ever the cold spirits or the Corpuscles of the highly refrigerated Air, were by the playing of the Bellows anew, approach'd to, or rather brought to touch in swarms the globular part of the instrument, the *Mercury* would manifestly ascend. And since we are speaking of Weather-glasses, I shall on this occasion subjoyn, That certain circumstances may also vary the success of another Experiment (somewhat of kin to that lately repeated, about the pendulous Drop) which is briefly mentioned not far from the beginning of / the first Præliminary Discourse.<sup>†\*</sup> For though the common Thermometers, that are here wont to be sold in shops, have usually the *Pipe* of the Bolthead very *large* in proportion to the *Ball*, and therefore are in that place said to be *Weather-glasses not nice*, and though on such instruments in certain Temperatures of the Air (intimated by the word *sometimes*, imploy'd in that passage) the Air blown out of a pair of Bellows against some part of the included Air, would not, especially at the beginning, make the Air sensibly contract it self, and the liquor ascend; though at the very first and second blast, the coldness of this artificial Wind,

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might be very sensible to the touch (*which was the thing intended to be taught in that passage*) yet having the curiosity with other Bellows, at another season of the year, to blow long upon the Ball of a not *common*, but *nice* Weather-glass of my own making, furnished with a pipe, that was very slender, I divers times (but not always) found the tinted liquor manifestly enough to ascend, as if / the Wind, consisting of a more compress'd Air, did by containing a greater number of cold particles in the same room, more affect the internal Air, then the contact of the calm and lax outward Air did before; which disparity of events has given me the design of making further Trials with differing Thermoscopes, at other seasons of the year, to see if I can bring the matter to some certainty, by discovering the cause of this contingency, in which I afterwards suspected, that some light degree of warmth or coolness in the Bellows themselves, which, as being unmanifest to the sense, scap'd unheeded, might have an interest. When I was about some of the former Experiments, I would willingly have had an opportunity of trying, with a good seal'd Weather-glass, what difference there would be, betwixt the cold of the nocturnal Air in a frosty night, in places where the Air was kept calm, by being shelter'd from the wind, not by inhabited buildings, but by some Wall, or other body, whence any warm *Effluvioms* / were least to be expected, and betwixt the cold of the same Air, in places where cold winds, especially Northerly or Easterly did freely and strongly blow. But my occasions then confining me to a Town, I had not conveniency to make any secure observations of that nature; and even in a more commodious place, unless it were determined, whether there be Corpuscles properly and constantly frigorifick, upon whose account some winds are so much colder then others, there may arise more scruples about this matter, then I must now stay to discuss.

There is one thing more, that, it may be, is not impertinent to mention, before I take leave of the XVIII. Title; for in confirmation of what is there delivered, concerning the Vicissitudes of these troublesome degrees of cold and heat, within the compass of the same Natural day, complain'd of by the Patriarch *Jacob*, and by *Olearius*,†\* I shall add, that having since had opportunity to inquire about such matters, of a learned Physician, lately come from the *Indies*, / he assur'd me, that notwithstanding the violent heats of the day, he usually observed the nights to be so very cold, that he was perswaded some positively frigorifick steams, did in the night ascend out of the Earth, and make it very expedient, if not necessary, for those English, that live in the warmer parts of *America*, to imitate the Natives, in keeping fires under their Hammacks, or hanging Beds.

I thought it might be a *Luciferous* Experiment, in relation to an *Hypothesis*, that might be propos'd about cold, to try, whether, if two such liquors were provided, as by being mix'd together, would so far forth lose their fluidity, as to obtain at least the consistence of an Unguent, this impediment put to the

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former confused and greater agitation of their parts, would produce any sensible degree of cold; this I thought fit to try, by immersing for a competent time, the Ball of a tender seal'd Weather-glass, into each of the liquors apart, and then into the soft mixture, their coalition would / compose. To produce such a mixture more ways than one, it was not difficult for me, by the help of some Experiments, I had provided to add to my *History of fluidity and firmness*.†\* But though a strong solution of *Minium* (or calcined Lead) in spirit of Vineger, or a very strong infusion of good quick-Lime in water, will either of them (and one of them I did make use of, though I have forgotten which) coagulate a just proportion of good Sallet Oyl (to name no other made by expression) into such a consistence as I have been speaking of; yet for want of a seal'd *Thermoscope*, tender enough, I cannot now repeat the Experiment, and till I do, I dare not draw any conclusion from it, though, if I much misremember not, when I show'd it an ingenious person, neither he nor I could perceive, that the liquors, by being depriv'd of their fluidity, had acquir'd any thing of coldness discoverable by the seal'd Weather-glass.

It is much controverted among the Curious, whether water be capable of Compression, and divers have of / late inclin'd to the negative, upon observing a want of cogency in the Experiments, that have been brought to evince the affirmative.†\* What Trials and Observations we long since made about this matter, may be met with in some of our other Treatises, wherefore I shall now subjoyn, that having imagin'd, that Cold might afford a hopefuller way, then (for ought I know) any man has us'd, of bringing this controversie to the dicision of an Experiment, I made that attempt, that is mention'd in the XII. Title; in prosecution of which, as soon as I could procure some, though but some of the accommodations, which I long wanted; I made an Experiment, which I shall subjoyn, because, though it be not so considerable, as with better implements I could have made it, yet the way, I chose, has (as I partly intimated elsewhere) these two advantages; that the force imploy'd to compress the Air, is both very great, and very gradually and slowly appli'd; and that the vessel will not, like those that have been hitherto made use of, give / any passage through its pores to water, though violently compress'd.

We took then a Round Ball of glass, furnish'd with a moderately long Pipe, and having fill'd it with water, till the liquor reach'd within some inches of the top, it was *Hermetically* seal'd up, and then the water by a mixture of beaten ice and salt, was made to freez from the bottom upwards, that without breaking the glass, the unfrozen water, by the expansive endeavour of that which was freezing, might be impell'd upwards, and so at once, both compress the Air, and be press'd upon by it, having by this means condens'd the Air, as far as we thought safe to do in a glass, that was not strong, we cropt of the small Apex of the glass, and immediately the compress'd Air flew out

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with a great noise, and that part of the Pipe which was unfill'd with water, was fill'd with smোক, that made it look white, and great store of little bubbles hastily ascended from the lower parts of the water, to the upper (where most of them quickly broke) in such a way, as put / me in mind of what usually happens upon the opening of vessels that contain'd bottled Beer. But that which was principally to be noted, was this, that besides the bubbles or froth, the *water it self* (at least supposing, that no little unheeded bubbles that did not quite emerge, could sensibly contribute to its height) immediately ascended in the Pipe about  $\frac{3}{4}$  of an inch, which (having carefully mark'd the first and second stations, with a Diamond on the outside of the glass) 'twas easie for us to measure.

I have elsewhere propos'd a suspicion,<sup>†</sup> that in the attempts, that had been till then made, to compress water, the condensation (in case there were really any) might perchance proceed from the compression of the Aerial particles, that I have shown to be wont to ly dispers'd in the pores of common water. But though the considerable expansion of water, notwithstanding the breaking of the bubbles in our present Experiment, seems manifestly to argue, that this could be but a concurrent cause (if it had any sensible effect at all) of our *Phænomena*, / yet I dare not absolutely *rely*, even upon an Experiment, that seems so cogent, till I have satisfi'd my self, that no springiness, which I have sometimes suspected, *might* be in the ice, had any interest in the produc'd effect; and that the great pressure of the forcibly condens'd Air, did not make the glass it self stretch or yield. For if it were able to do so, then the parts of the violently distended glass, upon the removal of the forcible pressure of the Air (which must cease upon the breaking of the *Hermetical* seal) returning to their former straitness below, will make the water ascend somewhat higher in the pipe. But though I could not procure glasses, as well very thick, as conveniently shaped, wherewith to examine this suspicion, which I would likewise have tri'd by the bulk of the glass in water, before and after the letting out of the compress'd Air; yet because most Readers will probably think so much caution more then necessary, I shall add, that if I had not wanted conveniencies, and had not had mischances, the Experiment would in likelihood / have been advanc'd, especially care being taken, that the Air left in the pipe should be well refrigerated before its being seal'd up (as we sometimes did by ice and salt, applied in a perforated Box to the outside) lest part of its spring should depend upon an evanid degree of heat, upon which account the pipe ought beforehand to be drawn so slender, that the glass may be melted together in a trice. For though for want of strong glasses, & the best sort of instruments to seal up such with, the success was not still so considerable as I hop'd for; yet as 4. or 5. other Trials, made, as well with another liquor, as with water, did exhibit a manifest intumescence of the liquors (without computing the froth produc'd at the top;) so in the Experiment lately mention'd, if we had judg'd them strong enough to indure such a compression of the included Air, as we have often made on other occasions, the effect would probably have been much more considerable: For though the difference betwixt the length of the

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same water compress'd and uncompress'd, amounted to an / Aqueous Cylinder of  $\frac{3}{4}$ <sup>†\*</sup> of an inch in height, yet the Air, that made this compression of the water, was it self reduc'd but from 8. inches to 5. so that it took up above half its former room, whereas we have sometimes reduc'd it to an 18. or 20. part thereof. If I had been accommodated with one of my Pneumatical Engines, I should have tri'd, whether water being first carefully freed from the latitant Air in the exhausted Receiver, and then compress'd after the manner hitherto recited, the event of the Trial would have been considerably varied.

I might add as other *Phænomena* of our Experiment, that when we broke off the seal'd Apex of the glass, before the included Air was much compress'd, there neither would be any great noise made, nor any considerable froth produc'd, at the top of the water, and that having had the curiosity to repeat the Experiment in one of the same glasses, that had been already us'd, and with the same water, that had been already compress'd in it, we found, that upon the breaking / off the *Hermetical* seal the second time, the water did nevertheless ascend in the Pipe betwixt  $\frac{1}{8}$  and  $\frac{1}{4}$  part of an inch. And to these particulars I could both *add* other *circumstances*, that I took notice of in the same



Experiment, and *subjoyn* many other *Experiments and Observations*, but that I am already tyr'd. And though I have not found Cold to be a subject over-fruitful in Experiments Pleasing and Curious, yet now I am grown somewhat acquainted with it, I find it may suggest so many *other new* ones, that since the Barrenness of my Theme, will not easily put a period to this Treatise, 'tis fit that now at length I should let my Weariness and want of Leisure do it.

*FINIS.* /  
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## An Examen of Antiperistasis

*An Examen*  
of  
*Antiperistasis*,  
As It is wont to be *Taught* and *Prov'd*.  
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In the original edition, the ensuing section of text is generally preceded by the 'Advertisement' that follows, printed on an inserted signature. The title-page is an editorial addition for the reader's convenience.

In some copies of the book, the Advertisement is bound with the prelims at the start rather than here, and this is followed by Birch. However, in most copies - including the presentation copy to the Royal Society (see above, Introductory Note) - it appears at this point, and this is clearly what was intended, as is shown by the reference to 'the following Dialogue'.

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## An Advertisement.<sup>†\*</sup>

That the Reader may not wonder to find the following Dialogue cited in the *History of Cold*, whereunto nevertheless it is *subjoyn'd*; he is to be inform'd, that a Section About *Antiperistasis* was really both written and transcrib'd before any part of that History was sent to the Press. But finding, that the Accession of new Particulars had so much swell'd it, that 'twas unfit to pass (as I first design'd it should) for one of the Titles of the *History of Cold*, I judg'd it convenient to *sever* it from the rest, upon the score of its bulk, and yet *annex* it to them upon the Account of those many Historical Passages in it, that belong to the same subject, that is handled in those Sections. / The Reader will quickly find, that the Tract consists of two parts, whereof the first (which to allow the more Freedom of Inquiry and Discourse, written in the way of Dialogue,) contains an *Examen* of *Antiperistasis*, without pretending to question it absolutely and indefinitely, but rather, As it is wont to be *Taught* and *Prov'd*. And this Dialogue, for reasons, that it too little concerns the Reader to know, and would take up too much time to tell him, both begins as a Continuation of some former Discourse, and somewhere mentions the Author, as a Third or Absent Person. And to make it the likelier to other Dialogues, the Quotations are not made with the Authors's punctualness in the rest of this Book, but yet with his usual Faithfulness; nor hath his Introducing men Discoursing (as it were by chance) kept him from putting into the Margent the very words of some Passages, which he thought the most important and likely to be distrusted. But though this first Part be entire and finish'd in its kind, and so might very well (if not best) have been / put forth Single, to invalidate the common Doctrine of *Antiperistasis*, (in the sense wherein 'tis there oppos'd;) yet because in Philosophical Matters, 'tis not so much Victory or Applause, that is to be sought, as Truth; I forbore not to subjoyn a Discourse, that may perchance satisfie most of my Readers, some scruples about which I wish'd for further satisfaction and Certainty my self; of the chiefest of which, the Sceptical

## *An Examen of Antiperistasis*

AN  
*Examen of Antiperistasis,*  
AS

It is wont to be *Taught* and *Prov'd*.†\*  
*Themistius, Carneades, Eleutherius*.†\*

### *Themistius.*

1. As for *Antiperistasis*, the Truth of it is a thing so conspicuous, and so generally acknowledg'd, that I cannot imagine what should make some men deny it, except it be, that they find all others to confess it. For though in other cases they are wont to pretend Experience for their quitting the receiv'd Opinions, yet here they quit Experience it self for singularity, and chuse rather to depart from the Testimony of their senses, / then not to depart from the Generality of Men.

2. And to evince, that this is not said *gratis*, I might observe to you, That there are no less then three grand inducements, that have lead both the Vulgar and Philosophers (two sorts of men, that seldom agree in other things) to consent in the acknowledgment of *Antiperistasis*; Authority, Reason, and Experience. But though I think fit to name them all three, yet since the first of them, by having as I just now noted, invited our Adversaries to dissent from the Truth, is a somewhat unlikely *Medium* to prevail on them to acknowledge it, I shall insist only on the two latter, having once declar'd, that I lay aside the first not as worthless in it self, but needless to my cause.

3. To begin then with the Arguments afforded us by Reason.

What can there be more agreeable to the wisdom and goodness of Nature, who designing the Preservation of things, is wont to be careful of fitting them with requisites for that preservation; then to furnish / cold and heat, with that self invigorating power, which each of them may put forth, when 'tis environ'd with its contrary. For the order of the universe requiring, that cold and heat should reside in those Bodies, that often happen to be mingled with one another, those two noble and necessary Qualities, would be too often destroy'd in the particular subjects that harboured them, if provident Nature had not so ordered the matter, that when a Body, wherein either of

them resides, happens to be surrounded by other Bodies, wherein the contrary Quality is predominant, the besieg'd Quality by retiring to the innermost parts of that which it possesses, and there by recollecting its forces, and as it were, animating it self to a vigorous defence, is intended or increased in its degree, and so becomes able to resist an Adversary, that would otherwise easily destroy it.

4. To illustrate as well as supply this Argument drawn from Reason, we shall need but to subjoyn the other afforded us by Experience, which / does almost every day give us not only opportunity to observe, but cause to admire the effects of this self invigorating power, which, when occasionally exerted, we call *Antiperistasis*: And these *Phænomena* ought the more to be acquiesced in, because they may safely be looked upon as genuine *Declarations*, which Nature makes of Her own accord,

and not as confessions extorted from Her by Artificial and compulsory Experiments, when being tortured by Instruments and Engines, as upon so many Racks, she is forced to seem to confess whatever the Tormentors please.

5. To proceed then to the spontaneous *Phænomena* of Nature I was recommending, we see, that whereas in Summer the lowest and highest Regions of the Air are made almost unsufferable to us by their heat, the cold expelled from the earth and water by the Suns scorching beams, retires to the middle Region of the Air, and there defends it self against the heat of the other two, though in the one that Quality be assisted by the almost perpendicular reflection of the / Sun-beams, and in the other it be rendered very considerable by the vastness of the upper Region of the Air, and its Vicinity to the Element of fire.

And as the cold maintains it self in the middle Region, by vertue of the intensness, which it acquires upon the account of *Antiperistasis*; so the Lightning that flashes out of the Clouds, is but a fire produc'd in that midle Region by the hot Exhalations penn'd up, and intended in point of heat by the ambient Cold, to a degree that amounts to ascension.

6. But though these be unquestionably the effects of that excessive coldness; yet we need not go so far as the tops of mountains to fetch proofs of our doctrine, since we may find them at the bottom of our Wells. For though *Carneades* perhaps *will not*, yet the earth as well as the Air *doth* readily acknowledge the power of *Antiperistasis*. And if the reason above alledged did not evince it, our very senses would. For as in Summer, when the Air about us is sultry hot, we find, to our great refreshment, / that the Air in Cellars and Vaults, to which the cold then retreats, is eminent for the opposite Quality; so in Winter when the outward Air freezes the very Lakes and Rivers, where their surfaces are expos'd to it, the internal Air in Vaults and Cellars in Winter, which becomes the sanctuary of heat, as in Summer it was of Cold, is able not only to keep our Bodies from freezing, but to put them into sweats. And not only Wells and Springs upon the account of their resting in, or coming out of the deepest parts of the earth, continue fluid, whilst all the waters, that are contiguous to the Air, are by the excessive cold hardened

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into ice; but the water freshly drawn from such Wells, feels warm, or at least tepid to a mans hand put into it. And as if Nature design'd men should not be able to contradict the doctrine of *Antiperistasis*, without contradicting more then one of their own senses, she has taken care, that oftentimes the water, that is freshly drawn out of the deeper sorts of Wells and Springs, should manifestly, / as I have seen it, smook, as if it had been but lately taken off the fire. And this may be said, without a Metaphor, to demonstrate *ad oculum*, the reality of *Antiperistasis*, there being no other cause to which this warmth can be attributed, then the retiring of the heat from the cold external Air, to the lower parts of the earth and water: since both these Elements themselves being naturally cold, and one of them in the supreme degree, the heat we are mentioning is so far from being likely to be generated in so unfit a place, that if it were not very great, it must be extinguished there, by the coldness of the superior Air, and that of the inferior parts of the Earth.

*Eleutherius.*

7. That *Carneades* may have but one trouble to answer the Allegations to be made in favour of *Antiperistasis*, I hope he will give me leave (according to my custom of siding with either party, as occasion invites me) to add to the familiar Observations mentioned by *Themistius*, some others / that are less obvious. For I franckly confess to you, that when I consider what interest, the unheeded dispositions of our own Bodies may have in the estimates we make of the degrees of cold and heat, in other Bodies; I should not lay much weight upon the *Phænomena*, that are wont to be urg'd as proofs of *Antiperistasis*, if some instances somewhat less lyable to suspicion, did not countenance the doctrine they are urg'd for. I know that *Carneades* being wont so to propose his opinion about

*Antiperistasis*, as only to deny, that it is clearly made out by the reasons or Experiments, that are *commonly* produc'd to evince it, it were somewhat improper to urge him with observations, that are not familiar, and wont to be imploy'd; but I know too, that he is not so rigid an Adversary, as not to allow me to mention some uncommon relations, that I learned from men of good credit. I shall tell you then, that having purposely inquired of ingenious men, that had been very deep under ground, some in Coal-pits, and / some in Mines. One of them affirmed, that at the bottom of the Grove (as they call it) or Pit, he found it very hot in *September*. And another, that he often found it hot enough to be troublesome in Winter. And a third (who is himself a great seeker for Mines, and a Master of considerable ones) that he found it to be hot all the year long. And to manifest, that such Observations will hold even in gelid Regions, I shall repeat to you, what I remember I read in the voyage of that ingenious Navigator, Captain *James*, who giving an account of *Charleton Island*, which by his relation seems to be as cold as *Iceland* it self, says,†@ *That his men found it more mortifying cold to wade through the water in the beginning of June, when the Sea was all full of ice, then in December,*

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*when it was increasing.* And he adds, that which makes more to our present purpose, and proves the other part of the doctrine of *Antiperistasis*; *That from their Well, out of which they had water in December, they had none in July.* And to strengthen the observation yet further, I will acquaint / you with a relation to this purpose not unworthy your notice. For hearing of an ingenious Physician, that liv'd some years in, and about *Musco*,†\* I applied my self to him (as possibly you may have done, for if I mistake not, I have seen you together) to know, whether in that frozen Region he observed the Cellars to be hot in Winter. And his answer to That, and some other Questions of the like nature I put to him, amounted in short to this, That when I enquired, whether their Springs and Wells were not all frozen in the Winter, he told me, that he saw some Springs, whose waters froze not at all near the Spring-head, but, at a good distance from thence, it began to be thinly cas'd over with ice. He added, That his own Well was about six fathoms deep, between the surface of the earth, and that of the water, and that the water in it, was, as I remember, about three or four fathoms deep, and that not only this Well froze not all the Winter, but that the Well of his neighbour, which was but one fathom deep to / the superficies of the water, did not freez neither. And to satisfie my curiosity about the steams of this water, he told me, that when a Bucket of water was newly drawn, if it were agitated, it would smoak. But that from the Well it self, when the water in it was left quiet and unstirred, he did not perceive any smoak to arise.

8. To all this I shall add this further circumstance, that having purposely inquired, whether in the Winter he found it as hot in Cellars at *Musco*, as it is wont to be in that season in ours? He answered me, that when the doors and windows were carefully shut, to hinder the immediate commerce betwixt the included and external Air, he often found, if he stay'd long in his Cellar, it would not only defend him from the sharpness of the *Russian* cold, as bitter as that is wont to be in Winter, but keep him warm enough, to be ready to sweat, though he laid by his Furs. So that if we may rely, either upon the Testimony of our senses, we must necessarily admit Cellars to be / warmer in Winter, then in Summer, and consequently allow an *Antiperistasis*.

9. *Carneades*. Though I were not in haste, I should not think it necessary to reply any thing else to the first part of what was said by *Themistius*, then that, what he alledges of the Universality of the Opinion he maintains, may serve to recommend that which he opposes. For the vulgar Doctrine about *Antiperistasis*, being, as he urges, receiv'd, and taught in all the Schools, the Innovators he declaims against, must have learned it there among the other *Peripatetick* tenents, that youth is wont to be imbued with in those places; so that it may rather seem the love of truth, then of singularity, that engages them against an opinion, which before was their own, as well as that of the generality of Scholars, and consequently against which they cannot maintain a Paradox, that does not imply a Retractation. But I shall not prosecute my

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Answer to *Themistius*'s preamble, / since *Eleutherius*, whom I am chiefly to speak to, is too much a Philosopher to think Truth less her self, for being slenderly attended; or to think any men the less like to be Her followers, because they are but few. To come then directly to the controversie it self, I think I need not tell one of you, that the other mistakes my opinion about it. For I perceive, *Eleutherius* hath not quite forgotten, that I have not been wont to deny an *Antiperistasis*, as it *may be*, but only as it is *wont to be* explicated. But since *Themistius* seems to be willing to have me his Antagonist in this controversie, and since *Eleutherius* himself seems to conspire with him, I am content to act for a while the part, you Gentlemen would have me take upon me, and will propose to you part of what I would say, for the opinion you impute to me, in case I were really of it.

10. To come then to the controversie it self, though *Themistius* has drawn his proofs for the *Antiperistasis* of the Schools, partly from Reason, and partly from Experience; yet the very / same two Topicks seem to me to afford considerations, that may justly warrant our calling it in question.

11. And first, if we look upon the reason of the thing considered abstractedly from the Experiments, that are pretended to evince an *Antiperistasis*, we cannot but think it may be very rational, I say not, to doubt of it, but to reject it. For in the first place, according to the course of Nature, one contrary, ought to destroy, not to corroborate, the other. And next, 'tis a maxime among the *Peripateticks* themselves, That natural causes always act as much as they can. And certainly as to our case, wherein we treat not of living creatures, I cannot but think the Axiom physically demonstrative. For inanimate Agents act not by choice, but by a necessary impulse, and not being endow'd with Understanding and Will, cannot of themselves be able to moderate or to suspend their actions. And as for what *Themistius* alledges, that it was necessary for the Preservation of Cold and Heat, that they should be endowed with such a power of intending / themselves, I must freely confess, that though in living creatures, and especially in the bodies of the perfecter sorts of Animals, I do in divers cases allow arguments drawn from final causes; yet where only inanimate bodies are concern'd, I do not easily suffer my self to be prevail'd upon by such Arguments. Nor is there any danger, that Cold and Heat, whose causes are so radicated in Nature, should be lost out of the World, in case each parcel of matter, that happens to be surrounded with bodies, wherein a contrary quality is predominant, were not endowed with an incomprehensible faculty of self invigoration. And Nature either does not need the help of this imaginary power, or oftentimes has recourse unto it to very little purpose; since we see, that these Qualities subsist in the world, and yet *de facto* the bottles of Water, Wine, and other Liquors, that are carried up and down in the Summer, are regularly warmed by the Ambient Air. And in *Muscovy* and other cold Northern Countries, Men, and other / Animals have oftentimes their Vital Heat destroyed by the cold that surrounds them, being thereby actually frozen to death. And I somewhat wonder, that the followers of *Aristotle* should not take notice of that famous Experiment, which he

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himself delivers, where he teaches, that hot water will sooner congeal then cold.<sup>†\*</sup> For if the matter of fact were true, it would sufficiently manifest, that the heat harboured in the water, is destroyed, not invigorated by the coldness of the Air that surrounds it; so that *Themistius* must, I fear, on this occasion, take sanctuary in my observation, and to keep *Aristotle* from destroying his own opinion, with his own Experiment, had best say, as I do, that it is not true. And though it is not to be denied, that white surrounded with black, or black with white, becomes thereby the more conspicuous; yet 'tis acknowledged, that there is no real increase, or intension of either quality, but only a comparative one in reference to our senses obtain'd by this Collation. Nor does a Pumice-stone grow more / dry, then it was in the fire or earth, by being transferred into the Air or Water, and consequently environed with either of those two fluids, which *Themistius* and his Schools teach us to be moist Elements: neither will you expect to find a piece of dim glass become really more transparent, though one should set it in a frame of Ebony, though that wood be so opacous as to be black. And whereas 'tis commonly alledged, as a proof of the power, Nature has given Bodies of flying their contraries, that drops of water falling upon a Table, will gather themselves into little

globes, to avoid the contrary quality in the Table, and keep themselves from being swallowed up by the dry wood; the cause pretended has no interest in the effect, but little drops of water, where the gravity is not great enough to surmount the action of the ambient fluid, if they meet with small dust upon a Table, they do as they roul along, gather it up, and their surfaces being covered with it, do not immediately touch the board, which else they would stick / to. And to show you, that the Globular figure, which the drops of water, and other Liquors, sometimes acquire, proceeds not from their flying of driness, but either from their being every way press'd, at least almost equally (for in some cases also they are not exactly round) by some ambient fluid, of a disagreeing Nature, or from some other cause differing from that the Schools would give, I shall desire you to take notice, that the drops of water, that swim in Oyl, so as to be surrounded with it, will likewise be Globular; and yet Oyl is a true and moistening liquor, as well as water. And the drops of Quicksilver, though upon a Table they are more disposed, then water, to gather themselves into a round figure; yet that they do it not as humid Bodies, is evident, because Quicksilver broken into drops, will have most of them Globular, not only in Oyl, but in Water. And to show you, that 'tis from the incongruity, it has to certain bodies, that its drops will not stick upon a Table, nor upon some other bodies, but gather / themselves into little sphæres, as if they designed to touch the wooden Plain but in a Point: To manifest this, I say, we need but take notice, that though the same drops will retain the same figure on Stone or Iron, yet they will readily adhere to Gold, and lose their Globulousness upon it, though Gold be a far drier body then Wood, which, as far as distillation can manifest, must have in it store of

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humid parts of several kinds, (I mean both watery and unctuous.)<sup>†@</sup> But this may relish of a digression; my task being only to examine the *Antiperistasis* of cold and heat, concerning which I think I had very just cause to pronounce the vulgar conceit very unconsonant to the nature of inanimate beings. For the *Peripateticks* talk of Cold and Heat surrounded by the opposite quality, as if both of them had an understanding, and foresight, that in case it did not gather up its spirits, and stoutly play its part against the opposite that distresses it, it must infallibly perish: and as if being conscious to its self, of having a power of self / invigoration, at the presence of its Adversary, it were able to encourage it self like the Heroe in the Poet, that said, *Nunc animis opus est, Ænea nunc pectore firmo*,<sup>†\*</sup> which indeed is to transform Physical agents into Moral ones.

12. *Eleuth.* The validity of the *Peripatetick* Argument, drawn from Reason, considered abstractedly from Experience, I shall leave *Themistius* to dispute out with you, at more leisure. And since you well know, that the only Arguments I alledge to countenance *Antiperistasis*, were built upon Experience, as judging them either the best of the only good ones, I long to hear what you will say to the Examples that have been produced of that which you deny.

13. *Carneades.* That, *Eleutherius*, which I have to answer to the examples that are urged, either by the Schools, or by you in favour of *Antiperistasis*, consists of two parts. For *first* I / might show, that as *reason* declares openly against the common Opinion, so there are *Experiments* which favour mine, and which may be opposed to those you have alledged for the contrary doctrine. And *secondly*, I might represent, that of those examples, some are *false*, others *doubtful*; and those that are neither of these two, are *insufficient*, or *capable of being otherwise explicated*, without the help of your *Hypothesis*. But for brevities sake, I shall not manage these two replies apart, but mention, as occasion shall serve, the Experiments, that favour my opinion, among my other answers, to what you have been pleased to urge on the behalf of *Aristotle*.

14. To begin then with that grand Experiment, which I remember a late Champion for *Antiperistasis*, makes his leading Argument to establish it, and which is so generally urged on that occasion, I mean the heating of quick-Lime in cold water. I confess I cannot but admire the Laziness and Credulity of Mankind, which have so long, and generally / acquiesc'd in what they might so easily have found to be false. This I say, because I was possibly the first, that has had both

the curiosity and boldness to examine so general and constant a Tradition; yet I doubt not, that you will soon be brought to take it as well as I, for as great as popular an error. For to let you manifestly see, how little the Incalescence of the quick-Lime needs be allowed to proceed from the coldness of the ambient water, if instead of cold water, you quench it with hot water, the Ebullition of the liquor, will not only be as great, as if the

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water were cold, but oftentimes far greater. As I have sometimes for curiosity removed boiling water from the fire, and when the liquor had left of boiling, but was yet scalding hot, I put into it a convenient quantity of quick-Lime, and after a while, the water, which, as I said, had ceas'd from boiling, began to boil afresh, with so much vehemence, and such large and copious bubbles, that it threatned to run over the Pot, of which, before the effervescence, a considerable / part was left unfill'd. And this was no more then what I might well look for, hot water being much fitter then cold to pervade nimbly the body of the Lime, and hastily dissolve, and set at liberty the igneous and saline parts, wherewith it abounds. And how much a greater interest salts may have in such incalescencies, then Cold, I have also taken pleasure to try, by pouring Acid spirits, and particularly spirit of salt upon good quick-Lime. For by this means there would be a far greater degree of heat excited, then if I had instead of spirit of Salt used common water: And this, whether I imploy'd the spirit cold or hot. For in either case, so small a portion as about the bigness of a Walnut of Lime put into a small glass, would by the addition of a little spirit of Salt put to it by degrees, both hiss, and smoak, and boil very surprizingly, and notwithstanding the small quantity of the matter, would conceive so great a heat, that I was not able to hold the glass in my hand. And to show some friends, how little, heat excited / in quick-Lime by cold water, proceeds barely from the coldness of that liquor; I caus'd a parcel of good Lime to be beaten small, and putting one part of it into a glass vessel, I drench'd it plentifully with oyl of Turpentine, more then it would imbibe, and the other portion of the Lime I likewise drench'd with common water: both these liquors having stood in the same room, that they might be reduc'd by the same Ambient Air, to a like degree of coldness, the event of this Trial was (what I look'd for) that the oyl of Turpentine, notwithstanding its actual coldness, and the great subtilty and piercingness of parts, which it has in common with other Chymical oyls, being of an incongruous Texture seem'd not to make any dissolution of the powdered Lime, and did not, for several hours, that I kept it, produce, that I perceived, any sensible heat in the Lime. Whereas to show, that 'twas not the fault of the Lime, that part of it, on which common water had been poured, did after a little while conceive so strong a / heat, that it broke a large open-mouth'd-glass, into whose bottom it was put, and not only grew so hot, that I could not endure to hold it in my hand, but sent out at the mouth of the glass, though that were considerably distant from the Lime, a copious white fume, so hot, that I could not well suffer the holding of my hand over it. And to prevent a possible, though invalid, objection, which I foresaw might be drawn against the Experiment made with oyl of Turpentine, from the Oleaginous Nature of that liquor; I covered a piece of the same sort of quick-Lime, I have been speaking of, with highly rectified spirit of Wine: but though I left them together all night, yet I perceived not, that the liquor had at all slack'd the Lime, which continued in an intire lump, till upon the substituting of common water, it did, as I remember, quickly appear to be slack'd, since it fell assunder into a kind of

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minute white powder, which was (bating the colour) almost like mud, and would easily by a little shaking be disperst, like it, through the water. /

15. *Eleutherius*. I ingeniously confess to you, *Carneades*, that what you say surprizes me, for I thought it superfluous to try my self, so acknowledged an Experiment, being not able to imagine, that so many learned men for so many Ages, should so unanimously and confidently deliver a matter of fact, of which, if it were not true, the falsity could be so easily discovered.

16. *Carneades*. For my part, *Eleutherius*, I confess I am wont to doubt of what they teach, that seldom or never doubt. And I hope you will forgive me, if having found an assertion so general and uncontroul'd of a falsity so easie to be disprov'd, I be inclinable to suspect the Truth of their other inferior Traditions about *Antiperistasis*; and of these I will mention the two chieftest I have met with among the moderns (for being contriv'd Experiments, I presume you will easily believe they came not from *Aristotle*, nor the Ancienter / Schoolmen that commented upon Him.)

17. The first of these is the freezing a Pot to a Joynt-stool, by a mixture of snow and salt, by the fires side: in which case 'tis pretended, that the fire does so intend the cold, as to enable it to congeal the water, that stagnated upon the surface of the stool, betwixt That and the bottom of the Pot. But how little need there is of *Antiperistasis* in this Experiment, you may guess by this, that I have purposely made it with good success, in a place in which there neither was, nor ever probably had been a fire, the room being destitute of a Chimney. And this Trial of mine I could confirm by divers other Experiments of the like nature, but that this one is sufficient.

18. I proceed therefore to the other Experiment, which is delivered by very learned men, and for whom I have a great respect: according to these, if you take a somewhat large Pot, and having fill'd it almost with snow, place in the middle of the snow a Vial full of water; this / Pot being put over the fire, the coldness of the snow will be so intended by the heat, from which it flies into the water, that it will turn that liquor into ice. But though I several times tri'd this Experiment, yet neither in earthen, nor in silver vessels, could I ever produce the promised ice. And I remember, that an eminently learned man, that wondered to find me so diffident of what he said, he knew to be true, readily undertook to convince me by an Ocular proof, but with no better success then I had had before. So that the Argument may be plausibly enough retorted upon them that urge it.

19. And in case the Trial should succeed sometime or other (for that it doth not ordinarily, I have shown already) yet will there be no necessity of deriving the effects from *Antiperistasis*. For though in such cases the fire would contribute to the production of the effect, by hastening the dissolution of the snow, yet the heat of the fire does but remotely, and by accident cause the production of ice, since other Agents will do the same / thing, that are qualified to make a quick dissolution of the snow, whether they be hot or no; as I have tri'd that spirit and crude salt of Nitre, will either of them by a due application, bring snow, by dissolving it, to congeal water, though the

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Spirit and the Nitre be generally agreed upon to [be] *actually* cold, and one, if not both of them, to be *potentially* cold too.

20. Having thus dispatch'd the *Experiments* pretended to evince an *Antiperistasis*, I must now examine the *Observations*, that are alledg'd to that purpose, of which the principal, if not the only, are these. The coldness of the middle Region of the Air. The increase of mens stomachs in Winter. The generation of Hail; and the heat and cold in Cellars, and other Subterranean places, when the contrary Quality reigns in the Air.

21. To begin with the first of these: I will not now dispute, whether the second Region of the Air, have really that coldness that is wont to be ascrib'd to it: Though our friend Mr. *Boyle* seems to doubt, whether that Regions being always, / and every where cold, have been as strongly proved, as asserted. But passing over that Question, I see no need of imploring the help of *Antiperistasis*, to keep the second Region of the Air for the most part cool. For without at all taking in the cause imagin'd by the Schools, an obvious and sufficient one may be easily assign'd. For the Air being, as to sense, cold of its own nature, so that when we feel it hot, it is made so by some adventitious agent, and that agent being for the most part the Sun, who heats the Air *chiefly*, though not *only*, by its reflected beams; their heat is so languid, by that time they arrive, dispers'd, at the second Region of the Air, that they are not able to overpower its Natural coldness, increased perchance by some frigorifick spirits, that may find a more commodious harbour there, then in other parts of the



Atmosphere. And whatever be the true cause of the coldness in the middle region of the Air, I cannot but admire to find, *that* Coldness so confidently ascrib'd to *Antiperistasis*, / by *Themistius* and his Friends the *Aristotelians*: For according to them, 'tis the Nature of the Element of Air to be as well hot as moist, and according to the same *Peripateticks*, both the upper Region of the Air always, and the lower in Summer is hot, the former by the neighbourhood of the imaginary Element of fire, and the latter by the reflection of the Sun-beams from the Earth: which two Positions being laid together, I would fain learn of any *Aristotelian*, how *Antiperistasis* comes to take place here? For, according to them, those Bodies have their cold and heat increas'd by *Antiperistasis*, that are on both hands assail'd by Bodies of a contrary Quality, to that which is natural to the surrounded Body, whereas the whole Element of Air, and consequently the middle Region, being, as they would perswade us, *hot*, of its own Nature; what shadow of probability is there, that the highest and lowest Regions, by being hot, should make the middle Region, which is also naturally hot, intently and durably cold. / But though the objection is so clear, that it needs not to be insisted on; yet because 'tis but an Argument *ad hominem*, I shall add this for their sakes, that are not in this point *Peripateticks*, That it does not appear to me, that if the Air be naturally rather cold, then hot, the second Region must owe the *Intensness* of that Quality to *Antiperistasis*. For the ground of the opinion, I oppose, being this, That both the first and the third Regions are considerably hot, I would gladly find it prov'd as to the upper Region. I confess I have

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not found the assertion contradicted, but that, as little convinces me, as the uncontrouledness of the Tradition about quick-Lime, that I lately confuted. 'Tis true, there are two reasons alledged, to evince the heat of the supreme Region of the Air, but neither of them to me seems cogent. For the first is, that the Vicinity of this Region to the Element of fire makes it partake a high degree of Heat. But if we consider the distance of that Element, which they place contiguous to the Orb of the / Moon, and how little nearer to it the concave part of the upper Region is, then the Convex of the middle, we may easily conceive, that in two distances, that are both of them so *immense*, so *small*, a disparity cannot be much (if at all) more considerable, then the greater nearness of one side of a sheet of paper held at three yards distance from an ordinary fire, in comparison of the distance of the other side of the same paper; or then the distances of a small Wart, and of the neighbouring parts of the face, when a man comes within 2. or 3. yards of the fire. But 'tis not worth while to prosecute this Consideration, because the Argument against which 'tis alledg'd, is built upon the groundless supposition of the Element of fire, a figment which many of themselves do daily grow asham'd of, as indeed its existence is as little to be discovered by reason, as perceiv'd by sense.

22. The other Argument for the heat of the third Region of the Air, is, that fiery Meteors are kindled by it. But not now to question, whether / all Meteors that shine, and therefore pass for fiery, are really *kindled* exhalations; we see, that in the lower Region of the Air, and in Winter, those fires that are called either *Helena*, or *Castor* and *Pollux*, are generated in great storms, and hang about the sails and shrouds of Ships. Nay, do not we much more frequently see, that Lightning is produced at all seasons of the year (for in warmer Countries thousands have observed it to thunder (and so have I) in Winter) in the middle region of the Air. And *since* 'tis not the *heat* of the inferior part of the Air, that kindles those Exhalations; and *if* notwithstanding the Coldness of the second Region, fiery Meteors may be frequently generated *there*; I see no reason why the Production of such Meteors should argue the heat of the third Region of the Air. And if that Region be not hot, then it will, I presume, be easily granted, that the coldness of the second must very improperly be attributed to such an *Antiperistasis*, as it is generally ascribed to. /

23. I come next to consider that Aphoristical saying of *Hypocrates*, *Ventres hyeme esse calidiores*,<sup>†\*</sup> together with the Observation whereon it seems to have been grounded. I will not now examine, whether any arguments for the contrary may be drawn from the heat and thirst men feel in Summer, and the refreshment they then find by Drinks and Fruits, and other Aliments that are actually cold. For that which I principally intended to say, is this, That I much more doubt the matter of fact

delivered in the Aphorism, then that, in case it be true, it may be made out without the help of *Antiperistasis* in the vulgar and Scholastick notion of that Term.

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24. I consider then first, that the proof, that is wont to be brought of the greater heat of mens stomachs in Winter, is, that men are wont to have *then* a greater appetite to their meat. But though I pay so much respect to the great *Hypocrates*, as to allow the Aphorism in a sense; yet I admit it to be true but upon an *Hypothesis*, that I do not admit to be so. For the Aphorism supposes, that the / digestion of meat in the stomach is made by heat, and consequently, that the stronger digestion, that is wont to be made in Winter, is an argument of the stomachs being *then* hotter, then at other seasons of the year. But the Erroniousness of this supposition, I think, I need not solemnly prove to *Eleutherius*, who I doubt not has taken notice of several things in Nature, that agree not with it, and particularly of the strong concoction, that is made in the stomachs of divers ravenous fishes, whose stomachs and blood are yet, as I have purposely observed, sensibly cold: but if it should in some cases prove true, that there is really in mens bodies a far greater heat in Winter then in Summer; yet this would not infer an *Antiperistasis* in the sense, wherein I oppose it. For the vital heat lodg'd in the heart, always generating out of the blood and juices, that continually circulate through that part, great store of spirits and warm exhalations, which are wont to transpire through the pores of the skin in much greater quantities, then, notwithstanding / the affirmations of *Sanctorius*,<sup>†\*</sup> any thing but my own Trials could have perswaded me, these warm steams finding the pores of the skin straitned and shut up, grow more and more copious in the body, and thereby heat the stomach, as well as the other internal parts of it: And perhaps also the same frigorifick Corpuscles or Temperature of the Air, that produce cold in Winter, may by shutting in certain kinds of *Effluvia*, or perhaps altering the motion or Texture of the blood, reduce it to such a disposition, as that the appetite shall be increas'd, as well as the concoction in the stomach promoted by the *Stomachical menstruum*, or ferment, which either is newly generated in Winter, or more copiously supplied (by the circulating of the blood to the stomach) in that season then in others. And to show, that a good appetite may be procur'd by agents endow'd with very distinct and contrary qualities: do not we see, that spicy Sauces, Wine and Vineger do all of them, in most men, beget an appetite, though / the two former be confessedly hot, and the latter cold. And so Worm-wood, and juice of Lemmons have both of them frequently reliv'd dull and weak stomachs, though the one be confessedly a hot simple, and the other a cold. And in some cases, either the frigorifick Corpuscles themselves, and perhaps some other unknown to us, that they may bring along with them, may so sollicite the stomach, as to breed an eager appetite, not precisely by their being cold or hot, but by their peculiar nature; as we have instances of some, that in these parts by walking on the snow, procure to themselves a *Bulimus*. And the learned *Fromundus* relating,<sup>†\*</sup> how he himself by walking long on the snow, was surpriz'd with such a *Βουλιμία*,<sup>†\*</sup> takes

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notice, that the chief cause of the fainting was in the stomach: And that he found by his own experience, that that part was discompos'd, convell'd, and provok'd to cast. To which he adds, (what makes much for my present purpose) that he now thinks the chief cause of the *Bulimia* to consist in certain / steams, that do peculiarly affect the stomach, which they gnaw and distend. And just before he observes, that straining to fetch deep coughs is a present remedy in this distemper, by discharging the stomach and Lungs of those snowy spirits, which were either attracted in respiration, or had some other way insinuated themselves into those parts: So that besides the cold abstractedly consider'd, the stomach may be peculiarly affected by other, either attributes or concomitants, of the frigorifick Corpuscles, that grow powerful in frosty weather; with which it well agrees, that divers have been observed to be subject to *Bulimias*'s in these parts of the world, though in our warmer Climates such men endure nothing near so great a cold, nor are so much inconvenienced by it, as multitudes of others, that in *Nova Zembla*, and other gelid Regions never

complain'd of having contracted even in the midst of Winter, any such disease.

25. Another argument that is specious enough, urg'd in favour of *Antiperistasis*, / is borrowed from the production of Hail, which is presum'd to be generated in Summer only, not in Winter, and, according to *Aristotle* and the Schools, is made in the lowest Region of the Air, by the cold of the falling drops of rain so highly intended by the warmth it meets with in the Air near the Earth, as to congeal the water wherein 'tis harbour'd. But though I freely confess to you, that I think the generation of Hail difficult enough to be solidly explicated; yet I scruple not to reject the receiv'd doctrine about it, for several reasons, of which I will now name four.

26. For in the first place, 'tis not universally true, as is suppos'd, and the *Aristotelian* doctrine requires, that Hail falls not but in Summer, or very hot weather. For I have my self observ'd it within this twelve moneth, to Hail at the latter end of *November*, and that, when some frosty days have preceded, and when the coldness of the weather was complain'd of. Nay, the longest shower of Hail, that either I, or some others / remember our selves to have ever known, I observ'd to fall about a week before the end of *January*, on a night preceded by a very frosty day, which it self was preceded by a sharp fit of frosty weather. And here I must not pretermitt this circumstance, that when the tedious shower was over, there came to the house, where I then was, a maid, that is servant to one of my Domesticks, and related to her Master, and others, how she was for a good while misled out of the beaten way, where the storm found her by an *Ignis fatuus*, which she followed, till by its passing over a place, where she found an unpassable hedge, it both show'd her, that she was out of her way, and that it was no candle, though she had so confidently thought it one, that she call'd out to the party, she presum'd it to be carried by. I will leave *Themistius* to unriddle, how the Nocturnal Air could kindle a fiery Meteor by its coldness, and at the same time congeal the falling drops of water into ice by its warmth, and shall only add, that I doubt

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not but / other observations of the like kind have been often made, though perhaps seldom recorded. For within the compass of a very few weeks of the storm, some servants of mine affirm'd themselves to have observed it to Hail two or three times besides that already mention'd.

27. Next, if *Aristotle* have rightly assign'd the cause of Hail, 'tis somewhat strange it should not fall far more frequently in Summer, and especially in hot Climates, then it does, considering how often in all probability the drops of rain fall cold out of the second Region into the warm Air of the first. And more strange it is, That even in those parts of *Ægypt*, where it rains frequently enough and plentifully (for so *Prosper Alpinus*, that liv'd long there, assures us it does<sup>†\*</sup>) though not about *Grand Cairo*, yet about *Alexandria* and *Pelusium*, it should never Hail no more then Snow, as the same learned Physician (a witness above exception) affirms. Besides, whereas it is pretended, that Snow is generated in the upper Region of the Air, and / Hail always in the lower, my own observation has afforded me many instances, that seem to contradict the Tradition. For I have observed in I know not how many great grains of Hail, that besides a hard transparent icy shell, there was as 'twere a snowy Pith of a soft and white substance, and this snowy part was *most commonly* in the middle of the icy, which made me call it Pith, but *sometimes* otherwise. And lastly, whereas the favourers of *Antiperistasis* would have the Drops of rain in their descent, to be congeal'd apart in the ambient Air; not to urge, how little the irregular and Angular figures we often meet with in Hail does countenance this doctrine; Hail often falls in grains, too great by odds to be fit to comply with *Aristotles* conceit. For not to mention the grains of Hail I have observed my self to be of a bigness unsuitable to this opinion, divers learned eye-witnesses have inform'd me of their having observ'd much greater then those I have done: and particularly an eminent *Virtuoso* of unquestionable credit, affirm'd both / to me and to an Assembly of *Virtuosi*, that he had some years ago at *Lyons* in *France* observ'd a shower of Hail, many of whose grains were as big as ordinary Tennis-balls, and which did the Windows and Tyles a mischief answerable to that unusual bulk. And *Bartholinus* affirms,<sup>†\*</sup> that he himself observ'd, in another shower of Hail, grains of a more

unwonted size; a single grain weighing no less then a whole pound. But though this it self is little in comparison of what I remember I have somewhere met with in learned Authors, yet it may abundantly suffice to disprove the vulgar conceit about the generation of Hail, till we meet in these Countries with showers of rain, whose single drops prove to be of such a bigness; which I presume those that ascribe to Hail to *Antiperistasis* will not easily show us.

28. I come now to consider the last and indeed the chiefest example, that is given of *Antiperistasis*, namely the coldness of Cellars, and other subterranean Vaults in Summer, and their heat in Winter. And as the Argument, wont to

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be drawn from / hence, consists of two parts, I will examine each of them by its self.

29. And first, as to the refreshing coldness, that subterranean places are wont to afford us in Summer, I both deny, that they are *then* colder than in Winter; and I say, that though they were, that coldness would not necessarily infer an *Antiperistasis*.

30. We must consider then, that in Summer our Bodies having for many days, if not some weeks, or perhaps months, been constantly environ'd with an Air, which, at that season of the year, is much hotter, then 'tis wont to be in Winter, or in other seasons, our senses may easily impose upon us, and we may be much mistaken, by concluding upon their Testimony, that the subterranean Air we then find so cool, is really colder, then it was in Winter, or at the Spring; as they that come out of hot Baths think the Air of the adjoining rooms very fresh and cool, which they found to be very warm, when coming out of the open Air, they went through those warm rooms to the Bath, and the deepness / and retiredness of these subterranean Caves keep the Air, they harbour'd, from being any thing near so much affected with the changes of the season, as the outward Air that is freely expos'd to the Suns warming beams, which pierces with any sensible force so little a way into the ground, that Diggers are not wont to observe the Earth to be dried and discolour'd by them beyond the depth of a very few feet. And I have found, that in very shallow Mines not exceeding six or seven yards in depth, though the mouth were wide, and the descent perpendicular enough, the Air was cool in the heat of Summer; so that the free Air and our Bodies that are always immers'd in it, being much warmer in Summer then at other times, and the subterranean Air by reason of its remoteness from those causes of alteration, continuing still the same, or but very little chang'd, it's no wonder, there should appear a difference as to sense, when our bodies pass from one of them to another.

31. And supposing, but not yielding, / that the Air of Cellars and Vaults were really colder in Summer then in Winter, that is, were discovered to have a greater coldness, not only as to our sense of feeling, but as to Weather-glasses; yet why should we for all that have recourse for the solution of the difficulty to an *Antiperistasis*, which 'tis much harder to understand, then to find out the cause of the *Phænomenon*, which seems in short to be this, That whereas (which I shall soon have occasion to manifest) there are warm Exhalations, that in all seasons are plentifully sent up by the subterranean heat, from the lower to the superficial parts of the Earth, these steams, that in Winter are in great part repress'd, or check'd in their ascent, by the cold frost or snow, that constipates the surface of the Earth, and choaks up its pores, these Exhalations, I say, that being detain'd in the ground would temper the Native coldness of the Earth and Water, and consequently that of Springs, and of the subterranean Air, are by the heat that reigns in the outward Air, call'd out at the / many pores and chinks, which that heat opens on the surface of the ground, by which means the water of deep Springs and Wells, and the subterranean Air, being depriv'd of that, which is wont to allay their Native or

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wonted coldness, are left to disclose a higher degree of it, and seem to have that quality increas'd, when indeed it is but freed from the mixture of its contrary that weakened it.



32. As for the heat, we find in Cellars and Vaults in Winter, the solutions already given will be applicable to that *Phænomenon* also, which by this way is yet more easie to be accounted for then the other. For having first question'd the matter of fact, 'twill not be difficult to show, that though it were true, it need not be ascrib'd to *Antiperistasis*.

33. I think then, that it may be justly question'd, whether Cellars in general are hotter in Winter then they are in Summer. For as for the Testimony of our senses, upon which alone men are wont to conclude the affirmative, it may in this case easily / and much delude us. For those places being shelter'd from the winds, and kept from a free communication with the outward Air, are much less expos'd then others to the action of those agents, whatever they be, that produce cold in the Air. So that our bodies being constantly immers'd in the Air refrigerated by the Winter, and consequently brought nearer to the temper of that Air, when we bring those bodies into Cellars the subterranean air must seem warm to us, though in it self it were really invaried as to its temper.

34. Now that many Cellars are indeed colder in the midst of Winter, then in the heat of Summer, though not in respect of our senses, yet in respect of other bodies that have not the same predispositions, I am induc'd to believe by some Experiments of mine own, purposely made. And first in a frosty evening having hung out in a Garden two seal'd Weather-glasses, that they might be reduc'd as near as could be to the temper of the ambient Air, I brought one of them into a Cellar, and it soon began / manifestly to rise, and in two or three hours ascended five or six divisions, whilst the water in another seal'd Weather-glass, that continued suspended in the same part of the Garden, did rather a little subside, then at all rise, which is agreeable to the first part of what I was saying; namely, that the Air, harbour'd in Cellars, is not so powerfully affected by the ordinary efficients of cold, as the free and external air. And now as to the second part of what I was saying, that the subterranean Air, though it be less affected by the outward cold, may be somewhat affected by it, instead of growing hotter by *Antiperistasis*; I shall add, that early in the morning in frosty Weather the liquor in the same Weather-glass appear'd more subsided, then over-night, which shows, that the external air did lessen, not increase the warmth of the air in the Cellar. And having there plac'd a wide mouth'd glass of oyl, which in thawing weather remain'd all night fluid as before, the same liquor, the very next night, which was a bitter frost, / was so far frozen and congeal'd, as to sink in other oyl, and keep its surface exactly, though the glass were inclined and turned upside down. And prosecuting my Trial, I found, that in a sharp frost, and great snow, the liquor, that on the Thursday night was beneath the fourth knub or mark of division, a sudden thaw coming with a South wind, the next morning in the same Cellar the liquor was ascended to the eighth mark. And continuing the Weather-glass in the same Cellar for a good while, to watch its alterations

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every night and morning, I remember I met with, and registred more observations, that confirm'd me in my opinion, though 'tis so long ago, that I have forgot the particular circumstances. And after these Trials meeting with a learned *Polander*, I did without declaring my opinion, inquire of him, whether in his Country he had at any time observ'd Beer to freez in Cellars in frosty weather, to which he answer'd, that in the coldest Winters, if the Beer were small, the Barrels would oftentimes be frozen, but not / if it were strong. But I need not have recourse to forrain Testimony, having my self observed here in *England* more then one Barrel of Beer to be frozen in the Cellar in exceeding cold weather. Insomuch that one of the Barrels being full, and the liquor expanded by freezing, was forc'd out at certain chinks, which seem to have been made by that expansive force, and the liquor so ejected, adhered in a considerable lump to the outside of the vessel; and yet this Cellar had its Windows carefully shut, and not only was near a Kitchen, where fire was constantly kept, but, which was more considerable, it had this principal mark of being a good Cellar, that in the heat of Summer it us'd to afford me drink sufficiently cool. And now to requite *Eleutherius* with the Testimony of that very person, Physician to the *Russian* Emperor, whose authority he lately alledg'd against me,<sup>†\*</sup> I shall confess, that as he suspects, I had conference with this Doctor, and when I diligently enquired of him, whether their Cellars at *Musco* were / really very cold in Summer, he answered

me, that they were not, and that they had distinct Cellars for Summer and for Winter, that their small Beer would quickly grow sour in their Cellars in Summer, if their vessels were not kept in Snow, that therefore their way was to make at the bottom of their Summer Cellars (to which belong'd a Well to receive the water dropping from the melted Snow) a deep layer of snow, on which they afterwards cast a convenient quantity of water, that the whole mass might be turn'd into a kind of ice. In this snow they keep their Casks, making sometimes a layer of Snow, and a layer of Cask, and digging out their vessels, as they had occasion to use them.]<sup>†\*</sup> By all which it may appear how groundlessly it's *universally* affirm'd of Cellars, that as they seem *to the sense*, so they *really* are hotter in Winter than in Summer.

35. But *if it should happen*, (as in some places 'tis not impossible, but that it may) that some Vaults and Cellars are really warmer in Summer than in / Winter; yet I see not why this should reduce us to the acknowledgment of an *Antiperistasis*; for neither could the effect be made out by that, nor would there be any necessity to have recourse to it.

36. And first I might content my self to repeat, what I have formerly said, to shew the incongruity of *Antiperistasis* in general to Nature's ways of acting. And I might add, that to imagine with some late *Peripateticks*, (whom all their reverence to *Aristotle* has not so far blinded, as not to let them see the

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unreasonableness of his conceit) that in Winter the warmth of the ambient air retreats into Cellars and Vaults to shun its contrary, is to make meer accidents, or at best inanimate agents, act with knowledge and design. But I will rather represent, that, though *Antiperistasis* were intelligible, it were improper to alledge it in our case. For to invigorate the warmth of the air by the Cold, the air must according to them be environ'd with other cold bodies, and the heat must retire it self as far as it can from them. And accordingly / 'tis observ'd, that in Winter the deepest Cellars are warmest; but in the case before us the subterranean air, though above, it have the cold that reigns in Winter; yet beneath, the subterranean heat makes the Earth very warm. This I shall not wonder, if you look upon, as new and Paradoxical. And therefore I shall apply my self to the proof of it, and to convince you, I shall not imploy the observations of Chymists and Mineralists, for fear you should suspect them of ignorance or design, but I will use only the authority of a learned Physician, *who* I think was also a professor of Mathematicks, *who* in but too many points is a stout *Peripatetick*, and *who* above all this professes himself to be an eye-witness of what he relates.<sup>†\*</sup> This Author then informs us, that about the year 1615. he had a curiosity to visit the Mines of *Hungary*, and particularly to go down into the deep Goden Mine at *Cremnitz*, and that after he had descended fourscore or a hundred fathom, he found it excessively hot, though he had but a slight linnen garment / on, and though he be a maintainer of *Antiperistasis*, yet he affirms that not only the Overseer and workmen of that Mine, but also those of divers other Mines unanimously assured him, That that lower Region of the Earth was all the year long very hot, and as well in Winter as he found it in Summer; so that it seems in Winter the heat of the subterranean parts less remote from the superficies, cannot be intended by the coldness of the more internal parts of the Earth, those parts bring themselves not always cold, but always hot.

37. *Eleutherius*. But you may, *Carneades*, remember, that this very Author tells you, that he found the supreme region of the Earth, as he calls it, which is that next the air, exceedingly cold, both as he went down into the Mine, and as he came up again, and that he ascribes that coldness to *Antiperistasis*.<sup>†@</sup> /

38. *Carneades*. Right, but you may remember too, that he relates, that 'twas in *July*, and in very hot weather, that he went down into the Mine,<sup>†@</sup> and that to avoid fouling his clothes, he put them off, and exchang'd them for a light loose Linnen garment, such as the Diggers wore, and this himself mentions, as that which much increased the coldness he felt: So that if besides this, we

consider, that he descended into a cooler place, with a Body already affected with the great heat, which he elsewhere takes notice, that that season had given the outward air, and perhaps much heated by riding or walking to the Mine, we shall not wonder, that he found the change very sensible as he went down; and we shall less wonder, that he found the upper Region of the Earth, as he calls it, more cold when he came up again: since besides the toil of going to and fro, and ascending through narrow, low, and difficult passages, he came out of a place excessively / hot; insomuch that he tells us,†@ that the Overseer of the Mine would not go back with him the same way he came, but took a far shorter, though it were a more dangerous way, causing himself to be drawn up in a perpendicular Groove, and rendring this reason, that 'twas very unhealthy, when one comes out of a place where the Diggers work naked, and where one is even melting into sweat, to make any long stay in the superior Region of the Earth. So that besides that this Author, although he maintains *Antiperistasis*, yet he allows this upper Region to be hot in Winter, as well as cold in Summer, and consequently, that in Winter it has not a cold region beneath, as well as above it, which is enough to vindicate the thing for which I first alledged his Testimony: Besides this, I say, to me, who, though I willingly thank him for his Narrative, am much more sway'd by what he *relates*, then by what he *thinks*; the matter of fact seems very favourable to my opinion; for you see, that I can justly refer the cold he felt / near the surface of the Earth, to the deception of his sense, but the heat he felt within the bowels of the Earth cannot be referred to the same cause, since he tells us, that at the top of that great and perpendicular Groove, by which the Mine-master was drawn up, there ascended a plentiful smoak, that was, even above the mouth of it, felt actually hot;†@ and besides his own confession, that the deep parts of the Mine, were more then seemingly hot, I can draw further proofs from these two circumstances, that I have elsewhere met with in his Narrative: The one, that on the surface of the Earth, it was then excessively hot; another, that the smoak, which, notwithstanding this heat appeared hot, had in its ascent passed through four or five hundred foot of a cold region of the Earth, whereby it may well be supposed, to have been much infrigidated.†@ To these relations of the learned *Morinus*, I will add, that the / Archbishop of *Upsal* affirms, that in the year 1528. being in *Poland*, he went to visit those deep mountains (as he terms them) whence they dig solid salt, and having descended fifty Ladders, found in the deeper places, that the workmen were naked, because of the heat.†@ so that supposing the time of the

year not to be considerable in this case, it seems by this relation, that, provided a man descends low enough into the bowels of the Earth, he will find it very hot, even in places that want those Metals, or Marchisites, or other like Mineral substances, by the action of saline liquors, or exhalations, upon which, you, *Eleutherius*, have, I remember, sometimes suspected, that the heat observed in Mines may be produced.

39. I have hitherto shown, that the heat of Cellars and Vaults in Winter, has been very *improperly*, / and now I come to show, that it has been as *unnecessarily* ascrib'd to *Antiperistasis*. For as the air of those places is protected from the greatest part of the adventitious Coldness that reigns in the outward Air: so the subterranean air has a positive cause of heat in Winter, that it has not in Summer. For as I formerly took notice, in Summer the pores of the Earth, being dilated and opened by heat, the warm exhalations, that were wont to be mingled with moist vapours in the bowels of the Earth, are call'd out, and exhal'd away. For as in the Winter the surface of the Earth being hardned by frost, or the pores of it choak'd up, or at least much obstructed, the hot steams, that, as I lately prov'd by our *French* Authors Testimony (to which I could add, that of eminent Chymists and Mineralists) do continually, *and* copiously enough ascend from the warm Region, or lower parts of the Earth, are in great part detained and imprison'd in Cellars, and other subterranean cavities, where consequently they produce such a heat, as / to those that come out of the cold air, may be very sensible. And the rather, because whilst men, by the coldness of the season, are more then ordinarily careful, to stop



up the passages, at which the external air may get in, they do, though designlessly, stop up the vents, at which the subterraneous exhalations might get out. And to shew you, that this last circumstance is not impertinently taken notice of, I shall tell you, that a very grave Author having occasion to mention Cellars, relates it, as a practise in divers houses of a Town, where he had been, to keep vents in their deep Cellars, which in the Summer, were from time to time opened, partly to keep the places sweet and wholsom, and partly to let out the warm Exhalations, that would else hinder their liquors from keeping so fresh, and well. And these steams were affirm'd to have been several times taken notice of to ascend visibly into the free air like a smoak, which several *Phænomena*, and particularly what I formerly related of the hot fumes, that manifestly / ascended out of the great Groove in the *Hungarian Mine*, may keep us from thinking incredible.

40. And now by what I have hitherto discours'd, I have made way for the solution of a *Phænomenon*, that is wont to be much urg'd in favour of *Antiperistasis*, namely, the smoaking of water, that is drawn in frosty weather, out of deep Wells and Springs.

41. But first I must advertise you, that 'tis improperly enough, that some urge for *Antiperistasis*, such examples as the strange Spring near the Temple of

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*Jupiter Ammon*, which *Lucretius* and others have observed to have been exceeding cold in the day time, and as hot at night;<sup>†\*</sup> for, not now to examine, whether this story be not fabulous, or might not be ascrib'd to some crafty trick of the Idolatrous Priests, that had a mind to impose upon *Alexander*, as well as others, and procure an admiration to the place; I consider, that this, and other the like cases, such as are the Springs mentioned in the Islands of *Maldiviæ*, by *Pyrard* (a French / Author, that was shipwrack'd, and liv'd long in those parts)<sup>†\*</sup> must be referred to the peculiar Nature of the Springs, or some other hidden cause, since, if the water of them were but ordinary, and the *Phænomena* were the effects of *Antiperistasis*, it might justly be expected, that the like should happen in all Springs, or at least in very many, which, that it does not, common experience shows us. And I would say, that this might be the case of the Spring, you mention out of Captain *James's* Voyage,<sup>†@</sup> but that *besides*, that he does not say expressly, that it was frozen in *July* but only that then it afforded him no water, which might happen upon divers other accounts: And *besides*, that 'tis manifest, that in far hotter Countries, where the excessive heat of the Air might more intend the subterranean cold, if *Antiperistasis* could do it, there is no talk of any such degree of cold in Summer, as to freez the Springs; *besides* this, I say, there seems to be, through some mistake or other, a contradiction in the relation it self, since in the same no water Voyage, / speaking of the same month of *December*, he expressly says, that their Well was then frozen up, so, *that dig as deep as they could, they could come by no water*.<sup>†@</sup> And he complains on that occasion, of the unwholsomness of melted snow-water. 'Tis true, that he soon after mentions a Spring, that he found under a hills side, which did not so freez,<sup>†@</sup> but that he could break the ice and come to it, but by his very sending far from his house to that Spring, it appears to have been a Consequence, and therefore a Proof, of the uselessness of his Well in *December*; as his affirmation, that it continued all the year so, as to be serviceable, when the ice was broken, shows, that the *Antiperistasis* did not freez it up in Summer. And having cleared my self of such a Testimony of this ingenious Navigator, as would appear very illustrious, if there had been no mistake about it, I shall not scruple to add, that the late publisher of the Latin Description of *Denmark* and *Norway* informs us,<sup>†\*</sup> that in or near that little Danish Island *Hueena*, wherein the famous *Tycho* / built his *Urani-Burgum*, there is one Spring among many ordinary ones, that even in the coldest Winter is never frozen, which, subjoyns my Author, does in these

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regions exceeding rarely happen to be found. *Olaus Magnus* also relates,<sup>†@</sup> that in another part of the King of *Denmarks* Dominions, namely, near *Nidrosia*, one of the chief Cities of *Norway*, there is a Lake, that even in that Northern Region never freezes. And the learned *Josephus Acosta*



mentions,†@ that among a very great number of hot Springs to be met with in *Peru*, *At the Baths*, which they call the *Baths of Ingua*, there is a course of water, which comes forth all hot and boiling, and joyning unto it, there is another whose water is as cold as ice. He adds, That the *Ingua* (or the *Peruvian Emperor*) was accustomed to temper the one with the other, and that it is a wonderful thing to see Springs of so contrary qualities so near one to another. These relations as I was saying, I scruple not to mention, / though at first sight they may seem to disfavour my cause. For by these and some others it may appear, that Springs may obtain very peculiar and strange qualities from the nature of the places whence they come, or through which they pass, or from some other causes, that are as hidden from us, as the originals of these rare waters. And this being once prov'd, who knows what interest, such causes, as we are strangers to, may have in some *Phænomena*, that are wont to be wholly ascrib'd to the heat and cold of the superficial part of the ground, and what influence they have upon many other Springs (besides those above mentioned) some of which that are very deep, may rise from the warm region of the Earth, where they may be affected by the place, as both these and others may be by Mineral juices and steams (*such, perhaps, as we know nothing of*) though we well know, that some of them that are saline, without being at all sensibly hot, will powerfully resist congelation.

42. But having hinted thus much / on this occasion, I shall now proceed to consider, The smoaking of waters drawn from deep places in frosty weather, and show, that it does not necessarily conclude, such water to be warmer in Winter, since that effect may proceed not from the greater warmth of the water in such weather, but from the greater coldness of the Air. For we may take notice, that a mans breath in Summer, or in mild Winter weather, becomes very visible, the cold ambient Air nimbly condensing the fuliginous steams, which are discharg'd by the Lungs, and which in warmer weather are readily diffus'd in imperceptible particles through the air. And I have observed upon the opening of issues in some mens arms, that though no smoak be visible in Summer, it will be very conspicuous in exceeding sharp weather, though mens arms, at least the external parts of them, seem to have less heat in frosty weather, then in Summer; since in the former of those seasons, they are wont to be manifestly more slender, the fleshy parts and juices being condensed / by the coldness of the Air. And though the insensible Transpirations, that continually exhale from all the parts of our bodies, are

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not wont to be visible here, even in Winter; yet in extremely cold Countries, as *Nova Zembla*, or *Charleton Island*, those *Effluvia* have been observ'd, not only to be thickned, but to be turned into ice it self, sometimes within the Sea-mens shooes. And here in *England*, having not long since imployed a labouring man to dig a deep hole in very frosty weather, two Servants of mine, that stood by to see him work, did both of them assure me, when they return'd, that the steams of his heated body, were frozen upon the outside of his Wastcoat, which, one of them, whilst the other was about to give me notice of it, inconsiderately wip'd off.

43. And since we see how fast the water in Ponds and Ditches, wastes and decreases in Summer, there is no cause to doubt, but that it does then continually emit Exhalations as well, if not much more plentifully, then in / Winter, which may be manifestly confirmed by this, that in the Summer, one shall often see in the mornings or evenings, the face of the water cover'd with a mist or smoak, that rises out of it. And I have sometimes taken pleasure to see this aggregate of Exhalations, hover over the water, and make, as it were, another River of a lighter liquor, that conform'd it self, for a considerable way, to the breadth and windings of the stream, whence it proceeded. And I think it will be easily granted, that the water in Summer time is at least as warm at noon, when such Exhalations are not visible, as in the morning when they are, though the Air be colder at this part of the day, then at that; which observation gives us the true reason of the *Phænomenon*.

44. And though notwithstanding all this, it were made to appear, that in some cases, the smoaking water of Springs may be really warmer in Winter then in Summer; yet a sufficient reason of the

*Phænomenon* may be fetch'd from what I have already / delivered about the detention of the warm subterranean vapours by the frost, and snow, and rain, that make the earth less perspirable in Winter.

45. And because I know *Themistius* will look upon a thing so disagreeable to the vulgar opinion, Of the Coldness of the whole Element of Earth, as a Paradox; I will take this opportunity to add a further confirmation, to what I have been saying.

46. And first, that there arise copious and warm steams from the lower parts of the Earth, may be prov'd, not only by what I have already mentioned, touching the *Hungarian Mines*, but by the common complaint of Diggers in most, though not in all deep Mines, That they are oftentimes troubled, and sometimes endangered by sudden damp, which do frequently so stuff up and thicken the subterranean Air, that they make it not only unfit for respiration, but able to extinguish the Lamps and Candles, that the Miners use, to give them light to work by. And I remember, / that I have visited Mines, where having inquired of the diggers, whether those hot exhalations, that compose their damp, did not sometimes actually take fire within the bowels of the Earth, I was answered, that in some of their Pits (and particularly in one, that they show'd me) though not in all, they did, insomuch that the exhalation suddenly kindling, would make a report at the mouth of the

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Pit like a Musquet, or a small piece of Ordinance, and the flame would actually burn off the hair, and scorch the skins of those workmen, that did not seasonably get out of the Pit, when the exhalation appear'd to be near an ascension, or did not nimbly fall down flat with their faces to the ground, till the flame was gone out. And one of these workmen that I ask'd, affirm'd himself to have been several times, to his no small trouble, so burned, and that (if I much misremember not) twice in one day. And it seems to me as well as to *Morinus* very probable, that those great quantities of rain and snow, and storms, and (perhaps) / some other Meteors, that are taken notice of in Winter, may rather consist of these subterranean steams, then the vapours and exhalations attracted by the Sun (or at least may as much consist of the former, as the latter.)<sup>†\*</sup> For his heat is then very languid, and acts upon the ground but during the day time, which is very short (whereas those Meteors are generated indifferently at all hours of the day and night) and the sky is oftentimes, for many days together, quite overcast with clouds, and the surface of the ground so constipated with frost, that it will sometimes freez even in the Sun-shine: So that 'tis not near so likely, that the heat of the Sun, in the midst of all these disadvantages, should be able to elevate so great a plenty of exhalations and vapours, as are requisite to compose the rain, and snow, and storms, that sometimes last almost all the Winter, as that they should be suppli'd by subterranean steams copiously sent up from the heat that continually reigns in the lower parts of the Earth, and by traversing the / Sea, and at other vents, get up into the Air.

47. To make out this, my formerly quoted French Author relates a very memorable thing,<sup>†@</sup> that was told him by the Masters of those Mines in *Hungary* (which are at least as deep as any that I remember I have seen or read of;) namely, that the Miners were able certainly to foretel sooner then any other mortals, the Tempests and sudden mutations, that were to happen in the Air. For when they perceived by the burning blew of their Lights, and by other manifest signs, that they could easily take notice of in their Grooves, that store of the Tempestuous Damp (if I may so call it) was ascending from the lower parts of the Earth, though the sky above were clear, and the Air calm; yet they could assuredly foretel the approach of a storm, or some other great alteration in the Air, which would accordingly ensue within no very long time after. And to confirm this Narrative, I shall add, not only that 'tis agreeable to what I lately told you was affirm'd to me by / other Mine-men, but that having enquir'd of a very ingenious Physician, who liv'd many years in *Cornwall*,<sup>†\*</sup> (a Country you know famous for Tin-Mines, some of which are infamous for the damp that infest them) he told me, that divers of the experienced Fishermen assur'd him, that oftentimes they did perceive fires shining

in the night, sometimes in one place, sometimes in another, which were suppos'd to be kindled by the sulphurous and other subterraneous exhalations, and that, when they perceiv'd those fires, (especially if any number appear'd in several places) those that were well acquainted with the coast, would not continue long out at Sea, but rather quit an opportunity of catching Fish, then not make seasonably to the shore, having often observed, and particularly this last year, that bold and unexperienced Mariners, by slighting these forerunners of storms, were in few hours shipwrack'd by them.

48. To this I shall add, what happened some years since, upon the *Irish* coast, near a strong Fortress, / called *Duncannon*, where divers of the ships Royal of *England* lying at anchor, in a place where they apprehended no danger from the wind, there seem'd suddenly to ascend out of the water, not far from them, a black cloud, in shape and bigness not much unlike a Barrel, which mounting upwards, was not long after follow'd, as the most experienced Pilot foretold, so hideous a storm, as forc'd those ships to go to Sea again, and had like to have cast them away in it. And this account was both written by the principal officers of the Squadron, to their superiors in *England*, and given soon after it happened, by the chief of those eye-witnesses (and particularly by the Pilot) to a very near kinsman of mine (well vers'd in Maritime affairs) that commanded the land forces in those parts, as a truth no less known then memorable.†\*

49. And on occasion of what I was saying, about the eruption of hot steams, in several parts of the Earth, I now call to mind something that I have met with in a very small, but / curious Dissertation, *De admirandis Hungariæ aquis*, whose Anonymous Author I gather from some passages in the Tract it self, to have been a Nobleman, Governor of *Saros*, and some other places in *Hungary*, and to have written this discourse, both for, and to that inquisitive *German*, *Baron Sigismundus Liber*, famous for the account he gave the world of the Embassy, whereon he was sent by the *German* to the *Russian* Emperor. This Anonymous, but noble writer, tells us then, that in that part of *Hungary*, which he calls *Comitatus Zoliensis*, there is a gaping piece of ground, which does emit such mortal expirations, that they suffocate, not only Cats and Dogs, purposely held at the end of long poles over the cleft, but kill even Birds, that attempt to fly over it.†@ And in other places of the same Tract, I have met with many other relations, which if I had time to make a particular mention of, / would much countenance what I have been lately saying: but

though I pretermit several other instances, I cannot but take especial notice of one, which (together with what I lately mention'd to have happened near *Duncannon*) may make it probable, that not only under the surface of the dry ground, but in that part of the Terrestrial Globe, that is covered with water, there may arise streams (and consequently Exhalations) *actually* and that *considerably*, hot. For in one place he takes notice,†@ that, not far from the well known City of *Buda*, there is a hot Spring (which they call *Purgatory*) which the waters of *Danubius* it self are not able to keep from being hot; nay, within the very Banks, betwixt which that great River runs, / there boil up hot Springs, where those that will go deep enough into the water, may commodiously bath themselves. And elsewhere speaking of the River *Istrogranum*, in the same County, he adds, That not only the Banks of it, but within the very River of it self, one may discover hot Springs, by removing the Sand at the bottom with ones feet.†@ To this I shall add, That having heard of a Ditch in the North of *England* (in some regards more *strange*, though less *famous* then the sulphureous *Grotta* near *Naples*)†\* whence not only subterranean steams, but those so sulphureous, as to be easily *Inflamable*, did constantly and plentifully ascend into the Air, I had the curiosity to make inquiry about it, of the Minister of the place, (a very learned Man, and conversant in Mines) who then happened to be my neighbour, and he attested the truth of the relation upon his own knowledge. And it was confirm'd to me / by a very *ingenious Gentleman*, who went purposely to visit this place,



That a lighted Candle, or some such actually burning body being held where this Exhalation issued out of the Earth, would kindle it, and make it actually flame for a good while, and (if I misremember not) as long as one pleas'd.

And as this place was but few years since taken notice of, so there may be probably very many others, yet undiscovered, that may supply the Air with store of Mineral exhalations, proper to generate fiery Meteors and Winds; I remember, that having lately ask'd an inquisitive Gentleman, that is a great searcher after Mines, whether he did not observe some meteors near those places, where he is most conversant, he told me, that 'tis very usual in some of them, to see certain great fires moving in the Air, which in those places, diggers, because of some resemblance (real or imaginary) are wont to call Draggons. [And the *Russian* Emperors Physician, you were speaking of,†\* inform'd me a while / since, that he had, not long ago, observ'd in Winter a River in *Muscovy*, where though the rest of the surface was frozen, there was a part of it near a mile long, that remain'd uncovered with ice, which probably was kept from being generated there by those subterraneous Exhalations, since he says he saw them ascend up all the way like the smoak of an Oven.] And in case the matter of fact delivered by *Olaus Magnus* be true, concerning the strange thaws that sometimes happen, with terrible noises, in the great Lake *Veter*,†@ those wonderful *Phænomena*, may not improbably be ascrib'd to the ascent of great store of hot subterranean steams, which suddenly cracking the thick and solid ice in many places at once, produce the hideous Noises, and the hasty Thaw that he speaks of. And this suspicion / may be countenanced partly by this circumstance, that before these sudden thaws, the Lake begins with great noise to boil at the bottom, and partly by what is related by a more Authentick writer, I mean, that learned Traveller the Jesuite *Martinius*,†\* who witnesses, that at *Peking*, the royal City of *China*, 'tis very usual, that after the Rivers and Ponds have continued hard frozen over, during the Winter, the Thaw is made in one day; which, since the freezing of the waters (as he tells us) required many, makes it very probable, That the sudden thaw is effected (as he also inclines to think) by subterranean steams, which I may well suppose to be exceeding copious, and to diffuse themselves every way to a very great extent, since they are able so soon to thaw the Rivers and Ponds of a

large Territory, and that (which makes mainly for my present purpose) beginning contrary to vulgar thaws, from the bottom upwards.

50. And having thus manifested, that the lower parts of the Earth do send up great store of Exhalations / and Vapours to the upper parts, it will be obvious to conceive, that as in divers places of the Terrestrial Globe, these steams get into the Air, either by the advantage of finding vents, such as those I have already mentioned, or by growing copious enough to force themselves a passage: So in most other places, where the ascending steams find no commodious vents, or are too faintly driven up to gain themselves a passage, they must be repress'd or detain'd beneath the surface of the Earth, which has its pores in Winter usually choak'd up with snow or rain, or its surface constipated and hardened with ice or frost, so that these exhalations being pent up, and receiving fresh supplies, from time to time, from beneath, 'twere no wonder, if they should somewhat warm deep Cellars and Wells, where they are thus detain'd; and therefore our Husbandmen do not speak altogether so improperly, when they say, that the snow keeps the ground warm. And I remember, that Dr. *Smith*, the learned English Ambassador into / *Musco*,†\* makes it to be one of the principal reasons of the great fertility, he justly ascribes to the Country there about, that during almost all the Winter, the ground is to a great height covered with snow, which does not only enrich it by the fertilizing salt, which the Earth gains from the snow, when that comes to be melted, but does also contribute to its improvement, by choaking up, or obstructing the pores, at which the Nitro-sulphureous, and other



useful Corpuscles, that are sent up by the subterranean heat, would easily get away. And least (Gentlemen) you should think, that 'tis only by the Ratiocination, that I conclude, that there is really great store of warm steams detain'd under ground in the Winter: I shall add this sensible observation, receiv'd from the *Russian* Emperors Physician already often mention'd,†\* by whom I have been assured, that about *Musco*, where the surface of the ground is far more constipated in Winter, than 'tis in these parts, and where they are wont to keep their Cellars much closer, / the subterraneous Exhalations being hinder'd to fly abroad, will in time multiply so fast, that he assures me, that upon the unwary opening of the doors of Cellars, that have been long kept shut, there would sally out a warm smoak, and very thick, almost like that of a furnace, and sometimes the steam that issues out will be so gross and plentiful, that it has brought men into danger of being suffocated by it.

51. And now, Gentlemen, having shown, that though Experience be so confidently appeal'd to, by the maintainers of *Antiperistasis*, yet she has not hitherto afforded them any thing, that much favours their Cause, it remains, that I show, that she bears witness against it. For besides that some passages of

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my late Discourses do really contain *Phænomena*, that not only do not favour *Antiperistasis*, but may justly be imploy'd as Experiments against it, I shall *ex abundanti* (as they speak) present you with something, which I necessitated Experience to supply me with, that seems expressly to overthrow it. /

52. I might urge against those, who, though they begin to be asham'd of the Doctrine of the Schools, would establish an *Antiperistasis* upon the account of what they call a *fuga Contrarii*,†\* that the very instance they are wont to bring for their opinion, may be retorted upon them. For when they tell us, that in Winter, the heat, to fly the cold of the external Air, retires it self into the lower parts of the Earth, and there harbours in Cellars and Wells, as may be prov'd by the smoaking of water drawn from deep Wells, which argues its heat, the vapours which fly away, being, as vapours, hot in comparison of the outward Air; we may easily answer, by demanding, why, if the heat, that was harbour'd in a smoaking Bucket of water, have the wit or instinct to fly from its Contrary, it does not in the Bucket, as 'tis said to do in the Well, retire it self as far as it can from the surrounding cold of the ambient Air, but instead of retiring to the innermost parts of the water (those being remotest from that) it needlessly flies abroad, with / the vapours it excites, and does, as it were, of its own accord cast it self into the arms of the enemies it should shun. And indeed what I just now mention'd to you, as related to me by the great Duke of *Muscovies* Physician,†@ does sufficiently manifest, that the cause, why the Corpuscles, that keep Cellars warm, abide beneath the surface of the Earth in Winter, is not that they fly the cold as their enemy, but that they are pent up beneath the ground, since, when vent is given them, they immediately rush into the open Air, without fearing the cold even of *Russia* in the very midst of Winter.

53. But I shall press this no further, but rather add, that the doctrine of *Antiperistasis* is as little beholding to the following Experiment, which I sometimes tri'd, in order to the disabusing some Abettors of *Themistius*. I took then an Iron-rod, of about the bigness of a mans finger, having at one end of it a very broad and thick piece of Iron (shap'd almost like a spatule) that the quantity of the matter, might upon / the ignition of the Iron, make the heat very considerable: then having caus'd this thick end to be made red hot in the fire, and having suddenly quench'd it in cold water, I could not perceive, that the other end of the rod, by which it was wont to be held, did at all grow sensibly hot, as a favourer of *Antiperistasis* would have expected it should do to a very high degree, as presuming, that the innumerable particles of heat, that swarmed in the compact body of the red hot part of the Iron, must, to fly the cold of the water, retire in throngs towards the other extreme of the Iron, and make it exceedingly hot. And least any preexistent warmth should hinder me from perceiving an increase of heat, in case any were produc'd in the handle of the Iron, I caus'd it the

in cold water, and yet even then the immersion of the broad and candent end into the cold water, brought as little of sensible heat to the other end, that I held in my hand, as it had done the time before, and having caus'd the / Experiment to be tri'd by another, the account I receiv'd was, that it succeeded with him, as it had done with me.

54. But this is not the main thing (Gentlemen) that I intended to acquaint you with, there being an Expedient, that I purposely devised to make *one Experiment*, more considerable against *Antiperistasis*, then are the *several* mistaken *observations* of the *Peripetatics* to establish it.

55. I took then a good seal'd Weather-glass, 12. or 14. inches long, furnished with good spirit of Wine, and having provided an open mouth'd glass of a convenient shape and size, and fill'd it but to a due height (that it might not afterwards run over) with common water, I so ordered the matter, that the stem of the Thermoscope being supported by the cork, into which by a perforation or slit it was inserted, when the glass was stopp'd by the cork, the whole ball of the Thermometer was immers'd in the water, that fill'd the wide mouth'd glass, and did no where touch either the bottom or the / sides of the glass, so that the ball or bubble was every way surrounded with water. The instrument being thus prepar'd, we observ'd at what station the ambient cold water had made the tinted spirit rest in the stem of the Thermoscope, and then having provided a fit proportion of warm water in a commodiously shaped vessel, I remov'd the instrument into it, and plac'd it so, as that the external warm water reach'd to a convenient height on the outside of the open mouth'd glass: But though I carefully watch'd, whether the heat of the external water, would increase or strike inwards the cold of that water, which did immediately incompass the ball of the Weather-glass; yet I perceived no such matter, the tinted spirit in the stem keeping its station (without sinking beneath it) till the heat, after a while, having by degrees been diffus'd through the formerly cold water, by the intervention of that now warmed, the tinted spirit in the Thermometer began to ascend.

56. And to reduce the other part / too, of the doctrine of *Antiperistasis*, to the determination of an Experiment, the same Thermoscope was plac'd in the same wide mouth'd glass just after the former manner, only instead of cold water, that, which immediately surrounded the glass, was warm, and when the warmth had impell'd up the tinted spirit, till its ascent began to be very slow, I immers'd the instrument to a convenient depth in a vessel, that contain'd highly refrigerated water, mingled with divers pieces of ice. But notwithstanding my watchfulness, it did not appear to me, that the warmth of the water, that did immediately encompass the ball of the Weather-glass, was at all increas'd or intended, by that Liquors being besieg'd by water exceeding cold; for the languid motion of the tinted spirit upwards, was not hereby so much as *sensibly* accelerated (as it must have been *considerably*, if the heat of the internal water had been so augmented, or struck inwards by the cold of the external, as the Schools Doctrine would have made one expect) but / rather the ascent was by the chillingness of the contiguous water

quickly check'd, and the formerly ascending spirit was soon brought to subside again. And to give my self the fuller satisfaction about some of the chief *Phænomena* of this, and the former Experiment, I had the curiosity to observe them more then once. /

### Postscript.

*A Sceptical Consideration of the Heat of Cellars in Winter,  
and their Coldness in Summer.*

The foregoing Discourses of *Carneades* seem to have sufficiently shaken the Foundations of the

Vulgar Doctrine of *Antiperistasis*, so far forth as 'tis superstructed upon the Vulgar Observations and *Phænomena*, whereon men are wont to build it; and it seems to have also made it highly Probable, that in case some of the Examples wont to be produc'd in favour of *Antiperistasis*, should prove Historically true, yet those *Phænomena* may more congruously, to the wonted proceedings of Nature, be explicated by the detention of calorifick / or frigorifick Corpuscles, by the operation of the external cold or heat, then to a certain inexplicable self invigoration, which is commonly propos'd in such a way as invests inanimate bodies with the prerogatives of free Agents. But though *Carneades* his Adversaries seem not to have well made out the Historical part of the receiv'd Doctrine concerning cold, yet upon an impartial survey of what has been alledg'd on both sides, I freely confess, that to me some of the matters of fact themselves seem not yet so clearly determined as I could wish: for as to the obvious *Phænomena*, that nature does, as it were, of Her own accord present us, they seem to have been but perfunctorily considered, and our senses only being the judges of them, we may easily, as *Carneades* argues, be impos'd upon by the unheeded predispositions of our Organs. And as for contriv'd and Artificial Experiments, there scarce seem to have been any made fit to clear the difficulties, that invite me to suspend my judgement as to the grand Question / (of fact) whether Cellars, and other subterraneous places be really hotter in Winter then in Summer.

'Tis true, that I have scarce met with any point, wherein the modern Schoolmen seem to have so much consulted Nature, as in this of *Antiperistasis*. For inquiring what has been written of that subject, that may either confirm or oppose what has in the precedent Dialogue been deliver'd about *Antiperistasis*; I found that the curiousness and importance of the subject have made two or three of those writers less negligent then I suspected. But though I have lately met with in them an Experiment or two, that seem cogently to evince, I do not say an *Antiperistasis* in the sense of the Schools, but, that subterranean places are really hotter in Winter then in Summer, yet I must for a while longer continue my suspension of judgement, which, that

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even such persons as are circumspect themselves, may not think unreasonable, I will briefly subjoyn the grounds of my Scepticism about this matter. /

First then the learned Jesuite *Zucchius*, who is wont to be far more industrious then other *Aristotelians* (and on some subjects is careful to propose Experiments, though he be not so clear and happy in expressing his thoughts)<sup>†\*</sup> assures us somewhere, that having kept a good seal'd Weather-glass, for three years together in a good Cellar, he found the water to rise by the Coldness of the ambient Air in the Summer, and to be depressed by the rarefaction of it in the winter; which seems undeniably to infer, that whatever be the reason of it, the heat in subterranean places is indeed greater in Winter then in Summer. And another recent Schoolman, who, as I am told, is of the same order, though the learned Man publish'd his little Book under one of his Disciples Names, affirms, that he found by a Weather-glass, that a Well at the place where he lived, was colder in Summer and hotter in Winter.<sup>†\*</sup> And these assertions of *Zucchius*, and the other Jesuite, do I confess restrain me for a while from yielding a full assent to what / *Carneades* hath delivered, as to the matter of subterranean Cold and Heat. But on the other side, I am not hitherto reduc'd by these Experiments, to declare with his Adversaries against him, because of the following scruples.

First then I consider, that 'tis not universally true, which is wont to be indefinitely affirm'd, and believ'd, that Cellars and other subterranean places are hotter in Winter then in Summer. For the instances produced by *Carneades*, seem plainly enough to manifest the contrary, and my own observations made in a Cellar with a seal'd Weather-glass, do keep me from dissenting from *Carneades* as to that point. I would therefore make a distinction of subterranean places; for some are deep, as the best sort of Cellars, other deeper yet, as the *Hungarian Mines*, mention'd by *Carneades* out of *Morinus*;<sup>†\*</sup> and some again are but shallow, as many ordinary Cellars and Vaults: of these three sorts of subterranean Places, the deepest of all do not, as far as the Authority of Mineralists

above / all'dg'd may be reli'd on (for I am yet inquiring further) grow hot and cold, according to the several seasons of the year, as the vulgar doctrine of *Antiperistasis* requires, but are continually hot: The shallower sort of subterranean places, though by reason of their being fenc'd from the outward Air, they are not so subject to the alterations of it, whether to heat or cold, as open places are, yet by reason of their vicinity to the surface of the Earth, they are so far affected with the mutations, which the outward Air is liable to in several seasons of the year, that in Winter, though they be warm in respect of the colder Air abroad, yet they are really (at least some of them) as far as I have tri'd, colder in very cold weather, and less cold in warm weather. And in this opinion, I am confirm'd by two things; the one, that having purposely inquir'd of the *Polonian* Nobleman

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mentioned by *Carneades*, whether he had observ'd in his Country, that in sharp Winters small Beer would freez in Cellars, that were not very deep, but would continue fluid / in those that were, he assured me he had taken notice of it: The other thing is the Confession of the Anonymous Jesuite lately mention'd, who acknowledges, that he found but little difference between the Temperature of the water in the Well he examin'd in Summer and in Winter, though it were a considerably deep one, and adds a while after, that at *Florence*, where the subterranean Vaults are shallower, the Air is observ'd to be colder in Winter then in Summer, though at *Rome* in their deep Cellars the contrary has been found. So that the lower-most sort of subterranean cavities being, for ought appears, perpetually hot, and the upper or shallower sort of them, being colder, not hotter in cold weather then 'tis in warm, 'tis about the Temperature of the middle sorts of them, such as are the deeper and better Cellars, that the question remains to be determin'd. And thus much of my first consideration.

The next thing I shall offer to be consider'd is this, That 'tis not so easie a matter, as even Philosophers / and Mathematicians may think it, to make with the weather-glasses hitherto in use, an Experiment to our present purpose, that shall not be liable to some exception, especially if the Cellars or Wells, where the observations are to be made, be very deep. For the gravity of that thick and vapid subterranean Air, and the greater pressure, which the Air may there have, by reason of its pressing, according to an Atmospherical Pillar lengthened by the depth of the Cellar or Well, may in very deep Cavities, as well alter the height of the water in common Weather-glasses, as heat and cold do, and so make it uncertain, when the mutation is to be ascrib'd to the one, and when to the other, or at least very difficult to determine distinctly, what share is due to the pressure, and what to the temperature of the Air. And this uncertainty may be much increas'd by this more important Consideration, that not only in places where the heights of the Atmospherical Cylinders are differing, the pressures of the Air upon the stagnant water in the / Weather-glasses may be so too, but even in the self same place the instrument remaining unmov'd, the pressure of the Atmosphere may, as I have often observ'd, hastily and considerably alter, and that without any constant and manifest cause (at least that I could hitherto discover,) so that the erroneous estimate, that may be hereby suggested of the temperature of the Air can scarce possibly be avoided, without the help of a seal'd Weather-glass, where the included liquor is subject to be wrought upon by the heat and cold, not pressure of the Air. So that to apply this to *Zucchius* his Experiment, unless he had been aware of this, and unless I knew, that he had divers times made his observations, with the assistance of a seal'd Weather-glass, it may be suspected, that he might accidentally find the water in his common Weather-glass (for such a one it appears he us'd, as probably knowing no other) to be higher, when he look'd on it in Summer, then when he look'd on it in Winter, not because really the subterranean / Air was colder in the former season, then in the latter, but because the Atmosphere chanc'd then to be

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heavier: and when I remember in how few hours I have sometimes, and that not long since, observ'd the Quicksilver, both in a good Barometer, and even in an unseal'd Weather-glass furnished with Quicksilver, to rise almost an inch perpendicularly, without any manifest Cause proceeding from



cold, I cannot think it impossible, that in long Weather-glasses furnish'd only with water, or some such liquor, the undiscerned alterations of the Atmospheres pressure, may produce very notable ones in the height of the water in such instruments.<sup>†@</sup> But this is not all, that a jealous man might suspect. For *Zucchius* having, for ought appears, made his Observations but in one place, we are not sure, but that may be one of those, whereof there may be many, on which the subterranean Exhalations have a peculiar, and not languid influence; as *Carneades* has towards the close of his Discourse made probable, out of the Relations / of *Olaus Magnus*, and *Martinius*, touching the great and sudden thaws, that sometimes begin from the bottom; and thereby argue their being produc'd by copious steams, that ascend from the lower parts of the Terrestrial Globe, which may be further confirm'd, by what he formerly noted of the sudden Damps, that happen in many Mines. But that which is of the most importance about our present inquiry, remains yet to be mentioned, which is, that having had the curiosity to inquire, whether no body else had made Experiments of the same kind; I find that the learned *Maignan* had the same curiosity that *Zucchius* had, but with very differing success;<sup>†\*</sup> and therefore, though this inquisitive person do admit in his Disputation about *Antiperistasis*, a Notion, that I confess I cannot approve,<sup>†@</sup> (since to ascribe, as he does, a *fuga Contrarii*<sup>†\*</sup> to / Cold and Hot spirits, is in my apprehension to

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turn inanimate Bodies into intelligent and designing Beings;) yet he does justly and rationally reject with *Carneades*, the vulgar doctrine of *Antiperistasis*, and confirms his rejection of it by two Experiments. For first, he says, that he found with a Thermometer, that when in Winter a cold Northerly wind froze the water without doors, it was not less cold in Wine-Cellars, then 'twas at the same season, and at the same hour of the day in his Study only the Paper-shuts of his window, that regarded likewise / the North, being put to. And though, if he had said nothing else. I should have suspected, that this might have proceeded from the shallowness of the Cellars he made his Trial in, yet he prevents that suspicion, by taking notice in one clause of his Relation, that the Cellars were of the very best of their kind, in which in Summer the greatest Cold was wont to be felt. But his next Experiment is yet more considerable, which I shall therefore deliver in his own words that follow. *Expertus ego sum* (says he) *Thermometro fidelissimo, & à præcedente hyeme in sequentem æstatem prorsus invariato, instructo etiam tali aquâ, nempe in hoc ipsum ex præscripto Trebellii,*<sup>†@</sup> *ita comparata ut non exhaletur, neque minuatur, expertus (inquam) sum in supradictis optimis Cellis Vinariis maximum, quod ardentissima æstate fuit, frigus, non adæquasse illud quod ibidem erat brumali tempore, ut dixi in superiori Experimento, siquidem in Tubo Vitrei Thermometri quatuor circiter palmos longo, & in octo gradus Graduumque minuta diviso, aqua hyeme ascendit ad gradus 7. cum semisse, / æstate autem vix gradum Sextum superavit, cum tamen ad sensum multo magis vigeret frigus istud æstivum.*

Thus far this learned, as well as resolute Author, who seeming by the Mathematical part of his *Perspectiva Horaria*,<sup>†\*</sup> to be an accurate and industrious maker of observations, we may oppose his newly recited Experiment to that of *Zucchius*, which it flatly contradicts; and therefore *since* the depth of the Cellars is of great moment in Experiments of this Nature; *since* also the particular Nature of the place or soil, where the Cellar or other Cavities happen to be, may in some cases not be inconsiderable; and *since* lastly, neither *Zucchius* nor *Maignan* seem to have been aware of the differing weights of the Atmosphere, in the self same place, (as not having seen the XVIII. of our *Physico-mechanical Experiments*,<sup>†\*</sup> before which I never saw nor heard of any thing publish'd, or otherwise written to that purpose) I hope I shall be

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excus'd, if I retain some scruples about the Historical Question I have been considering, till the / Experiment have been carefully made, for a competent space of time in several places, and that not with common Weather-glasses (like those us'd by my two learned Authors) wherein the liquor may

be made to rise and fall by the differing gravities of the Air, but with seal'd Thermoscopes, wherein the alterations may more safely be suppos'd to proceed only from its heat and cold.

And to conclude, since *Carneades* has speciously enough answered the other Observations, that are wont to be produc'd in favour of the *Aristotelian Antiperistasis*, if *Maignans* relation be better warranted by future Experiments, then that of *Zucchius*, it will very much disfavour the whole Doctrine it self, which seeming to have been devis'd, but to give an account of the *Phænomena*, to which 'tis wont to be appli'd, considering men will be but little invited to imbrace it, if the *matter of fact* be as little Certain as what is propos'd in the *Hypothesis* is Intelligible.

FINIS. /  
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### An Examen of Mr *Hobs's* Doctrine touching *Cold* — 500 —

In the original edition, the following section of text is preceded by the advertisement (only) printed on an inserted signature which was clearly intended to appear at this point (see above, p. 460). The title-page has here been added for the reader's convenience.

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### An Advertisement.

The Author of the following Discourse intending it should make a part of certain Considerations upon the four famousest *Hypotheses*, or Opinions, of the Nature and Cause of Cold; which (Considerations) he thought fit to reserve for the latter end of the History of that Quality, was invited to suppress it ever since the former part of the year, that preceded the last. And though this Discourse, (both for other Reasons, and because he found it more ready and finished, then some other Papers, that belonged to the same part of the newly mentioned History) comes abroad unaccompanied; yet he judged it not amiss, to intimate thus much, That the Reader may be informed, / upon what Account Mr. *Hobs's* Opinions come to be examined in a Historical Treatise;<sup>†\*</sup> and may not wonder, either to find, that divers passages of It are omitted, that are unfavourable enough to Mr. *Hobs's* Doctrine, or to meet with in a Discourse postpon'd to the *History of Cold*, some Experiments, that seem to argue it to have been written before they were cast into the Order, wherein they now appear. To this I have nothing to add, but that whereas through haste the Scheme referred to in the long citation out of Mr. *Hobs's*,<sup>†\*</sup> has not been added to the others, that belong to this Book, I am not much troubled at the Omission, (as also that in other Quotations the place is not always as well mentioned as the words,) because, that if any shall be found, that after having considered, what I urge against the (Great, but Imaginary) Interest, Mr. *Hobs* would ascribe to Winds (whether he explicate their causes rightly or not) not only<sup>†\*</sup> in the Production of lesser degrees of Cold, but, (how improbably soever) of congelation / it self, shall think the sight of that Scheme of any Importance: this Learned Mans Book *De Corpore*, is in so many hands, that any Reader that shall desire it, may very easily have an opportunity to consult the Scheme in the particularly cited place. /

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### An Examen of Mr *Hobs's* Doctrine, touching *Cold*.

1. Mr. *Hobs's* Theory concerning Cold, does to me, I confess, appear so inconsiderately pitch'd

upon, and so slightly made out, that I should not think it merited, especially in an Historical Treatise, a particular or sollicitous Examination, but that in proposing it, he scruples not to talk to his Readers of his Demonstrations; and the preference, he is wont to give himself above the Eminentest, as well of Modern as of Ancient Writers, has had no small effect upon many, who not knowing how indulgent some writers are wont / to be, to the issues of their own brain, as such are apt to mistake Confidence for Evidence, and may be modest enough to think, that their not discerning a clearness in his Explications and Reasonings, is rather the fault of their Understandings, then of his Doctrine. Mr. *Hobs* delivers his Theory in the seven first Articles of the 28. Chapter of the fourth part of his Elements. But because the whole discourse is too long to be here transcrib'd, and because in the 2, 3, and 4. Sections, that which he treats of, is the generation of winds, and that which he handles in the fifth, is the notion of a hard body; we may safely leave out those four Sections, especially since, though there be in them divers things about the motion of the Sun, and other matters, that are more strongly asserted then prov'd, yet his doctrine tending but to shew how the winds are generated, though it were granted, would make but very little, if any thing at all, towards the evincing of his Theory about cold.

2. And that we may not be suspected / to injure his opinion or his arguments, we will, though the Citation will be somewhat prolix, first recite them as himself delivers them in those three Sections, that treat immediately of Cold, and then we will subjoyn our Animadversions on them.

3. [These things (says he<sup>†@</sup>) being premis'd, I shall shew a possible cause, why there is greater cold near the Poles of the Earth, then further from them. The motion of the Sun between the Tropicks, driving the Air towards that part of the Earths superficies, which is perpendicular under it, makes it spread it self every way; and the velocity of this expansion of the Air grows greater and greater, as the superficies of the Earth comes more and more to

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be straitned; that is to say, as the Circles which are parallel to the *Æquator* come to be less and less. Wherefore this expansive motion of the air, drives before it the parts of the air, which are in its way continually towards the Poles more and more strongly, as its force comes to be more and more united, that is to / say, as the Circles which are parallel to the *Æquator* are less and less; that is so much the more, by how much they are nearer to the Poles of the Earth. In those places therefore which are nearer to the Poles, there is greater cold, then in those which are more remote from them. Now this expansion of the air upon the superficies of the Earth from East to West, doth by reason of the Suns perpetual accession to the places which are successively under it, make it cold at the time of the Suns rising and setting, but as the Sun comes to be more and more perpendicular to those cooled places, so by the heat, which is generated by the supervening simple motion of the Sun, that cold is again remitted, and can never be great, because the action by which it was generated was not permanent. Wherefore I have rendred a possible cause of cold in those places, that are near the Pole, or where the obliquity of the Sun is great.<sup>†\*</sup>

4. How water may be congealed by Cold, may be explained in this manner.<sup>†@</sup> Let *A.* (in the first figure) represent / the Sun, and *B.* the Earth; *A.* will therefore be much greater then *B.* Let *E. F.* be in the plain of the *Æquinocial*, to which let *G. H. I. K.* and *L. C.* be parallel. Lastly, let *C.* and *D.* be the Poles of the Earth. The air therefore by its action in those parallels will rake the superficies of the Earth; and that with a motion so much the stronger, by how much the parallel Circles towards the Poles grew less and less. From whence must arise a wind which will force together the uppermost parts of the water, and withal raise them a little, weakening their endeavour towards the Center of the Earth. And from their endeavour towards the Center of the Earth, joyned with the endeavour of the said wind, the uppermost parts of the water will be press'd together and coagulated, that is to say, the top of the water will be skinned over and hardened, and so again the water next the Top will be hardened in the same manner, till at length the ice be thick. And this ice being now compacted of little hard Bodies, must also contain many particles / of air receiv'd into it.

As Rivers and Seas, so also in the like manner may the Clouds be frozen: For when by the ascending and discending of several clouds at the same time, the air intercepted between them is by compression forced out, it rakes, and by little and little hardens them. And though those small drops (which usually make clouds) be not yet united into greater bodies, yet the same wind will be made, and by it, as water is congealed into ice, so will vapours in the same manner be congealed into snow. From the same cause it is, that ice may be made by art, and that not far from the fire: for it is done by the mingling snow

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and salt together, and by burying in it a small vessel full of water. Now when the snow and salt (which have in them a great deal of air) are melting, the air which is pressed out every way in wind, rakes the sides of the vessel; and as the wind by its motion rakes the vessel, so the vessel by the same motion and action congeals the water within it.

5. We find by Experience, that cold / is always more remiss in places where it rains, and where the weather is cloudy (things being alike in all other respects) then where the air is clear. And this agreeth very well with what I said before; for in clear weather the course of the wind, which (as I said even now) rak'd the superficies of the Earth, as it is free from all interruption, so also it is very strong. But when small drops of water are either rising or falling, that wind is repelled, broken and dissipated by them; and the less the wind is, the less is the cold.

6. We find also by experience, that in deep Wells the water freezeth not so much, as it doth upon the superficies of the Earth. For the wind by which ice is made, entring into the Earth (by reason of the laxity of its parts) more or less loseth some of its force, though not much. So that if the Well be not deep, it will freez, whereas if it be so deep, as that the wind, which causeth cold, cannot reach it, it will not freez.

7. We find moreover by experience, that ice is lighter then water, the / cause whereof is manifest from that which I have already shown, namely, that the air is receiv'd in, and mingled with the particles of the water, whilst it is congealing.]

8. To examine now Mr. *Hobs*'s Theory concerning Cold, we may in the first place take notice, that his very Notion of Cold is not so accurately, nor warily deliver'd. I will not here urge, that it may well be Question'd, whether the tending outwards of the spirits and fluid parts of the Bodies of animals, do necessarily proceed from, and argue heat. Since in our Pneumatical Engine, when the air is withdrawn from about an included viper (to mention no other Animals) there is a great intumescence, and consequently a greater indeavour outwards of the fluid parts of the body, then we see made by any degree of heat of the ambient Air, wont to be produc'd by the Sun. This, I say, I will not insist on, but rather take notice, that though Mr. *Hobs* tells us, that to cool, is to make the exterior parts of the body indeavour inwards: yet our Experiments tell us, that / when a very high degree of Cold is introduc'd, not only into water, but into Wine, and divers other partly Aqueous liquors, there is a plain intumescence, and consequently indeavour outwards of the parts of the refrigerated Body. And certainly Cold having an operation upon a great multitude and variety of bodies, as well as upon *our* Sensories, he that would give a satisfactory definition of it, must take into his consideration divers other effects, besides those it produces on humane bodies. And even in *these*, he will not easily prove, that in every case any such *indeavour inwards* from the Ambient Ætherial substance, as his Doctrine seems to suppose, is necessary to the perception of Cold, since as the mind perceives divers other qualities, by various motions in the Nervous or Membranous

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parts of the sentient; so Cold may be perceiv'd, *either* by the Decrement of the agitation of the parts of the Object, in reference to those of the Sensory; *or* else by some differing impulse of the sensitive parts occasion'd by some / change made in the motion of the blood or spirits, upon the



deadning of that motion; *or* by the turbulent motion of those excrementitious steams, that are wont, when the blood circulates as nimbly, and the pores are kept as open as before, to be dissipated by insensible transpiration.

9. It may afford some illustration to this matter to add, That having inquir'd of some Hysterical Women, who complain'd to me of their distempers, whether they did not sometimes find a very great coldness in some parts of their heads, especially at the Top, I was answered, that they did so, and one of them complain'd, that she felt in the upper part of her head such a Coldness, as if some body were pouring cold water upon it. And having inquired of a couple of eminent Physicians, of great practise, about this matter, they both assur'd me, that many of their Hysterical patients had made complaints to them, of such great Coldness in the upper part of the head, and some also along the / *Vertebra's* of the Neck and Back. And one of these Experienc'd Doctors added, that this happen'd to some of his Patients, when they seem'd to him and to themselves to be otherwise Hot. The noble *Avicen* also some where takes notice,<sup>†@</sup> that the invenom'd Bitings of some kinds of Serpents; (creatures too well known in the Hot Countries where he liv'd) made those that were bitten by them, either become or think themselves very cold. But that will perhaps seem more remarkable, which I shall further add, namely, that I know a Nobleman, who follow'd the Wars in several Countries, and has signaliz'd his Valour in them;<sup>†\*</sup> and yet though his stature be proportionate to his courage; yet when this person falls (as frequently he has done) in a fit of the stone, he feels an universal cold over his whole body, just like that which begins the fit of an Ague. And though he assures me, that the stones, that torment him, and which he usually voids, are but very small; yet whilst the fit continues, which oftentimes / lasts many hours, he does not only feel an extraordinary Coldness, but which is more strange, and which I particularly inquir'd after, cannot by clothes, or almost any other means, keep himself warm.

10. I elsewhere take notice of some other Observations, agreeable to these, by some of which we may be perswaded, that there may be other ways, besides those already mention'd, of perceiving cold, though the outward parts of our bodies were not prest inwards. And whereas Mr. *Hobs* infers, that *He*, who would know the cause of cold, must find by what motion or motions the exterior parts of any body endeavour to retire *inwards*, that seems but an inconsiderate direction. For in *compressions*, that are made by *surrounding bodies*, there is produc'd an endeavour inward of the parts of the compress

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body, though no Cold, but sometimes rather Heat be thereby generated. And I hope Mr. *Hobs* will not object, that in this case the parts do not *retire*, but *are thrust* inwards, since according to him no body at all can be moved, / but by a body contiguous and mov'd. But what I have hitherto taken notice of, being chiefly design'd to shew, that the *notion of cold in general* is not so obvious a thing to be rightly pitch'd upon, as many think, and that therefore it needs be no wonder, that it hath not been accurately and warily propos'd by Mr. *Hobs*: I shall not any further prosecute that discourse, but proceed to what remains. Next then, the Cause he assigns, why a man can blow hot or cold with the same breath, is very questionable; partly because he supposes in part of the breath such a *simple motion*, as he calls it, of the small particles of the same breath, as he will not easily Prove, and as eminent Astronomers and Mathematicians have Rejected;<sup>†@</sup> and partly because that without the suspected supposition, I could (by putting together the Conjectures of two learned Writers, and what I have elsewhere added of my own) give a more probable account of the *Phænomenon*, if I had not some scruples about the matter of Fact it self: which last clause I add, / because, though I am not sure, that further Trials may not satisfie me, That the Wind or Breath, that is blown out at the middle of the compress'd Lips, has in it such a real coldness, as men have generally ascrib'd to it; yet hitherto some Trials, that my jealousy led me to make, incline me to suspect, there may be a mistake about this matter, and that, in estimating the Temper of the produc'd Wind, our senses may impose upon us. For having taken a very good and tender seal'd Weather-glass, and blown upon it through a glass-Pipe (of about half a yard long) that was chosen slender, to be sure that my breath

should issue out in a small stream; by this wind beating upon the ball of the Weather-glass, I could not make the included spirit of Wine subside, but manifestly, though not much, ascend, though the Wind, that I presently blew through the same Pipe, seem'd sensibly cold, both to the hand of by-standers, and to my own, and yet mine was then more then ordinarily cold. So that having no great encouragement / to enter into a dispute about the cause of a *Phænomenon*, whose Historical circumstances are not yet sufficiently known and cleared, I will now proceed to add, that whatever be the cause of the effect, there are divers things that make Mr. *Hobs's Hypothesis* of the Cause of Cold unfit to be acquiesc'd in. For we see that the grand cause, he assigns of cold and its effects, is *wind*, which according to him *is Air moved in a considerable quantity, and that either forwards only, or in an undulating motion*: and he tells us too, that when the breath is more strongly blown out of the mouth, then is the direct motion prevalent (over the simple motion) which, says he, makes us feel cold;

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for, says he, the direct motion of the breath or air is wind, and all wind cools or diminishes former heat. To which words in the very next line he subjoyns,<sup>†@</sup> that not only great, but almost any ventilation, and stirring of the Air doth refrigerate. But against this doctrine I have several things to object.

11. For first, we see there are very / hard frosts, not only continued, but oftentimes begun, when the Air is calm and free from winds, and high and boisterous Southerly winds are not here wont to be near so cold as far weaker winds, that blow from the North-east.

12. Next, if Mr. *Hobs* teach us, that 'tis the direct motion of the stream of breath, that is more strongly blown out, that makes us feel Cold, he is obliged to render a reason, why in an *Æolipile* with a long neck, the stream that issues out, though oftentimes far stronger then that, which is wont to be made by compressing the Lips, at a pretty distance from the hole, it issues out of, is not cold, but hot.

13. Thirdly, Mr. *Hobs* elsewhere teaches, that when in our Engine the pump has been long imploy'd to exhaust (as we say) the Receiver, there must be a vehement wind produc'd in that Receiver, and yet by one of our other Experiments, it appear'd, that for all this in a good seal'd Weather-glass plac'd there, before the included Air begins to be (as we say) / emptied, there appear'd no sign of any intense degree of cold produc'd by this suppos'd wind, so that either the wind is but imaginary, or else Mr. *Hobs* ascribes to winds as such, an infrigidating efficacy, that does not belong to them.

14. Fourthly, we find by experience, that in hard frosts water will freez, not only though there be no wind stirring in the ambient Air, but though the liquor be kept in a close room, where, though the wind were high abroad, it could not get admittance; and some of our Experiments carefully made have assured us, that water seal'd up in one glass, and that glass kept suspended in another glass carefully stopt, to keep out not only all wind, but all Adventitious Air, may nevertheless be not only much cool'd, but turn'd into ice.

15. Fifthly, we found by other Experiments,<sup>†@</sup> that a frozen Egg, though suspended in, and perfectly surrounded with water, where no wind can come at it, will be every way crusted over with ice, in which case there is no probability, that the ice / should be generated according to the way propos'd by Mr. *Hobs*. For he will scarce prove, nor is there any likelihood, that a wind pierc'd the shell and closer coats of the Egg to get into the contain'd liquors, and freez them; and a *more* unlikely assertion it would be, to pretend, (as he that maintains Mr. *Hobs's* doctrine, must) that so very little Air, if there be any, as is mingled with the juices of the Egg, is, by the Cold, which is not wont to expand the Air (nor water, till it be ready to make it freez) turn'd into a wind subtile enough, freely to penetrate the shell and coats of the Egg, and great enough to diffuse it self every way, and turn on every side the neighbouring water into ice; and all this notwithstanding, that not

appear'd not by bubbles breaking through the water, that there is any Adventitious Air, that comes out of the Egg at all; but that also, supposing there were some such contain'd in the Egg, yet what shadow of reason is there to conceive, that the Air which was engag'd in, and surrounded / with the substances of the white, and the yelk of the Egg, must needs be a wind, since, according to Mr. *Hobs*, that requires a considerable motion of most of the parts of the mov'd Air the same way, and according to him also a body cannot be put into motion, but by another body contiguous and mov'd.

16. Sixtly, Mr. *Hobs* does indeed affirm, that all wind cools, but is so far from proving, that the *highest degrees* of Cold must needs proceed from wind, that he does not well evince, that all winds refrigerate. Nor are we bound to believe it without proof, since wind being, according to him, but Air mov'd in a considerable quantity, either in a direct or undulating motion, it does not appear how Motion should, rather than Rest, make Air grow cold. For though it be true, that *usually* winds seem Cold to us; yet (in the first place) it is not *universally* true, since some, that have travelled into hot Countries, and particularly the / learned *Alpinus*, have complain'd, that the winds coming to them in the Summer, from more torrid Regions, have appear'd to them almost like the steam that comes out at the open mouth of a heated Oven.<sup>†@</sup> And if *Marcus Polus Venetus* be to be credited, (for I mention his Testimony but *ex abundanti*) the Southern winds near *Ormus*, have been sometimes so hot, as to destroy an Army it self at once.<sup>†@</sup> And *secondly*, even when the wind *does* feel cold to us, it may oftentimes do so but by accident; for, as we elsewhere likewise teach, the steams that issue out of our bodies being usually warmer then the ambient Air, (whence in great Assemblies, / even those that are not throng'd, find it exceeding hot, and I have several times observ'd a hot wind to come from those throngs, and beat upon my face:) and the more inward parts of our bodies themselves, being very much hotter then the ambient Air, especially that which is not yet full of warm steams; the same causes that turn the Air into a wind, put it into a motion, that both displaces the more neighbouring and more heated Air, and also makes it pierce far deeper into the pores of the skin, whereby coming to be sensible to those

parts, that are somewhat more *inward* then the *Cuticula*,<sup>†@</sup> and far more *hot*, the Air turn'd into a wind seems to us more cold, then the restagnant Air (if I may so speak,) upon such another account, as that, upon which, if a man has one of his hands hot, and another not, the same body that will appear lukewarm to this, will appear cold to the other; because, though the felt body be the same, yet the Organs of feeling are differingly dispos'd. And to confirm this doctrine by an Experiment / (which has succeeded Often enough, and need not succeed Always to serve our present purpose,) we will add, that though Air blown through a pair of Bellows upon ones hand, when 'tis in a moderate temper, will seem very cold; yet, that the ambient Air by being thus turn'd into wind, does indeed acquire a relative coldness, so as to seem cold to our senses, but yet without acquiring such a cold as is presum'd, may appear by this, that by blowing the same air with the same Bellows upon Weather-glasses, though made more then ordinarily long, and by an Artist eminent at making them, we could not observe, that this winds beating upon them did sensibly refrigerate either the Air or the liquor. Though 'tis not impossible, but that in *some* cases the wind may cool even *inanimate* bodies, by driving away a parcel of ambient air, impregnated with exhalations less cold, then the air that composes the wind. But this is not much, if at all, more then would be effected, if, without a wind, some other body should precipitate / out of the air near the Weather-glass, the warmer *Effluvia* we have been mentioning, especially if the Precipitating Body introduce in the room of the displaced Particles, such as may in a safe sense be term'd *Frigorifick*.

17. Seventhly, Nor can we admit without a favourable construction, Mr. *Hobs* his way of expressing himself, where he says, as we have lately seen, that *All wind cools or diminishes former heat*. For if



we take heat in the most common sense, wherein the word is used, not only by other writers, but also by Philosophers, to make wind the adequate cause of cold, it must in many cases do more then diminish former heat. For water, for instance, that is ready to freez, is already actually cold in a high degree, and yet the wind (if Mr. *Hobs* will needs have that to be the efficient of freezing) must make this not hot, but already very cold liquor, more cold yet, before it can quite turn it into ice.

18. These things thus establisht, it will not be difficult to dispatch the remaining / part of Mr. *Hobs* his Theory of Cold; for to proceed to his sixth Section, we shall pass by what a Cosmographer would perhaps except against in his doctrine, about the generation and motion of the wind upon the surface of the Earth, and shall only take notice in the remaining part of that Section of thus much; That the most of what Mr. *Hobs* here shews us, is but, that there is an expansion of the air, or a wind generated by the motion and action of the Sun; but why this wind thus generated must produce cold, I do not see that he shews; nor does his affirming, that it moves towards the Poles, help the matter, for besides that we have shewn, that wind as such, is not

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sufficient to produce far less degrees of cold, then those that are felt in many Northern Regions, there must be some other cause, then the motion of the air or steams driven away by the Sun, to make bodies not in themselves cold, (for so they were suppos'd not to be, when the Sun began to put them in motion) become vehemently cold in their passage. / For Mr. *Hobs* cannot, as other Naturalists, derive the coldness of freezing winds from the cold steams they meet with, and carry along with them in their passage through cold Regions, since then those steams rather then the wind would be the cause of that vehement coldness; and so it might justly be demanded, whence the coldness of those cold exhalations proceeds. Besides that, 'tis very precarious and unconsonant to observation, to imagine such a wind, as he talks of, to blow, whenever great frosts happen, since, as we noted before, very vehement glaciations may be observ'd, especially in Northern Regions, when the air is calm and free from winds.

19. The account he gives in his seventh Section of turning water into ice, is the most unsatisfactory I have ever yet met with: for a good part of that Section is so written, as if he were affear'd to be understood: But whereas he supposes, that by the indeavour of the wind to raise the parts of the water, joyn'd with the indeavour of the parts of the water / towards the Center of the Earth, the uppermost parts of the water will be prest together and coagulated, he says that, which is very far from satisfactory. For first, ice is often produced, where no wind can come to beat upon the uppermost parts of the water, and to raise them: and in vessels Hermetically seal'd, which exactly keep out air and wind, ice may be generated, as many of our Experiments evince. And this alone were a sufficient answer, since the whole explication is built upon the action of the wind. But this is not all we have to object; for not to urge, that he should have prov'd, that the uppermost parts of the water must be raised in congelation, especially since oyl and divers other liquors are contracted by it, not to urge this, I say, what shew of probability is there, that by the bare indeavour of the wind, and the gravity of the superficiate parts of the water, there should be any such forcible compression made, as he is pleas'd to take for granted. And yet this it self is less improbable, then that supposing the / uppermost parts of the water to be pressed together, that pressure is sufficient to coagulate, as he speaks, or rather congeal them into ice. So bold and unlikely an assertion should at least have been countenanced by some plausible reason, or an example in some measure parallel. For I remember not any one instance, wherein any degree of compression, that has been imploy'd, much less so slight a one as this must be, considering the causes whence 'tis said to proceed, can harden any liquor into ice, or any other hard body. And in the Experiment, we have elsewhere mentioned, <sup>†</sup>@ of filling a Pewter vessel with water, and when 'tis exactly clos'd, compressing it by the knocks of a Hammer, till the water be reduc'd to penetrate the very Pewter,

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we found not that so violent a compression did give the water the least disposition to turn a hard body. And as for the way Mr. *Hobs* assigns of Increasing the thickness of ice, 'tis very difficult to conceive, how a cake of ice on the top of the water being hard frozen to the sides of the containing / vessel, and thereby severing betwixt the included water and the external air; the wind that cannot come to touch the water, because of the interposition of the hard and rigid ice, should yet be able, sometimes at the depth of nine or ten foot, or much further, to beat upon the subjacent water, and turn it into ice. And it is yet more difficult to conceive, how the wind must do all this, when, as was lately noted, the water does very often freez more and more downwards, to a great depth, in places where the wind cannot come to beat upon it at all. And as to what Mr. *Hobs* further teaches, that the ice must contain many particles of air receiv'd into it, we have elsewhere occasion to show, how erroneously he discourses about those Icy Bubbles.<sup>†@</sup>

20. The reason he assigns of the freezing of water with Snow and Salt, does as little satisfie as the rest of his Theory of Cold. For not to mention, that he affirms without proving it, that Snow and Salt have in them a great deal of air; it is very precarious to assert, that this air must be / prest out every way in wind, which must rake the sides of the vessel, for 'tis strange, that far more diligent observers then Mr. *Hobs* should take no notice of any such wind, if any such wind there were; but this is yet less strange, then that which follows; namely, that this wind must so rake the sides of the vessel, as to make the vessel by the same motion and action congeal the water within it. For what affinity is there between a wind, passing along the outside of a glass, altogether impervious to it, and the turning a fluid body, included in that glass, into a hard and brittle body. The wind indeed may perhaps, if it be strong, a little shake or agitate the particles that compose the glass, and those may communicate some of their motion to the contiguous parts of the water; but why all this must amount to the turning of that water into ice, is more, I confess, by far then I can apprehend. Especially seeing, that though you long blow upon a glass of water with a pair of Bellows, where there is not an Imaginary wind, as Mr. *Hobs*'s, but a / Real and manifest one; yet the water will be so far from being frozen, that our formerly mentioned Experiments (of blowing upon Thermometers) make it probable, that it will scarce be cool'd. And if Sea-salt do contain so much air, by vertue of which, it, as well as the Snow, produces so intense a degree of Cold, how chance that being resolv'd in a little water without Snow, it does not produce at least a far greater degree of cold then we find it to do? Besides, in the Experiment we made (and elsewhere mention<sup>†@</sup>) of freezing water seal'd up in Bubbles, though the Bubbles were suspended in other glasses, whose sides no where touched them, and the remaining part of whose cavities were fill'd some with air, and some with unfreezing liquors; what likelihood is there, that Mr.

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*Hobs*'s insensible Wind should be able to occasion so many successive Rakings through differing Bodies, as there must be, to propagate the congelative motion (if I may so call it) of the wind, through the first glass, to the included Air or Liquor, and / through that new *Medium* to the glass containing immediately the water, and through that to the innermost parts of the seal'd up water. And it might be further objected, if it were worth while, that Mr. *Hobs* does not so much as offer at a reason, why spirit of Wine, *Aqua fortis*, or even Brine, if it be of the strongest sort, are not either by this mixture, or (here in *England*) by the Wind in the open Air turn'd into Ice, as well as many other Liquors are.

21. The reason why Cold is wont to be more remiss in rainy or cloudy weather, then in that which is more clear, is not better given by Mr. *Hobs*, then by some others that have written before him: for not to mention, that I have seen great frosts, and lasting enough in cloudy, and sometimes very dark weather; that which he talks of the winds being more strong in clear weather, then in cloudy, is of no great importance, since common Experience shews, that in clear weather the Air may be very cold, and the frost very great, where no wind is felt to rake, as he would / have it, the superficies of the Earth. Nor does experience bear witness to what he not warily enough pronounces, that *the less the wind is, the less is the Cold*. There are but two *Phænomena* more, which in this Section Mr. *Hobs* pretends to explicate; The one is, that in deep Wells the water does not freez so much, as it

does upon the superficies of the Earth. But the reason of this we elsewhere take occasion to consider,†@ & therefore in this place we need only note, that Mr. *Hobs* has not rightly assigned it by ascribing it to the winds entring more or less into the Earth, by reason of the laxity of its parts; since besides that it is very improbable, that the wind should not, as he says it does not, lose much of its force by entring into the Earth at its pores, and other lesser cavities (for that seems to be his meaning by the laxity of the Earths parts) to so great a depth as water lies in several Wells subject to freezing: besides this, I say, Experience teaches us, that Wells may be frozen, though their Orifices be well covered, and the wind be thereby kept / from approaching the included water by divers yards; and very many Wells, that are subject to freez, when Northerly and Eastwardly winds reign, will likewise be frozen in very cold Winters, whether any wind blows, or not.

22. The other and last *Phænomenon*, Mr. *Hobs* attempts to explicate, is, That ice is lighter then water; the cause whereof, says he, is manifest from what I have already shewn; namely, That air is receiv'd in, and mingled with the particles of the water whilst it is in congealing. But that this is not the true reason, may be argued from hence, that if a conveniently shap'd glass-vessel be fill'd top full with water, and expos'd either unseal'd or seal'd to congelation, the ice will have store of bubbles, which, at least in the seal'd vessel, cannot by Mr. *Hobs*, who will not affirm glass to be pervious to the Air, be pretended to proceed from bubbles, that got from without into the water,

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whilst it was in congealing. And we have sometimes had occasion to manifest by particular / Experiments purposely made, how little of Air there is even in those bubbles that are generated in ice, made in vessels, where the Air was not kept from being contiguous to the water.†@

23. And thus have we gone through Mr. *Hobs*'s Theory of Cold. In his Proposing of which, we wish'd he had in Divers places been more Clear; and in our cursory *Examination* of which, we have seen that most of the particulars are either *precarious* or *erroneous*, and were they neither, yet the whole Theory would, I fear, prove very *insufficient*. Since an attentive Reader cannot but have marked, that this learned Author has past by far the greatest part even of the more obvious *Phænomena* of Cold, without attempting to Explicate them, or so much as shewing in a general way, that he had Consider'd them, & thought them explicable by his *Hypothesis*: By which he that will fairly explain all the *Phænomena* recited in the Notes we have been drawing together, and which yet contain but a Beginning of the *History of Cold*, shall give me a very good opinion of his Sagacity. /

### *A Postscript.*

Though the hast, I am obliged to comply with, keep me from annexing the Historical Papers, wherewith I had thoughts to Conclude this Book, concerning Cold; yet since the Nature of the past *Examen* gave me but little Opportunity to teach the Reader any thing more considerable, then that Mr. *Hobs*'s Doctrine is Erroneous; I am very inclinable to make him here some such little amends, as the Time will permit, for that Paucity of Experiments. And therefore since in the last Section of the foregoing History, upon occasion of an Experiment very Imperfectly, and not intelligibly deliver'd by *Berigardus*,†@ I intimate my having elsewhere Plainly set down, either the same he meant, or one of that Nature; and that with considerable *Phænomena* unmention'd by him: I chuse rather to borrow *some* Account of it from another Treatise, / to which it belongs, then not gratifie some of the Curious, to whom the *Phænomena* I shew'd them of it, seemed no less pretty then surprizing.

The way then that I us'd in making this Experiment, may be gathered from the following directions.

Take a good unslak'd Lime three parts (or thereabouts†@) of (yellow) Orpiment one part, of fair

water 15. or 16. parts; beat the Lime grossly, and powder the Orpiment (with care to avoid the noxious Dust that may fly up<sup>†@</sup>) and having put these two ingredients into the water, let them remain there for two or three hours, or longer, if needs be, remembring to shake or stir the mixture from time to time. By this means you will obtain a somewhat

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fætid Liquor, whereof by warily Decanting, or by Filtrating it, the Clear part must be severed from the rest. /

In the mean time take a piece of Cork, and having lighted it so, that it is kindled throughout, remove it from the fire, whilst 'tis yet burning, and by a quick immersion, quench it in fair water. And having by this means reduc'd it to a coal, you may (in case you have not err'd in the Operation) by grinding it with a convenient Quantity of Gum-water,<sup>†@</sup> bring it to the colour and consistence of a good black Ink, that you may use with an ordinary Pen.

Whilst these things are doing, you may take what quantity you think fit of common *Minium*,<sup>†@</sup> and two or three times it weight of spirit of Vineger (which needs not be for this purpose much stronger then phlegm, and to which even undistill'd Vineger may be a *succedaneum*) and putting the powder and liquor into a / glass-Vial, or any other convenient vessel, let them infuse over hot Embers, or in some considerably warm place for two or three hours more or less, till the liquor have acquir'd a very sweet taste.

All things being thus prepar'd, take a new, or at least a clean Pen, and write with it some such thing, as you either desire or need not fear to have read, *between* (if you please) or, which is safer, *Over*<sup>†@</sup> the Lines, which contain your secret, and which are to be trac'd with the solution of *Minium*; for this Liquor, if it be either well decanted or filtred, will be so clear, that what is written with it by a new Pen, will not be seen upon the Paper when it is dry.

Lastly, when you would show the Experiment, dip a small piece of Sponge, or a Linnen-rag (or for a need, a little paper wreath'd) in the / water, that was made with Lime and *Auripigmentum*, and with this liquor, which, though it smell ill, will look limpid and clear, wiping over the Paper, it will presently at once, both wipe out or obliterate what was written with the black Ink, and make all that was written with the invisible Ink, though perhaps in the self-same Lines, appear black, so as to be very easily and plainly legible.

This is the way, to which many years ago my Trials led me, of making this odd Experiment. For the performing whereof, if any can propose a more Easie and Better way (for I find by an Inquisitive Traveller,<sup>†@</sup> that there are more ways then one) I shall willingly learn it. In the mean time the Reader may perceive, that I did not causlessly intimate, That the learned *Berigardus*; though he would manifest a great thing in Philosophy by this Experiment, did yet either not Understand himself that part of it, he pretends to / Teach,

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or has omitted one of the main Ingredients of the water of Orpiment he speaks of.<sup>†\*</sup> For I did not find, that even by a long Infusion, nor by some Decoction of the Orpiment alone (without the Quick-lime) there would be produc'd a Liquor, either obviously fætid, or that would perform so much as a Less matter, then what that, which he mentions, should. And whereas he seems to commend this way (though but between Lines written with common Ink) for the writing of things one would not have to be discovered, and though I have yet met with no body, that having seen the Experiment, is not of his mind; yet I remember, that, when many years ago, I was making Trials concerning the several ways of making invisible Inks, my Conjectures led me to discover, that I

could very readily bring, what was written with a solution of *Minium*, to be Legible, by the help of the fire; as well as I could also detect by the same way several invisible Inks, which are believ'd to require appropriated Liquors to make them Confess / their secrets. But I must reserve the Reflections, and other particulars that relate to this Experiment, for the Treatise to which it belong'd. Only I will now add, That besides the above-specified motives to communicate what I have at present written of it, I was the rather induc'd to do so, because I had *mention'd*, but not *taught* this Experiment, in the *History of Whiteness and Blackness*;†<sup>\*</sup> and because also *Berigardus* is not the only Author of Note I have met with, that having made particular mention of the Experiment, has given the Curious but a Lane and unsatisfactory Account of it.

FINIS. /  
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## List of Boyle's Philosophical Writings

Philosophical Writings†<sup>@</sup> already publish'd  
by this Author.†<sup>\*</sup>

*New Experiments Physico-mechanical, touching the Air*, 1660. *publish'd about* Midsummer.

*Certain Physiological Essays, written on several occasions*, 1661. *in* March.

*The Sceptical Chymist*, 1661. *In* August.

*A defence of the Doctrine touching the Spring and Weight of the Air, against the Objections of Franciscus Linus*, 1662. *in the* Spring.

*The Usefulness of Experimental Philosophy*, 1663. *in* June.

*Experiments and Considerations touching Colours*, 1664. *in* May. /

Such Philosophical Writings of the same Author, as being occasionally mention'd (here and there) in the above-nam'd Books, are not yet publish'd, but (though not absolutely promis'd) by divers of the Curious expected.

*The second Section of the second part of the usefulness of Experimental Philosophy.*

*Two Essays concerning the Concealments and Disguises of the Seeds of Living Creatures.*

*Some Additional notes design'd by way of Appendix to the Physico-Mechanical Treatise.*

*Two Historical Dialogues, one concerning Flame, the other concerning Heat.*

*Hydrostatical Paradoxes made out by Physico-Mechanical Experiments.*

*An Essay of the Origine of Forms and Qualities.*

*Of the Production of Qualities (manifest and occult) by Art.*

*The Sceptical Naturalist, being a Letter about the Imperfections of Natural Philosophy, as we yet have it.*

*A Discourse of Improbable Truths. /*



***An Account of Freezing, by Christopher Merrett***

An Account of Freezing  
made in *December* and *January*, 1662  
by  
Christopher Merrett  
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In the original edition, the 'Advertisement' (written by Boyle) follows immediately after the list of Boyle's writings. We have here added a title-page for clarity.

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**An Advertisement to the Readers**

AN  
ADVERTISEMENT  
TO THE  
READERS  
OF

The following Experiments, by the Author  
of the foregoing History.<sup>†\*</sup>

At the same time,<sup>†@</sup> that the *Royal Society* required / of me an Account of what I had observed, or tried, concerning Cold, they recommended the making of Trials, about that subject, to the Learned Dr. *C. Merret*, who having dispatched what he intended, much earlier then I could bring in my far more Voluminous Papers, he long ago presented His to that Illustrious Company:<sup>†\*</sup> and since That, has thought fit to let them indear my Treatise, by their being Annexed to it, and composing a part of It; and that such a part, as much might be said of it, if after I have inform'd the Reader of its having obtained the Thanks of a Society, that is too much accustomed to receive and produce Excellent things, to be suspected of valuing Trifles, I could think it needful and proper to give those Papers any other *Elogium*. And it falling out fortunately enough, That the Doctor and I (being at some miles distance) did not communicate our Designs to one another; as I knew Nothing of what he had been doing, till I heard it publickly read at *Gresham Colledge*,<sup>†\*</sup> when far the greatest part of my Experiments / were (as is known to more Persons then one) already recorded; So I afterwards scrupulously abstained from borrowing the Trials mentioned in his Papers, to enrich mine: Which forbearance was the more easie to me, because after the first time I heard those Papers read, I never Desired a Copy, nor Had a Sight of them. By this means it happened; That besides those many Titles, which being handled at large in the History, are left untouched in the following Tract, even on those Occasions, where the Learned Doctor and I happen to treat of the same subjects;

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our Trials are but Very few of them coincident; upon which score, the Reader will meet with more Variety betwixt us, then probably he would have expected to find on such an Occasion.

Having drawn up this Advertisement about the Doctors Papers, as supposing them the very same, he presented to the *Royal Society*; upon a sight / of the following Sheets, (as they were some hours

since brought me from the Press;) the Additions I there find, make it appear necessary to say something further to the Reader. I must inform him then, that about the middle of this Winter, and about the end of *December 1664*. I presented to the *Royal Society* several Books, containing each of them Eighteen or Nineteen of the Twenty One Titles, whereof my History consists; that the *Virtuosi* might have the Opportunity of the Cold (which then began to be so strong, as to keep the Press from dispatching the rest of the Book) to examine my Experiments, and add to them;†@ and one of these being delivered to the Doctor, as the likeliest Person to make use of it, together with an Order to the Stationer, to let him have the remaining Sheets of the Book, as fast as they should from time to time be Printed; he had the Curiosity, as to Enlarge some of the things he had already tried and brought in himself; (as is intimated in the Forty Sixth Page†\*) so to make Trial of some / particulars, that I had proposed and performed, which either their Importance (as the way of freezing from the Bottom upwards, by me suggested, and the weight of Bodies frozen and unfrozen) or his Opportunity invited him to make choice of; and has been pleased to afford them place among his own Experiments†@; by which means, though the coincidence of what we deliver will appear to happen more frequently, then the Advertisement will make one expect: yet to such Readers as do not prefer Variety before Certainty, these coincident Passages will not in likelihood be unacceptable. For in those Cases, where the Events of our Trials are the same, 'tis like the Truth will be the more confirmed†@; and in Cases where / the successes are very differing, the Reader will be excited to make further Trials himself, and will be thereby enabled to judge, which Trials have been the most carefully made, and the most warily delivered. And, though I think it but a Necessary Profession for me, to say on this occasion, That I am pretty Confident of my having performed *my Duty*, as to the Historical part; yet this need not hinder, but that most of the differing successes, we are speaking of, may prove but Instances of the Truth of what I

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long since admonished the Reader (in my *Preface*,) That there are among the Experiments of Cold, *divers* that are liable to Contingencies: So that, *as* I would not have the Papers of this Learned Man comprehended in what I said, of the Jejune-ness of the Writers I had met with, who treat of Cold, in a Preface written, when I was not sure the following Papers would be made publick; *so* I hope the Reception of these Papers of this Ingenious Person will be such, as may invite / him to hasten the Publication of those fruits of his Learning and Industry on another subject,†@ which *divers* of the *Virtuosi* do not more Expect, then Desire, to have communicated to them. /

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### ***An Account of Freezing made in December and January 1662.***

Since the business of Freezing is obnoxious to many various contingencies, I must necessarily premise these following circumstances, that these experiments were made in very hard weather, yet with some alternate relaxations, the frost continuing above six weeks. And the place I chose was in stone-windows, exposed to the North, and North-east winds, and some upon the ground. The vessels were Glass-canes of several bores, earthen and pewter, small pans and porringers, spoons of pewter, and silver, glasses of various figures, as Vials, Cylindrical, round, and square, flasques, recipients, bolts-heads and some Conical ones. Most whereof by the diversity of their figure, their openness or closeness produce / various effects in freezing, as the following observations will shew. The quantity also of the liquor exposed is to be considered, for what will shew a small thin plate of ice in a small parcel of some liquors, will shew none in a greater.

The method I shall follow in delivering my observations shall be, first to run over the various liquors or bodies, whether fluid or consistent, simple or compound, &c. used in this work. Secondly, what figures observable in those ices. Thirdly, some effects arising thence. Fourthly, some

properties and qualities. Fifthly, some lets or helps both to freezing and thawing. Sixthly, some uses of ice.

In pursuance of which particulars, I had recourse to those ingenious *Quæries* of Mr. *Henshaw*, registred in your *Cimelia*,<sup>†\*</sup> and then to *Bartholinus* his late Book *De Nive*,<sup>†\*</sup> and to my own collected notes from various Authors, adding whatsoever trials I thought meet. And in all these I have barely set down matter of fact, neither mentioning the Authors nor / their errors, which would have been both nauseous and tedious, nor shall I endeavour to render a reason of the various *φαινόμενα*<sup>†\*</sup> (which cannot be done without a volume) but shall leave that province to an Honourable person of this Society, who hath had much experience and reflections on this subject.<sup>†\*</sup> And now to my task. As to my first head of things used, I shall begin with

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common water, which I exposed in a triple state, in like quantities, and in open pans, viz. first cold, secondly, boiling hot, thirdly, an equal mixture of both the former. The effect was this, the cold was frozen in one hour, the boiling hot in two hours, and the mixt in hour 1. and  $\frac{1}{2}$ ; but with this difference, that the cold did freez first at the top, and sides, and had a large thick crust before there was any shew of ice in the boiling hot; but the mixt and boiling hot began to freez first at the bottom of the vessels, and when the top was cold then it freezed there also, leaving betwixt the bottom and top of the vessel a cavity for the water, which in time was wholly / converted to ice. The same succeeded most manifestly in these waters, powred on a smooth table, where the cold water was presently frozen before the boiling hot water could become cold at the bottom.

Water exhausted of air in Mr. *Boyles* engine was frozen almost as soon as a like quantity expos'd in an open pan. The ice whereof appeared white, and to consist purely of bubbles. The glass used was a four ounce round vial, and a small Tube one foot long half filled with water.

Fair water wherein *Arsnick* had been infused eight moneths, congealed much sooner then a like quantity of water, into very white ice.

Solutions of all the sorts of *Vitriols* freezed sooner in pans and Tubes, then water or any other solution of the other salts by much, though that of Alume came very little short of it. The ice kept both colour and taste upon the least touch of the tongue, in all of them.

A solution of Alume did freez into an ice whiter then milk, and stuck so close to the sides of the pan, that it / could hardly be separated from it: this was the firmest ice offered to me in all my trials, next to which in both these qualities were the *Vitriols*, especially the *Roman*.

*Sandever* quickly freezeth, Frit sooner then it, and Kelp then them both; all of them into lumps very white, and consequently not Diaphanous.

*Sal Armoniac* shewed some variety in point of time, for in the same pan, quantity, and place with the other salted waters 'twould for the most part freez long after the former, though once it did freez before them.

Common salt two drachms dissolved in four ounces of common water (for that proportion I observed in all my solutions) did in 30. hours space in the hardest season turn to pretty hard and white ice, whereas the former solutions became so in two or three hours at the most.

A beer-glass was filled with stinking Sea-water full of salt, which within 26. hours acquired at the top a plate of ice of the thickness of an  $\frac{1}{2}$  a Crown piece, with few bubbles in / it. This tasted salt and stinking as before, but being dissolved at the fire, or thaw'd of its self, the stinking taste was gone, but the saltish continued. The residue in the glass within four days (the season continuing)

and plates taken off (once in 24. hours) was frozen throughout, but that at the bottom of the glass seem'd to have a much brisker taste then that at the top, neither was it so firm and friable as that. I tried another beer glass with the same water, which froze most part of it, but the season continued not so constantly sharp so long together, as in the former experiment,

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and therefore I could conclude nothing therefrom. But in small broad earthen-pans set in ice in 36. hours the same water became ice throughout, and with the addition of a parcel of ice or snow much sooner.

Some water was impregnated with as much bay-salt, some with as much Salt Petre, some with as much *Sal Armoniac* as the water was capable to receive, and neither of these did congeal with the highest degree / of cold, continued six days together.

A solution of salt of Tartar soon converted into ice, but in much longer time then common water. I observed that it began to freez in a Tube at the top, bottom, and sides first, leaving the liquor in the middle unfrozen, whereas other solutions and liquors congealed uniformly, by descending, or ascending, or both at the same time, from side to side through the middle: of this I made but one Trial.

Salt Petre required 28. hours in a very cold season, and in that time became in the open pan a most pure white ice perfectly like *Sal Prunellæ*, which an Apothecary mistook it for. This ice thrown into the fire (after the aqueous humidity was evaporated) did sparkle as that salt useth to do. A strong *Lixivium* made hereof with an addition of Cooperas or Alume singly, or mixt, set in snow and salt, or snow alone, was frozen in one night.

*Sal Gem* alone of all the salts, though snow and ice were mixed with it in great proportion, and / though the pan was set in salt and snow, could not all that time be brought to congelation: an odd experiment. Phlegm of *Vitriol* did freez sooner then the solutions before mentioned.

Oyl of *Vitriol* begins congelation (or coagulation rather) near as soon as fair water. A pretty large Tube was fill'd  $\frac{3}{4}$  full with this oyl, and about  $\frac{1}{4}$  thereof was frozen, the rest remaining at the bottom uncongealed. This Tube was broken in the presence, and by the command of this Honourable society, the coagulated part whereof was tasted by many then present, and concluded by all those, that it was a strong Vitriolate taste. This coagulated part was of a paler colour then the other, and both these mixed and powred into a vial glass heated it so hot, that none there could hold it. This coagulated part kept so in the air a week after all my other liquors have been thaw'd, and would in probability have continued so much longer had not the glass been broken. I exposed another lesser Tube with the same oyl, which / became frozen throughout, and required very much relaxation in the air to return to its former fluidity.

I had set a mark on these Tubes (as on all the rest, to observe their several risings) and the oyl of *Vitriol*, when coagulated, sunk more then half an inch below it, and being dissolved at the fire returned to its first station, as you also saw. And this *φαινόμενον*<sup>†\*</sup> is peculiar to this oyl alone, all other liquors rising higher then the mark.

I now come to my stronger liquors of Beer, Ale and Wines.

I exposed at the same time a flask of small Beer, and another of strong Ale, the former whereof was frozen throughout in 38. hours, but three pints of

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the Ale continued unfrozen after six days continuance of very hard weather. And the air then



disposed to thawing, I broke the flask, and with the unfrozen liquor made an excellent mornings draught at four in the morning. This Ale in colour, strength, and quickness seemed to me and the other three tasters that sate up with me, much better then when 'twas first put into the flask, and by / comparing it with some other in the house of the same barrel, we plainly found the said difference. After this I took the icy part of the Ale and thawed it at a fire, which was in all a pint of liquor (though the flagon containing three pints of liquor, was fill'd with that ice) very pale, and of a quick and alish taste, very much resembling that drink which the brewers call blew John. This ice was not so firm as that of water, but fuller of bubbles.

I assayed the same a second time, but could not by reason of the changableness of the Weather attain so great a thickness of ice as in the former. And in this also I found the same changes as before.

A beer-glass of *Hull* Ale in 24. hours contracted a crust of ice as thick as an  $\frac{1}{2}$  Crown, and proceeding as in Sea-salt water, the *φαινόμενα*<sup>†\*</sup> were the very same, all the *Laminae* taken off, appeared of the same colour and taste, and the lowest ice was the most tender. Another glass of the same Ale exposed did not freez throughout (no crust being taken off) / in five days, when my own Ale did in a like glass, both being set out together. Now the taste and colour appeared the same, or at least had no sensible difference, when they had been thawed of themselves, and when first exposed. *Hull* Ale hath a brackish taste.

Claret very strong exposed in a spoon in 35. hours hard freezing became an ice all of it, it was soft, kept its former colour and taste, soon discovering to the tongue of one who knew not whence it was, its nature, quality and kind.

Canary at the same time in a spoon exposed in 38. hours acquired on its surface an exceeding thin plate of ice as thin as the finest paper, and proceeded no farther in four days following.

Neither Claret nor Canary would shew the least sign of congelation in Tubes, much less in Bottles.

Two ounces of the best spirit of Wine exposed in an earthen pan did all evaporate in less then 12. hours, but the same quantity of Brandee left near a spoonful of insipid ice without / any taste of the spirit, which cast into the fire flamed not at all. I could discern no bubbles in this phlegmatick ice, but having interposed it betwixt mine eye and a candle, it manifested many bubbles by its shadows. *Quære*, whether this may not turn to profit in colder Countries in rectifying spirits of Wine.

We now come to consistent bodies, and shall begin with animals and their parts.

Two eyes, the one of an Ox, the other of a Sheep in one night were both totally frozen, the three humors very hard, not separable one from another, neither of them Diaphanous, as naturally they are, and the Chrystalline was as white as that of a whittings boil'd. The Tunicles, Fat and Muscles were also

frozen, as appeared by their stifness, and by putting them into cold water. The ice of the waterish and glassy humors seemed to be made of flakes.

A pint of Sheeps blood did freez at the top, and all the sides of the dish wherein 'twas put, and was nothing else but the *serum* of the blood. / This ice being separated from the blood, and thaw'd at the fire, and then again exposed congealed into a seeming membranous substance, and was taken for such by some that saw it, and so continued in a warm season, and appeared in all respects a membrane. This also was seen and registred in the Journal. The blood remaining gave me no signs that frost had taken it.

I dissected a Dog and a Cat, having lain dead in the open air, and found their entrails, nay the very heart stiff, and some little ice in the Ventracles of their hearts, and their *Vena Cava*.†\*

Milk soon freezeth into most white flakes of ice, retaining the proper taste of Milk: these flakes are soft and manifest not many bubbles.

Several Eggs were exposed, and both yolk and white in one night were hard frozen. They require a longer time to freez then Apples do. The best way to thaw them both is to lay them on *Newcastle*-coal, or in a deep Cellar. Whether Eggs once frozen will produce Chicken or no, / I cannot say, but have been told by good house wives they will. Some affirm that Eggs and Apples put into water, the ice will be thawed within them, and the ice appear on the shell and skin. 'Tis true, if you hold either of them near the surface of the water, they will soon gather a very thick crust upon their outsides, but if you then break the one or cut the other, you shall see them full of ice, and the Eggs then poched will taste very tough. So that this ice seems to be gathered from without, and not to come from within. And besides if it did so, they must needs lose their weight, the contrary whereof will anon appear. But for the more surety I proceeded to this farther experiment. I immersed in my Cistern an Egg and an Apple two foot deep into water, and there suspended them with strings tied about them to keep them from sinking for the space of 24. hours, and then took them out and opened them. I could never observe in that time, though I often looked at them, any ice on their outsides, and the one being broken, / and the other cut, were found both of them full within of ice.

The next order shall be Vegetables, and of them a few instances, especially of those which are of a biting or sowre taste. Now for the first I employed the roots of horse-raddish and Onions (for other edible roots and plants every one knows will freez) which shewed the frost had taken them by their taste, and ice was found betwixt each of the skins of the Onions retaining the taste of the root; yet I have observed Beer wherein Horse-raddish and garden-Scurvigraass have been infused, will not freez so soon as other stronger Beer without them.

Oranges and Limons frozen have a tough and hard rind, their icy juices lose much of their genuine taste; they were both frozen hard in 26. hours or a little more, having a thick rinde. They as other fruits, when thawed, soon

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become rotten, and therefore the Fruiterers keeps them under ground in low Cellars and cover them with straw, as they do their Apples. /

Which did exposed in one night freez throughout. If you cut one of them through the middle, 'twill have on both the plains a most pure thin ice hardly discernable by the eye, but easily by the touch, or by scraping it off with a knife. The cores of these Apples soon turn brown, and begin their corruption there.

Oyl exposed did acquire the consistency of butter melted and cool'd again; but in Caves and Cellars I could never see it more then candy.

Strong White-wine Vinegre did all soon freez in a Tube, and without any apparent bubbles.

And to conclude without mentioning Nuts, Bread, Butter, Cheese, Soap, and many other things which came under my trial, 'tis most certain, that whatsoever hath any waterish humidity in it, is capable of congelation: what are not you have in the next Paragraph.

Having now done with what will freez, I shall briefly recount some things whereon the cold hath no such effect.

We mentioned before spirit of / Wine, add to it such strong waters as are made of it, *viz. Aqua*

*Mariæ, Cælestis, &c.* and Canary Wines in larger vessels. Secondly, the strong Lees of Soap-boylers, and others made of other salts, to which refer the spirits extracted from salt; *Vitriol*, Salt Petre, *Aqua fortis*, and spirit of *Sulphur*, which last precipitated to the bottom of the Tube, a small quantity of powder very like in colour to *Sulphur Vivum*, which being separated from the spirit (for nothing of that evaporated) cracked between my teeth, and tasted like Brimstone, and being put into water, made it as white as *Lac Sulphuris* doth, but 'twould not flame, perhaps because too much of its strong acid spirit was mixed with it. Spirit of Soot afforded also a precipitation or sediment (the spirit not congealing) at the bottom of the Tube of a yellowish colour, but much bitterer then the spirit its self, and inflamable also.

But here 'tis to be observed that the said spirits that would not freez alone, yet with the mixture of about 12. / parts of water, or less of ice, or snow, did freez throughout; except the spirits of Salt, of Nitre, and *Aqua fortis*, which would not freez with those quantities of water, ice, and snow. I intended to have tried them with a great quantity of the said ingredients, but the weather failed me.

Whether the salt water freez in the Sea, I cannot experimentally determine, but shall add what was told me by one that said he had dissolved ice in the Northern Seas, and found it very salt.

The next proposed was the figure of liquors frozen; wherein I shall observe in general, that most of the liquors differed one from another in their figures, and being permitted to freez, and thaw often, they still returned to the same figure, most whereof were branched. Alume appeared in lumps, Salt Petre, Tartar, milk, Ale, Wine, and *Sal Armoniac* in plates, and other liquors mentioned to freez into a very soft ice, seeming to be made up of small *globuli* adhering each to other. Fair / water kelp and the frits resembled an oaken

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leaf, the leafie parts being taken away, and the fibres only remaining, the *interstitia* being fill'd up with smoother ice. The middle rib (if I may so say) as in plants was much bigger then the lateral ones, all which seemed but different *stiriæ*, whose points extended towards the outside of the vessel containing the water, and made acute angles with the middle rib towards the lesser end of the said leaf.

Concerning the figures of frozen Urine I shall say nothing, the accurate description of curious Mr. *Hook*<sup>†\*</sup> having so fully and truly performed that part of my task.

Now as to the famous experiment of *Quercetan*,<sup>†\*</sup> and affirmed by many other Chymists, I made experiments in these following Vegetables, Rosemary, Rue, Scurvigraass, Mints, and Plantane, wherewith I thus proceeded, I mixed with ½ a pint of their distilled waters ½ or ¾ of an ounce of their own salts, the Rosemary and Rue were calcined, and their salts extracted with their own waters, and / then were added to their salts their own distill'd waters in the above mentioned proportions. The glasses wherein the Rue and Plantane were put, being seal'd with *Hermes* seal,<sup>†\*</sup> and the other glasses left open. The effect was, that neither of them shewed the least resemblance of the plants, from which they were extracted, neither figure nor shew of roots, stalks, branches nor leaves, (but only a lump or heap of small *globuli*) much less of flour or seed. Besides the kelp frozen hath many fibres, which is made the most of it of *Alga Marina*, whose leaf is long and smooth without fibres in it. This one thing I cannot pretermit, that the sented waters seemed upon their thawing to have acquired, and advanced much in their sents, and especially the Rosemary, whose salt hath no smell, and its water but little; yet thawed, they smelt as strong almost as fresh leaves rubb'd and smelt too.

A large recipient was fill'd with water, which being frozen throughout, and the upper crust of the ice broken, there appeared in the middle / of it a multitude of thin *laminæ* of ice, some more some less wide, from which proceeded *stiriæ*, or teeth pointing inwards, and set at pretty equal distances, so

that the *laminæ* and *stiriæ* resembled very much so many combs placed in no order, some lying directly, others obliquely, none transversely, having intervals betwixt each of them; betwixt some of them I could put my finger without breaking the points of the *stiriæ*: these combs were placed round about a cavity in the middle of the receiver, sufficient to receive two of my fingers.

In a flask filled competently with water, when 'twas frozen there appeared throughout the ice infinite silver-coloured bubbles, very like unto tailed hail-shot of several sizes, the largest about  $\frac{1}{4}$  of an inch long, where thickest, of the bigness of a great pins-head, others much less in all dimensions. The

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points of them all looked outwards, and the bigger part inwards towards the Centre, where also were the largest. For there they would easily admit a little / pin into all their cavity, without the least resistance. The figures of them were pretty regular, first a small thread, and then a head as big as a shot, and thence gradually ended in a point. Some of these were straight, most a little crooked. There was a cavity in the centre of this ice filled with unfrozen water, from which I could find multitudes of cavities of bubbles, not fully formed. And in the more solid parts of the ice cut, you may discern them by a black spot where the hole enters into the cavity. All the same *Phænomena* appeared in a second trial, but that the bubbles were shorter and larger, and not so sharp pointed. The like I also observed in a Conical glass seal'd up.

The next thing to be treated of, is the effects of freezing, *viz.* the expansion of liquors frozen, and consequently thereunto the breaking of bodies wherein they are inclosed. All the liquors tried did sensibly in glass Tubes rise beyond my mark, before the liquors could sensibly be discerned to freez, and after rose somewhat higher with freezing. The / height of the rising I shall here set of a few experiments, instead of many made (having troubled your patience too long in the former Paragraphs) in several processes. Vinegre and Urine rose about half an inch, and Lees made with salts of Rosemary, kelp, the frits, about  $\frac{1}{4}$  of an inch. Solutions of Alume and Copperas somewhat less, and in general the saline liquors less then water, which rose a full inch, and small Beer in a very narrow Tube four inches, but water in the small capillary Tubes could not be perceived, either to expand its self, and certainly not to freez at all. Oyl of *Vitriol* alone (as hath been said) sinks below the mark. Hot water put into a Tube first sinketh till 'tis cold, and then riseth before it freez.

Open-mouth'd glasses, such as Beer-glasses, &c. fill'd with water up to the brim, when frozen, the ice will manifestly rise above the superficies, and make a solid triangle there. But narrow necked glasses more plainly shew this rising. In a flask filled with water four inches below / the mouth, the ice rose above the mouth, and hung two inches without it. And once in a Bolthead the ice rose five inches above the watermark. And here I shall briefly add two things, first, that if glasses be fill'd about  $\frac{2}{3}$  full they seldom break, but if more they will for the most part break. Secondly, that round figured or spherical glasses for the most part break in an uniform manner. I fill'd a Bolthead full to the neck, and stopt it at the top, which was 12. inches distant from the body, with a piece of melted candle. The ice rose above three inches in the neck, and the glass brake in the thinnest part of the body, from the point of breaking, as from a pole, the cracks run as so many meridians, but unequally distant each from other, and consequently concurred not in an opposite pole on the other side, besides there was great difference in the length of those cracks, none whereof went round the glass. In a flask thus crackt, in many places the cracks were very irregular in all the places, for some of them ran from their centres / upwards, others downwards, some somewhat parallel, but most obliquely, and few of them were considerably straight. Glass-bottles, and

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especially stone Jugs keep very little, and the last no method in their breaking; the same also befalls square glasses: woods follow their grain, and metals no order at all.



And now I come to some remarks proceeding (as I said) from this expansion, viz. the breaking of the vessels, or force of freezing, wherein also you may take notice of that quality of cold, mentioned by the Poet, *penetrabile frigus*, piercing where light comes not.†\*

Two Oval Boxes, the one of Box, the other of Maple (both firm woods) containing each above two ounces of water, were fill'd full, and with screws closed very fast, both these Boxes were rended from the bottom to the top in one night, with gaps big enough to receive a barley corn into them; these woods stretch but little, and therefore break more surely, and with larger rents then softer wood will do. /

Secondly, a Pepper Box of Laton made of Iron, covered with Tin, had the neck broken off, and holes made in the top near the neck; and the bottom, where 'twas souldred, was so dissevered that water would easily run out there.

Leaden pipes laid above ground were broken in many places. One I saw 20. yards long broken in seven places, and another in my Cellar six yards long broken in two places. I saw likewise in many places of this City Leaden pipes, above a foot deep under ground, broken in several parts.

Cocks of Cisterns, and other brass Cocks, and also the barrels in pumps made of brass or lead, usually break with the frost.

I exposed a Copper Box of a pear fashion, which did bear three several freezings, by reason of the great extensibility of that metal, but at the fourth assay it crackt all along one side of it, almost to the screw.

Next I tried a Cylindrical silver Ink-horn, but that did bear five trials, and therein I could perceive neither / crack, nor dilatation of its superficies. I intended to have tried it in a small bottle, but the weather fail'd me. I exposed also a round silver ball of the bigness of a large Nut, the silver became very sensibly extended to a larger superficies, but did not suffer any solution of its continuity.

Tobacco-pipes, and all earthen ware taking any frost in their drying (before they are burnt) become very brittle, and being put into a strong fire will certainly break into many pieces. Tyles of houses, and hard stones in buildings, scale and break off upon thawing, and thence 'tis that the Northern sides of stone-buildings first decay, and moulder away, as 'tis most manifest in ancient magnificent structures.

Alabaster and Marble having any chinks in them, frequently break with frost, and the Statuaries tell me, they never saw any solid Marble break: as for Flints, Paving-stones, precious stones, and such as will receive a polish, the bitumens, as Amber, Kennel-coal, &c. I could never see any effect on them. /

The next effect shall be that of adhæsiion, concerning which take the following experiments.

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A smooth piece of ice was laid on a smooth Table, and common salt throwed upon it, the effect was, that the ice stuck so firmly to it, that it could not be severed from the table, without breaking the ice into many small pieces; & 'twill continue in this close cohæsiion till the salt hath corroded through the ice to the very table (making many holes in the ice) and hath melted it to the very bottom. But if you lay salt first upon the table, and ice upon it, then the ice sticketh not, but thaweth. These following salts applied (as before common salt was) cause adhæsiion to the table, but not so firm as it, viz. *Kelp*, *Sandever*, *Sal Indus*, *Gem. Prunellæ* and *Armoniac*, and Pot-ashes, but not Alume or *Vitriol*.

The next experiment of adhæsiion was this; I held a nail betwixt my lips in the open air a very little

space, which stuck so firmly to them, that I could not pull it thence without difficulty and pain. /

Another effect is concentration of spirits and colours. Concerning the former you have already as much as I know, especially in the Paragraph of freezing Beer and Ale. Concerning the latter take these following trials. Cochanele was boiled in water to a very high tincture, and frozen, and to twice four ounces of this decoction was added in one glass a little spoonful of spirit of Wine, and in another as much Sea salt-water. All these were frozen throughout, and every part of this ice seemed to me of an equal colour, though the edges, as thinner and nearer the light appeared of a brighter colour (as they do unfrozen) but the glasses being broken, shewed no discernable difference in any of them, neither as to colour nor taste. The like trials were made with Maddes weed and Indico, and the success was the same.

Secondly, I exposed a pint Porringer full of the decoction of soot, which (the air relaxing) did only freez an inch thick, this continued above a week consistent (in a thawing season) and very solid. Some / that saw it judged it to be brown Sugar Candy, the taste whereof was near, is not altogether as strong as the uncongealed liquor remaining at the bottom. And in another trial, when the whole was frozen, no concentration was seen. But though it was not my hap to find this effect, my trials having been made in Vials, square, Cylindrical or round, yet Mr. *Hauk*<sup>†\*</sup> a worthy fellow of this Society happily lighted on it, as you may perceive by his relation, and Schemes of his Glasses hereunto annexed.<sup>†\*</sup>

Some affirm as an effect of freezing, an addition of weight made in the bodies frozen, but this affirmation answers not my trials. For in four Eggs and four Apples fully frozen, I found the weight of them the same when frozen, and thawed, as they had before they were exposed, each of the Eggs and Apples being weighed in this triple state both severally and joynly, with the

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particular weights I shall not trouble you. Besides that freezing adds no weight, 'tis apparent in sealed Glasses, from / whence nothing can expire, and by exact ponderation of them, I could not perceive any the least difference in weight in the said triple state. This I tried several times with as much exactness as possibly I could, and still found the same event.

Another property of freezing is to render many bodies more friable and brittle, as most woods, as also Iron and Steel, as every one knoweth that hath used Crosbows in frosty seasons, and so likewise the bones of animals, and 'tis commonly observed by Chirurgions, that more men break their legs and arms in such seasons, then at any other time of the year, especially such who have been tainted with the *Lues venerea*, as *Hildanus* somewhere notes.<sup>†\*</sup>

I shall now conclude the effects of freezing by ranging them into good and bad. The good are the long preserving bodies most subject to putrifaction, healthiness, and confirming the tone of all animals, and thickning the hairs and furs of such as have them, fatten some. Besides it exceedingly clears the air, and other bodies, / as 'tis manifest by the stinking Sea-salt-water before mentioned, as also by this that follows, namely, I took six of the most musty stone-Bottles I could procure, and competently fill'd them with water, which after freezing and thawing again, became as sweet as ever they were before.

Bad effects are the killing and destroying animals, and vegetables by congealing and stopping their vital and nourishing juices, rendring them totally immovable. 'Tis observable that in *Greenland* and *Nova Zembla* nothing but grass grows, as also what was told me by Dr. *Collins* the present Physician of the Emperor of *Russia*, that no thorny plant nor thistles grow in that Countrey.<sup>†\*</sup> And this present year most of the Rosemary and Sage about *London* was wholly destroyed, besides most of the more tender Plants.

My fourth proposal was the properties and qualities of ice, some whereof my task engageth me to enumerate only, such are its slipperiness, smoothness, hardness, whereby and by its bulk and motion it breaks down / bridges, &c. its firmness and strength to bear carriages, and burdens; its diaphaneity, which is much less then the liquor of which 'tis made. For I could never discern any object, though but confusedly, a foot beyond the clearest piece of ice, by reason of the many bubbles and luminous parts within it. Which bubbles shew only shadows, but the ice its self interposed betwixt your eye and a candle, appears in many round circles, from which proceeds many rays of light, four or five or more, in the form of a Star of about a  $\frac{1}{4}$  of an inch in diameter, which so glase your eyes, you can scarcely see any thing, but bright light and shadow.

As for its penetration and thickness something hath been said above, to which I shall add, that I have seen the *Thames* ice of the thickness of eight inches, or more near the middle of the River, and on the sides much more. And in Garden walks the earth frozen near two foot deep, whereas on the

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sides of the same walks, on a richer mould, the frost did not reach much above one foot / and  $\frac{1}{4}$ , and Pipes of Lead have been broken above a foot under the surface of the ground. I shall not mention the huge mountains of ice found in the most Northerly Seas, but proceed to its weight.

'Tis generally known, that ice swims upon the water. But I have seen snow-balls moistened only with water, and then compressed with a strong force, and afterwards frozen, to sink: besides the congealed oyl of Vitriol descends in water, and common ice is frequently observed under water; whether the solutions of salts frozen will sink, was by me forgotten to observe, and whether coagulated oyl will sink in unfrozen, as *Bartholine* affirms.<sup>†\*</sup> Some affirm that snow-balls hard pressed, without addition of water, will sink, but experience teacheth me the contrary.

As for its tactile qualities, every one knows 'tis colder then water, which you may increase by adding salt unto it, or rather snow.

Smell it hath none, but it binds up that quality in all, but most spirituous bodies, which it also in some degrees refracts in them. /

Lastly, ice yields both reflection and refraction, whereof I shall speak when I come to its uses.

My fifth head was lets and helps in freezing, which I shall briefly dispatch. Those besides the North and North-east winds, the absence of the Sun, and the highest parts of houses or mountains, are the mixture of snow and salt (then which there's nothing more painfully and unsufferably cold to my feeling) as is apparant by the trick of freezing with snow and salt by the fire side, as also by the ingenious way of making cups of ice, invented by an incomparable person.

Add hereunto, that water falling or thrown upon ice or snow, soon becomes congealed. A mixture also of ice beaten into powder, and mixed with common Sea-salt (which is best) or with Kelp, Alume, Vitriol or Nitre. And here note, that vessels fill'd with water, and set upon these mixtures, begin their freezing at the bottom of the liquor, and consequently are not so subject to be broken, as those are which are not / set in these mixtures, and that the water riseth higher with, then without them. I find also, that oyl of Vitriol alone, mixed with snow or ice have the same effect, though not so powerful.

One affirms, that Salt-peter dissolved in water, and put into a Bolt-head, and long agitated, not only cools the hand exceedingly (which is very true) but also converts it to ice, yea, in the very Summer month, which answereth not my trial, though kept a whole hour in that agitation in the hardest season.

This following Experiment also I add, proposed to me.

I fill'd a Bolt-head, containing a quart of water, and set it in an Iron pan, surrounding it on every side with snow, which covered also part of the neck, and then set the Kettle over the fire, and took now and then the Bolt-head from the fire, whilst the snow was thawing, but not the least sign of freezing appeared in the water put into the Bolt-head.

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As for the helps of thawing, take this Experiment. I set in the same / Cellar three pans full of ice, one on *Newcastle* coal, a second on sand, a third on the earthen floor, they thawed in the same order they are mentioned, which was thrice repeated, and once that placed on the coal did thaw, when the other continued their ice. Seal'd glasses seem neither to promote or hinder this act of freezing. The same success I had with Eggs and Apples in my Cellar.

The last thing I shall speak to is the use of ice, you may therewith make a siphon, being fashioned and applied as usually siphons are, and this will happen, whether you make it one continued piece of ice, or two contiguous ones, for in both the water will run exceeding fast, and this siphon soon empties all the water out.

A second use is for refraction, whereof Mr. *Hook* hath given you already a learned demonstration.<sup>†\*</sup> And I having formed some smooth ice into various figures, like most of those mentioned by the Dioptrick writers, the *φαινόμενα*<sup>†\*</sup> were the very same as in the like figured glasses; but how / *Des-Cartes* made Dioptrick glasses of it I know not,<sup>†\*</sup> especially to make use of them: and lastly you may make a *speculum* of it, especially if a piece of blacked paper be placed behind it, and if you hold a candle at a convenient distance, there will appear very many *speculums* to your eye, according to the number of the bubbles contained in the ice. But I could not observe any heat proceed from ice, though cut in the true figure for burning-glasses, and exposed in naked ice, but frozen in spherical glasses 'twill heat a little.

I shall here subjoyn some propositions of learned *Bartholinus*, taken from his book *De Nive*, being near to the former Argument, who affirms.<sup>†\*</sup>

1. That the more subtile distilled spirits gain a clear splendor and elegancy from snow placed about them.
2. That the rays from snow newly fallen glitter, and excel in a kind of splendor wherewith the eyes are dazled. *Both these are true, and have / but one common cause, viz. the multitude of reflections caused by the infinite globuli, whereof every flake of snow consists.*
3. That he saw Cabbage growing in his garden, putrifie on that part, which was above the snow. 'Tis certain, that frost alone, with or without snow, hath this effect on Cabbage, being of the tribe of succulent plants, and I observed, that this year 1644. our great Houseleek or American Aloes (usually hung up in houses) kept in an upper room, was totally destroy'd by the cold. And that Apples will rot I have said before, and Houswives to prevent the rotting of Onions, commonly hang them up in their Kitchens, or keep them in Ovens, or some close place. And this present year 1662.

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I saw at Mr. Boxes, the eminent Druggists house, abundance of Squils or Sea-Onions quite rotten, they were laid not in an open, but close Garret.

4. When snow melts by the Suns heat, copious vapours from the Earth cloud the Sun. *He should rather have said vapours from the melted snow, and 'tis no wonder, that vapours cloud the Sun.*

5. Snow melts and falls off from / Ivy. *I have observed all the sorts of Ivies, and ever-greens with us, and some biting plants too, but find in them all the contrary to what is here asserted. Nay, no*



*difference hath been observed even in hoar frosts, which fall equally, and continues on all sorts of Plants.*

6. He excludes not a small portion of earth from snow, though pure, which, saith he, is manifest from distillation. *This Experiment I have found true by evaporation, which is tantamount to distillation, and indeed all melted snow leaves an earthy and foul settling behind it.*

7. Viscosity with softness is greater in new, then in old snow, and therefore 'tis brought into a mass. *Viscosity in it I understand not, its softness indeed is manifest too, by the tracks of beasts, which appear more fair, the snow not rising on the sides of the impression made by their feet (as it doth in old) but retains their perfect character.*

8. Watercresses and Scurvigraass grow under the snow in Gardens. *I apprehend not that any Plant whatsoever grows at all in hard seasons, my meaning is, that no Plant acquires any greater / bulk of quantity, but keeps at a stand only, and this Country-men affirm of grass and corn, and Gardiners of other Plants. 'Tis true many Plants will upon thawing shew a finer verdure, and if warm weather presently follow, all vegetables will thrive exceedingly. For how they should thus grow when their nourishing liquor is congeal'd, and consequently become immoveable, I understand not.*

9. Air is included in Snow, Which this way of mine to make snow, fully convinceth. *I took the whites of Eggs and beat them in the open air with a spoon, into a frothy consistence, as women do to make their snow possets, and then taking a little of this substance, and laying it on a trencher, it soon became plain flakes of snow, so that none that saw them could judge otherwise. Another accidental Experiment proves the same, for having put water into a Tube, and having long and strongly agitated it, there arose many bubbles at the top, which soon freezing (my agitation ceasing) became perfect snow. And now having here set down the way of counterfeiting, at least, if not of making snow, I will add how a pruina or hoar frost also may be imitated. I took / a Pail filled with warm water, and hung over it Hair, Moss, and a piece of Rosemary, now the atomical vapours rising from the water, fixing themselves on the Moss, Hair and Rosemary, became on them a perfect hoar frost. The like is dayly seen on the Beards and Hair of men and horses, travelling in cold Winter nights or mornings, proceeding from their breaths, steams of their bodies, or moist atoms of the Air. I tried also to make hail with drops of water, but could not hit on't, for they would never become white: Whence 'tis manifest, that hail is not drops of rain suffering glaciation in the falling, as the received opinion of Philosophers asserts.*

10. Snow abounds with fat. *This I understand not.*

11. Snow with ice swims on water. This is a clear consequence from the seventh assertion.

12. Snow-water boils meat sooner, and makes the flesh whiter. *I tried this in*

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*flesh and fish, but could find no manifest difference, either to their sooner boiling or whiteness.*

13. Snow newly fallen hath no / taste, but lying long on the ground, or frozen, somewhat bites the tongue. *My taste was not so acute, as to distinguish the biting of one from the other. 'Tis true indeed, that snow frozen doth more affect the tongue with its coldness, then snow alone.*

14. Worms are sometimes found in snow. *This neither my own observation, nor relation from others can make out.*

15. From snow by a peculiar art, a salt of wonderful strength is drawn. *He saith not this of his own observation, nor teacheth the way to extract it.*

16. After much snow plenty of Nuts. This frequently suits with the Country-mans observation, but

many times fails, such years also commonly produce plenty of Wheat, other seasons concurring.

I shall here also insert two remarks out of the same Authors concerning freezing.<sup>†\*</sup> The one is, that the great Duke of *Tuscany* distilled spirit from Wine, only by putting snow upon the Alembick, without help of fire. The second, that the Duke of *Mantua* had a powder which / soon congealed water into ice, even in the Summer.

And to conclude, take these general observations made by the command of the Royal Society, with Weather-glasses fram'd after the *Italian* mode, and fill'd in part with tinged spirit of Wine. Which I shall deliver briefly and in gross, and not each days alteration apart; I took then two of the said glasses of equal dimensions, as near as might be, and fill'd them with the same spirit of Wine, one of them I placed in my Study-window, standing North-west, the other in Mr. *Pulleyns* Warehouse under St. *Pauls*-Church, and chose there a small recess or room, which was most remote from the entrance, and the warmest in the whole Warehouse;<sup>†\*</sup> both the glasses were settled in their station the 15. of *October* 1662. the spirit in both having the altitude of three inches just. When the glass in my Study was depress'd, by the cold, an inch, I went and observed that in the Warehouse to have received no manifest change in its station. And / at a second visit the spirit was depressed  $\frac{1}{4}$  of an inch below, when that above-ground was depressed near two inches. And during the long continuance of all that hard Winter, it never descended above  $\frac{3}{4}$  of an inch, and never was higher there then three inches and  $\frac{1}{4}$  in a mild season in *April* following, by which time the papers fixed to the glass, and whereon were fixed the degrees, was quite rotten, and the characters scarcely legible. And at the same time, that in my Study was raised to four inches  $\frac{3}{4}$ . By which it appears, that the said Warehouse was in the coldest season as warm as in a mild *March*, for at that station the glass in my Study stood, commonly betwixt two inches and 2. and  $\frac{1}{2}$ . And

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so indeed this place appeared to one that went into it at the coldest season. And to this purpose I several times sent in at night my hardest frozen liquors, which were constantly thawed in the morning, though it freezed exceeding hard above ground.

The glass in my Study, after two days hard freezing, was sunk below / my marks, into the very ball, so that I could make no farther observations concerning the cold above ground.

From the former observations, that popular error is manifestly refuted, *viz.* that Cellars and Subterraneous places are hotter in the Winter then in the Summer, which though they appear so to us, because they warm us in the Winter, and cool us in the Summer, yet they are not so in themselves, for it appears by the former Experiment, that in the coldest season the spirit was depressed to two inches and  $\frac{1}{4}$ , and rose in *April* to  $3\frac{1}{2}$ , and no doubt would have risen about  $\frac{3}{4}$  of an inch higher, had it continued there till the hottest season of the year.

One thing more I observed, *viz.* that the tinged spirit of Wine had in this subterraneous Vault totally lost its colour, whereas that in my Study (two years after) still remains its former tincture.

Since the printing of the foregoing Papers, *viz.* 1664. (there being no frosts in *England* 1663.) I made / these following Experiments.

Finding the third of *January* the season disposed to freezing, I exposed a Pint bottle of Claret, and a glass-Cane filled with Canary, a solution of *Sal Gem*, Train-oyl, and the Oyl of *Fructus musæ*, and on the fourth of the same month, the night being the coldest and sharpest that I ever felt, (which all I spake with the next day confirmed) the wind then blowing hard at South-west, I found in the morning all the liquors frozen, except the *Sal Gem* exposed in an earthen pan, which shewed at the bottom of the dish some seemingly Crystallized salt, the oyl of the said fruit became very friable, and of a milky white colour, but the Train-oyl only lost its fluidity, and became of the consistence of

soft greese. And the same night a bottle of the Rhenish Wine, called Backrag, and another of lusty White-wine, standing in a room a story high, exposed to the said wind, had most of the Wine frozen in them, the ices whereof being taken out, tasted somewhat weaker then the Wine it self. All / the same things happened the sixth night of the same month. It is to be observed, that the pint of Claret, and the Sack in the tube, were both frozen throughout these two nights, and after their double freezing and thawing, they lost nothing of their spirit, colour, and taste; nay, the Claret being a strong *Burgundy* Wine, though it often suffered glaciation and thawing for three weeks together, yet in all that time suffered no manifest alteration, but appeared the same to sence, as when it was exposed, in colour, taste, and strength.

As to the concentration of coloured liquors, Mr. *Haak* shewed me an Oval glass,<sup>†\*</sup> having at one end a narrow Cane above an inch long, almost filled with water, tinged with Cochineel, frozen throughout, the ice round about,

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towards the sides of the glass, shewed wholly colourless, but that in the midst was of an exceeding high dye, but the ice that was raised to the neck of the glass, was lightly tinged with a scarlet hue. Hereupon having some flasks by me, I put / into one a strong decoction of Cochineel, and into another a like decoction of Soot, which being exposed to the air, and encompassed in a vessel with snow and salt, they did freez to the thickness of an inch or more, and the air then beginning to relax, I broke the flasks, and the desolved ice yielded a water totally colourless. I made also an Experiment with a very strong decoction of Gentian roots, which being exposed in a four ounce vial, the ice thereof had a far deeper colour, and bitterer taste in the middle, and towards the bottom, then towards the outsides of it.

And whereas *Barclay* relates,<sup>†\*</sup> that King *James* being in *Denmark* to fetch his Queen thence, in the Winter season had his nose and ears in danger of Gangreening, which being timely perceived by some of the King of *Denmarks* Nobility, they caused the parts to be rubbed with snow, and so the danger was avoided; the same travellers affirm, that in the Northern parts, where men become stiff with cold, and almost frozen to / death, that they rub the frozen parts with snow, or else cast the whole body into water, by which means the whole body is crusted over with ice, as Eggs and Apples are, as if the freezing Atoms did pass from the body frozen into the water or snow; and this way of curing Gangreens from cold, *Sennertus* doth prescribe.<sup>†\*</sup> To make some Experiment hereof, I exposed flesh and fish, and found, that by immersing them into water, they soon became more limber and flexible, and more easily yielding to the knife, and compassed with a crust of ice of the thickness of about half a crown, manifest tokens of their thawing, and being cut, they discovered nothing of ice in them. This for more certainty, I often reiterated, as also in Eggs and Apples, above a dozen times, and never failed of unthawing them by this way. 'Tis to be noted, if you immerse the flesh, fish, eggs, or apples deep into the water, no ice will appear on their outsides, but only when you hold them neer the surface of the water. /

As to the *Persian* Experiment mentioned by *Olearius*, of making huge heaps of ice to be preserved for cooling of their drinks,<sup>†\*</sup> I observed, that by pouring water into an open Pan, or into a Flask gradually, some at one time, some at another, I could quickly freez by this way a whole Flaskfull, when near half of a Flask filled at one, though helped by art, was unfrozen. I observed also, that the ditches betwixt *Southwark* and *Redderiff* had acquired an exceeding thickness of ice, caused by the flowing of the water in them at full Tide, for new water being brought in by the Tide, was there congeal'd to the thickness of some inches every ebbing and flowing. I observed also the ice on the banks of *Thames* above two yards thick; the inhabitants told me

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they had seen it three or four yards thick, which thus came to pass; the Tide flowing in, and meeting

with great flakes of ice, drove them to the banks, and lodged them on the ice there frozen, which flakes uniting there with the former ice, raised it to that excessive height or thickness. / Besides every one may observe in *London* Streets, and elsewhere, in Channels where no constant current is, that water coming from the houses, soon fill the Channels with thick ice, for running but a little at a time, it freezeth almost as fast as it cometh thither. Nay, I have seen ice of some yards thickness in such places, where a small rill or stream of water gently falls on the side of a hill.

Amongst those things that will freez, Mortar and Plaister of *Paris* were omitted, and thence 'tis that Plaisterers and Bricklayers play all the Winter.

My Lord *Verulam* in his natural Histroy (and some from him have affirmed to me) that Apples and Eggs covered with a wet cloath, will not freez, but I find no difference in those that are thus covered, and them that are not.<sup>†\*</sup>

Add to those that sink upon congelation, all oyls from Animals, and from Vegetables, that are extracted by expression or boiling.

Add to those that freez not water and Sugar boiled to the consistence of / a Syrup, and also all other Syrups, none whereof I could ever take notice, or learn by others, that they would freez. 'Tis true, that water having an equal quantity of Sugar dissolved in it will freez, but with a little more mixed therewith, freezeth not.

To try the effect of cold upon Loadstones, I exposed several of them in the open Air, and also within rooms in the most severe weather, the needle being kept in a warm place. At other times I exposed the needle to the cold air, keeping the stones warm, at other times both were exposed, but in none of my Experiments could I conclude any thing certain to their attractive faculty, for the sphere of their activity was found to be sometimes greater, and sometimes less, to a considerable difference, in ten several good stones imployed for this purpose.

I essayed also to find out a standard of cold, whereby to fit the tinged spirit of Wine for the Weather-glasses, and to that end made use of Conduit water, and the distilled waters / of Plantane, Poppies, Black-Cherry, Nightshade, Scurvigrass, and Horse-raddish; all which were first placed in the same room where a fire was kept, and then removed, and measured out into spoons in equal quantities, and also a drop of them dropt on the same bench, but though this was often tried, I could not make any sure inference from them, only I observed that the black-Cherry water did for the most part freez first, but the other with very great uncertainty. The Horse-raddish and Scurvigrass waters were for the most part froze last. The best way to discover the very beginning of freezing of liquors, is to move a Pin or Needle through the liquors, whereby the ice will be raised, and become discernable, when the naked eye can discover none at all.

*FINIS.* /  
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## **Appendix ... to the precedent History of Cold (1683)**

AN  
APPENDIX  
CONTAINING SOME  
Promiscuous *EXPERIMENTS & OBSERVATIONS*  
relating to the precedent History of  
COLD.

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A Great progress having been made at the Press in the Second Edition of the *History of Cold*, before the Author was acquainted with it, he did not think fit to make any alteration of the Former Edition, but left it to come forth this year, just as it was printed seventeen years ago, viz. in the year 1665.

For the same reason, the Author declin'd making any alteration in the *Introductory Preface*; whose prolixity seem'd very excusable, because it was not barely a Preface to the particular Book whereunto it was prefix'd, but (as the Title was design'd to intimate) contain'd divers Considerations Introductory to the *History of Cold* in general, and superadded divers Experiments and Observations to those that were deliver'd in the History it self./

The Author thinks he may justly hope, that Equitable Readers will not look upon the *Thermometrical* Discourses that are premis'd to the *History of Cold*, as unfit to appear again with it, tho' some of the Particulars, that are there deliver'd as *Paradoxal*, are now acknowledg'd for *Truths* by most of the *Virtuosi*; and *others*, that are propos'd as *new* Observations and Practices, are at present come into common use among the *Curious*. For the ancient date of these Discourses will easily make it appear, that the things they mainly consisted of, were *then* Novelties, And he hopes it will not disparage them among Equitable Readers, that *many* have since thought fit to embrace the Opinions, and make use of the Practices, there propos'd.

But 'twill be now expected that somewhat should be said about the following *Appendix*, wherein the Author is very sensible that he stands more in need of the Reader's equity and favour. For, not being solicited to make a Second Edition, (after Mens *Curiosity* and the Fire of *London* had dispers'd or destroy'd the First) till want of leisure made him *unwilling*, and want of health almost *unable*, to revise and prosecute that Work; he threw aside the Particulars he intended to add among other loose Papers, where for many years they

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lay neglected, and probably there were divers of them lost: so that when the Stationer and several other persons were pressing to have a New Edition of the former *History of Cold*, and gave the Author notice that a good part of it was already printed off, and waited for some *Additions* that they earnestly desir'd, he had much ado to retrieve any considerable number of Notes; most of which too being occasionally written, when he could not get one *Exemplar* to collate them with, (the Stationer himself having not so much as One Book left) it was almost necessary that many of them should not be so written as to be easily and smoothly joyn'd to the Titles of the formerly Publish'd History. So that the Author (who at that time was much indispos'd) having neither health nor leisure to put this *Chaos* of loose *Memoirs* into some order, desir'd a / learned Friend to take upon him the trouble of doing it for him: yet 'twas not possible for all that ingenious person's care and diligence, to give a good method and smooth connexion to so confus'd an heap of Particulars; all that the difficulty of the Attempt permitted him to doe, being to refer the Particulars, as near as might be, to the respective Titles they seem'd most to belong to.

It remains that the Reader be told, whence the Materials have been taken, whereof the following *Appendix* doth consist. Some few of them have been drawn out of Printed Books, because *Cold* (in it self a subject barren enough) has been left so uncultivated by Classick Authors, that, according to our judicious *Verulam's* advice, it was not thought fit to cast away any credibly related matter of fact that might add to the History of it. But the greater number by far of the following Particulars was taken from the Relations of Navigators and Travellers, whom the Author had the Curiosity to consult about the *Phænomena* of *Cold*, they had met with. And for the better gaining of such Informations, he became an Adventurer in that which is commonly called the *Company of Hudson's Bay*;<sup>†\*</sup> to which those that are from time to time sent from *London*, do, either in their Voyages thither and back again, or in their stay in that frozen Country, not unfrequently meet with

considerable, thô unwelcome effects of *Cold*. But two persons there are above the rest, from whose Answers the Author drew the considerablest part of the following *Appendix*. One was an ingenious English Physician, dead many years since, that was *Archiater* to the then *Russian Emperour*;†\* for whom our Author having furnish'd this Physician with some pleasing, and yet effectual, *Chymical Medicines*, that were very well lik'd, and nobly rewarded, by the *Czar*, the Author desir'd as his recompence, to have some *Observations* about *Cold* (whereof he sent a list) made in *Russia*, and especially at the City of *Mosco*, where the Physician, attending his Master, resided. The Answers to / Mr. *Boyle's Queries* were by misfortune not sent by themselves, but in several Letters intermingled with so many other Passages, relating to the *Russian Monarch's* Government, Religion, &c. that to put them in the Writer's own words (which was thought the fairest and surest way to prevent mistakes)

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some of them must be dismembred from the Context, and so must appear as torn and incoherent *rags*, and consequently to the great disadvantage of the Papers, they are made parts of. The other principal Informations to be met with in this *Appendix*, the Author receiv'd in divers Conferences he procur'd with an ancient *Sea Captain*, who was lookt upon as the greatest *Navigator* into the *Northern Seas*, that has been known; upon which account his *Majesty* himself had the Curiosity to send for him and discourse with him.†\* This lusty old man had made above *thirty* several *Voyages* into the *Frigid Zone*, and being then (as he still is, if he be yet alive) in the service of the Company of *Hudson's Bay*, was upon that account the more willing, and the more free, to make Answer to the Author's Questions, even when it requir'd the discovery of his most secret Observations. And, that this Preface may not be altogether useless to the design of the *History* it belongs to, I shall add on this occasion, that the Author having been visited by the principal person that ventur'd to winter in *Hudson's Bay* (where the ingenious Captain *James* (often mentioned in the foregoing History) found it almost, if not quite, as rigorously *cold*, as the *Hollanders* found *Nova Zembla*) he was particularly inquisitive to learn of this Person, how he was able to support the extreme rigours of the *Cold* all the Winter long; to which enquiry the *Navigator* answer'd, that the *Cold* was scarce sufferable the first year he settled there; but that afterwards they had found an Expedient to make their Wintering not onely tolerable, but comfortable enough: and being press'd to name this Expedient, he ingenuously confess'd it to be this; that they dug so deep into the Earth, where they thought fit to / erect their Wintering house, that about one half of their Mansion, and that part wherein they dwelt themselves, was built under ground; by which means the *Cold Air* could not laterally pierce into it, so that they slept warm enough, and in the day-time could keep themselves from excessive *Cold*, as long as they continued in that Subterranean part of their house.

The following Papers having been sent away to the Press, without being review'd by the Author after the Particulars that compose this *Appendix* were rang'd in the order they now appear in; when afterwards he receiv'd them all at once as they now stand printed, he found (not without being troubled at it) that to comply with the design of referring Particulars to their proper heads, some Passages in this new Model of them had lost the dependencies or the connexions they had in the Papers whence they were taken. As for instance, the *Governour of Smolensco* upon the borders of *Poland* and *Russia*, thô not here call'd by his own name, is mention'd as a Person formerly nominated; which might well be done in the Papers whence this Particular was extracted, because he had in them, before that, been quoted by the name of *Lieutenant General Drummond*.†\* Nor is this ('tis fear'd) the onely Passage wherein the almost necessary dislocation of Particulars, that before had a

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manifest connexion, may need the *Reader's* pardon, which is therefore begg'd by the Author; who yet hopes that these Passages will not be found numerous, and that an attentive *Reader* will by Circumstances easily enough discern what things are his own, and those comparatively *few*, that

might have been more expresly deliver'd, as receiv'd from others. This inconvenience was not espy'd till it was too late to prevent it.

Some few Particulars in the following *Appendix* may perhaps be found coincident as to the main, with some Passages of the Book 'tis annex'd to, and yet differing in some Circumstances. But these the Author thought it candid not to suppress, because in Historical matters Truth is the thing that is to be / principally regarded: besides that in these points wherein the *Relations* of the *History* and of the *Appendix* agree, they will mutually confirm each other, (which in matters whereof few Tryals or Observations are yet extant, is a thing of no small moment;) and those Circumstances that may suggest Limitations or Cautions may be of good use in the *Philosophick History of Cold*, and engage the curious to make a farther search by heedfull and repeated Tryals.

The Author had divers other Papers that might have enrich'd the present *Appendix*, if the confusion that was occasion'd among his Manuscripts, by a sudden Fire that oblig'd him very hastily to remove them after midnight, had not suppress'd them, (and which he hath elsewhere complained of, as very prejudicial to him in reference to other Tracts) at least till another opportunity. But without the prospect of a larger *Appendix* the *Printer* wanted not encouragement to press for a *Second Edition*, by the favourable reception that was given to the *First* by divers Learned Men, not onely at home, but abroad; where *Monsieur Duhamel*, famous for many curious and elaborate Pieces, in one of his learned Treatises, gives this Character of the foregoing *History*: *Cum anno superiore inciderim in Librum de Frigoris Historia, ab illustri & doctissimo Viro D. Boyle compositum, quo Argumentum, à Philosophis penè intactum tam diligenter & eruditè pertractavit, ut vix quicquam accuratius sperari possit. Duhamel Corp. Aff. Cap. de Frigore.*<sup>†\*</sup> And since him an ingenious Doctor of Physick, (that in a Cold Climate has written, thô not copiously, yet learnedly and *ex professso*, of *Cold*) speaks thus of our Author and of his *History*: *Agmen hoc eruditum* (having spoken before of the chief Authors that have written about *Cold*) *claudat Dominus Boyle, Delicium & Ornamentum nostri temporis, cui jam nunc omnem nostram attentionem renovare æquum est. Non enim aliam ob causam hoc loco ultimo eum amplectendum nobis servavi, quàm ut veluti per compendium, liberiori tamen / paulò excursione factâ, & seposito nonnihîl capitum præcedentium ordine, tum ea quæ jam partim allata fuere, tum quæ dicenda adhuc restant, sine tedio & concinnâ brevitate, Ejus quasi ductu, quem, toto hoc lubrico & glaciali itinere ducem*

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*mihi proposui, examini ulteriori subjicerem.* Conradi Dissertat. Medicophys. de Frigoris Natura & Effectibus, pag. 51.<sup>†\*</sup>

These just Elogies of our Honourable Author (to omit many others) are here inserted, not to grate on his modesty by the repetition of his own (thô merited) Praise, but as incitements to the studious to peruse his Philosophy, with the Principles thereof, contained in This and his other Tracts, (which in many things differ from those of the *Peripatus*, the *Academia*, and the *Stoa* too) in regard they have already passed the Test of the Learned both at home, and also beyond the Seas. For thô, as an Ingenious Writer speaks in another Case, little heed is to be given to the Gale of a private man's Fancy, yet 'tis considerable, when the Wind blows from all Quarters. The universal approbation, which the Labours of our Author have met with, requires an high veneration for, and medullary search into, his Writings. It was the saying of an ancient Rhetor, in reference to Oratory, *Ille se multùm profecisse sciat, cui Cicero valde placuerit;*<sup>†\*</sup> which may be applied with as much veracity to Mr. *Boyle*, whose Philosophical Lucubrations about the Subjects he is pleased to ventilate and discuss, are the Top of their kind. Therein the *Initiati* may find great encouragement for their progression, and also, Those who are more experienced, and sit but one Form below the *Adepti*, may count it no Disparagement to learn of Him, whose Disquisitions have been so Instructive to the Learned World.



**An Appendix, containing some promiscuous  
Experiments and Observations relating to  
the precedent History of Cold.**

*Particulars referable to the II. Title.*

1. The 26th. of *December* in the morning, (being an extraordinary hard Frost,) there appeared to be quite frozen in my window near the fire, a Vial full of the solution of *Minium*, [made in the spirit of Vinegar,]<sup>†\*</sup> so strong, that part of it was shot into *Saccharum Saturni*: only at the top there was a little (less than a quarter of a spoonful) that look'd yellow, and was not frozen, though, being poured out, it did not feel like an oyl.

2. A solution of *Gold*, made with Salts, and standing in the same window by the Solution of *Minium*, was also frozen.

3. There was exposed a Pint-vial full of the Tincture of *Lignum Nephriticum*, which being frozen, there appeared no colour in the ice, when the Vial was held from the light, as in the liquor before 'twas frozen.

4. The often mention'd ingenious Gentleman, Mr. *Drummond*, who was Lieutenant General of the *Russian Army*, told me, that he had divers times seen *Brandy* frozen in *Russia*, but the ice was not so hard as common ice.<sup>†\*</sup>

5. A *French Chirurgion*, that waited upon an Extraordinary Ambassadour to *Moscou*, being enquired of by me about the freezing of *spirit of Wine*, answered me, he had good store of Brandy frozen in the Bottles, and that the unfrozen part, which was retired to the middle, was a *Spirit of Wine* by great odds stronger than ordinary Brandy.<sup>†\*</sup> /

The above mentioned Ambassadour also told me, that it was usual in their Country to have *Wine* freeze.

*Passages taken out of a Letter of the Russian Emperour's Physician to Mr. Boyle.*<sup>†\*</sup>

6. The 7th. of *December*, I put some very strong *French Brandy* into a *China Cup*, such as they drink Coffee out off, and exposed it to the Air; in three

hours time it was turned into a crusty Ice all about the sides of the cup, as if some cold-blast had forced it abroad.

Water and Sallet-oil were exposed, the Oyl reduced to the firmness of Tallow, the Water not frozen.

*Particulars referable to the III. Title.*

1. The Spirit of *Sal Armoniac* made with Quick-lime, Volatile oyl of *Amber*, a small quantity of oyl of *Vitriol*, Solution of *Silver* in *Aquafortis*, some diluted with Rain-water, and a vial full of weak *spiritus sanguinis Humani*, was exposed two nights and a day to the Cold. The event was, that the three former liquors appeared not to be at all frozen; the Solution of *Silver* was frozen, and the Spirit of *Blood* appeared almost totally turned into ice, (for something seemed to be unfrozen at the



bottom, especially near the middle part of it) and the ice rose up high enough to fill the neck, and thrust out the Cork, which I found lying upon the ice above the neck. (The Glass was not hereby broken.)

2. Unrectified Oyl of *Turpentine*, though being exposed all night in a single Vial, it would not freez; yet the same quantity (which was about two or three Spoonfuls) being exposed in an open earthen Pottinger, the upper part afforded ice of a pretty thickness, and figured almost like that of frozen Urine.

3. A Traveller, bred to be a Physician, informed me, that in that part of *Scotland*, where his Father now lives, there is a Lake, out of which runs a little River, whose waters neither in the one, nor the other, are ever frozen in the midst of Winter, (which in those Northern parts is wont to be very sharp) and that lumps of Snow and Ice cast into that Lake do readily dissolve there; and yet, as he answered me, this Water doth not differ in Taste from common Water, as he divers times observed./

4. I was told by a Colonel, that the Souldiers this Winter making use of *Sallet-oyl* to keep their Locks from freezing, found they could not discharge, but being advised to Hemp-oyl they froze not; neither will Train-oyl freez, as it is told me.†\*

### *Particulars referable to the VI. Title.*

1. Lievtenant G. *Drummond* told me, that he had often been fishing on Ponds and Lakes, that had been frozen over so strongly, that men might march with Canon over the ice, and yet these Lakes were stored with live fish, as they found when they broke the ice in several places, and drew their Nets under the ice from one hole to another. The fish being drawn out, and pack'd up in this state, would continue good for a month, or better, without being either salted or dry'd.

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2. And it was remarkable, that the fishes being drawn out of the water found so great a difference between it and the cold Air, that presently after, as soon as they had made some few leaps up and down, they were in a trice frozen as stiff as boards. He told me that flesh and fish, if when it was frozen, it was thawed by the fire, would be quite spoiled, and the flesh would not only lose its natural taste, but would be incredibly hard and tough. But flesh and fish being kept in cold water, would there thaw, and become tender, and so grow fit to be dressed; yet he observed not, that in the thawing they acquired a coat of Ice, as I told him I observed frozen Eggs and Apples do in *England*, when thawed in cold water.

3. He also told me, he had frequently seen men to have the end of their noses, and the upper part of their cheeks frozen, even when themselves were not aware of it, and that they were very careful not to enter into a Stove, or come near the fire, to relieve themselves; because if they did, the frozen parts would be apt to mortifie, and come off, but they would rub them well with Snow, by which means they were thawed, though not without some trouble, yet without danger.

4. In *Lapland* they use another way to restore frozen limbs, viz. by making a certain kind of a cheese of Deers milk, which they tost against the fire, anoint the place affected with the Caws-boby, and that restores the frozen member immediately./

5. I had some *Cheshire* cheeses, of which I desired the Cheesemonger to choose the fattest and firmest to resist the frost; but they were all frozen, as also a *Chedder* cheese of a hundred pound. I threw one into cold water, and in a quarter of an hour it gather'd ice about it, or rather the water extracted the Iceickles out of it.†\*

6. Fish thaws sooner and kindlier in cold water, than in the warm Stove; but thawing in water is not

so proper to flesh, which must have a time answerable to its bulk, or else it will never rost further than it is thawed, rost it never so long, and carefully.

7. 'Tis their custom in *Russia*, especially at *Moscou*, to thaw their fish (before they put it to boyl) by letting it lay in cold water, 'till it hath got a cake of ice about it, which they take off, and then put the fish into new cold water, and, when it is covered with ice, take it out again: This they continue to do, till the fish will occasion no more ice, and unless this be first done, they find it will never be well boyled.

8. An old Sea-Captain told me, that they very often met with large white Bears, out of which they had fat enough, when well ordered, to yield a hogshead of oyl.<sup>†\*</sup>

The same Captain told me also, that white Bears in or about *Greenland*, notwithstanding the coldness of the Climate, have an excellent scent; and that sometimes, when the Fishermen had dismissed the carcass of a Whale,

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and left it floating on the waves three or four leagues from the shore, from whence it could not be seen, these Animals would stand as near the water as they could, and raising themselves on their legs would loudly snuff in the Air, and with the two paws of their fore-legs, as with Fans, drive it as it were against their Snouts; and when they were (as my Relator supposed) satisfied, whence the odour came, they would cast themselves into the Sea, and swim directly towards the Whale, as my Relator and others observe, who had sometimes the curiosity to row at a distance after them, to see whether their Noses would serve them for Guides, when their Eyes could not. He saw no other bears in those parts but white ones.

9. An inquisitive Doctor of my Acquaintance<sup>†\*</sup> bought at *Moscou* a small quantity of *Malaga Sack*, that did as it were drain out at the bottom of a Pipe or Hogshead, that had not been tapped, 'till it was (unawares to the Owner) frozen. This Liquor was much stronger and better than the Wine / that afforded it, and the fame of its goodness making others crowd to buy the remaining part of it, they found it, when the vessel was kept in a warm place, which thawed the ice, to be little else than strengthless Phlegm. The same Physician had likewise some strong *Beer* frozen, whereof the part that resisted the Cold (and was taken up neer the top of the vessel) was stronger than Wine, but the rest which had been once ice, was worth little or nothing. And as to these and the like Glaciations the same Doctor told me, that he observed not the unfrozen liquor to retire always into the middle of a vessel, but rather (especially in Sack) to be intermingled with the ice, almost (as I guess by his description) as Honey is dispersed into an Honey-comb.

10. Some of the men of the old Sea-Captaines, that had been in the frigid Zone, being on shore of *Greenland*, opened a Barrel of good Beer standing on end, and before they had drank much of it, the wind turning suddenly fair, and being hastened aboard, they left the barrel behind them, and the next year coming again to the same place to fish for Whales, some of them went a shore in a Shallop, but were by extreme cold, and the inter-position of some ice, kept for a day or two from being able to get to the Ship, by which accident their provision falling short, one of them remembred the barrel left behind, and coming to it, found it standing where they left it, but very hard frozen; whereupon they took a Spit they had with them, and made a good fire, and therein heating it red hot, they broached the frozen barrel with it, and when the Spit had reached almost the middle part of it, there came out some quarts of a turbid liquor, but so strong and heady, that it made most of them drunk, and fall asleep for divers hours; after which waking, they did for curiosity sake stave the Cask, and found, that about this spirituous liquor the waterish part of the Beer had been hard frozen on every side, and the liquor had been altogether inclosed in thick ice. This relation I had from the old Captain himself, who was employed in that Voyage.

11. The often mention'd Governour of *Smolensco*,<sup>†\*</sup> a famous Fortress between *Russia* and *Poland*, told me, it often happened, that the *French*, and sometimes the *Spanish* Wines, that are yearly brought from *Archangel* to *Moscou*, are so frozen by that time they come thither, that their Owners are fain to break the Cask, and cleave the ice with Hatchets, and then they transport it from place to place in ordinary Jars, (so hard it is frozen,) and when they mean to reduce it to liquor again, they put it into another Cask, and that Cask into a deep hole made into the ice or snow, where it / will slowly thaw, and be far less impaired, than if it had been thaw'd by the fire side, or in a Stove.

***The Phænomena of an Experiment about freezing, made by Mr. Boyle, referable to the VII. Title.***

[*This Paper was produc'd and read in the Royal Society, Nov. 23, 1671.*]<sup>†\*</sup>

We took a Bolt-glass, bigger than two Turkey-Eggs, with a stem, which we caused to be drawn out at a Lamp, till it was as slender as a Goose-quill, or thereabouts. This vessel was filled with water, 'till the liquor reach'd to a pretty height in the slender part of the stem. Then I put it into a mixture of beaten Ice and Salt, in which mixture a cavity had been before made to receive a good part of it. But though upon our putting the Glass into this cavity, there would at the very top seem to be some little shrinking down of the water, yet that was very small, and sometimes very scarcely, if at all, discernable; nor did the water afterwards appear to subside, and exhibit the other *Phænomena* of freezing water mention'd by the Excellent *Florentine Virtuosi*: only when the Liquor began below to be turned into ice, the quick ascent of it was manifest enough.<sup>†\*</sup>

Wherefore we afterward caus'd the stem of a round Bolt-head of clear Glass, whose globous part was about 3½ inches in *Diameter* (taken on the out-side with *Callaper* Compasses) we caus'd, I say, this stem to be drawn out at the flame of a Lamp, till it was at least as slender as a Raven's quill; and the Glass being fill'd with water to a competent height, that the Expansions and Dilatations of it might be very manifest in so very slender a Pipe, we observed the ensuing *Phænomena*.

First, as soon as the globous part of the Glass came to be as it were immers'd in the frigorifick mixture, the water in the small stem instantly ascended, sometimes the length of a Barly-corn, and sometimes less, and sometimes more. And this ascension was so hastily made, that it often begun and ceased almost in the same moment; after which the water began (though more slowly) to subside again to its former station, or thereabout, which with other circumstances made it very probable, That as the *Florentine Virtuosi* ingeniously labour to prove, this sudden change proceeded rather from the

Constriction of the Glass it self upon the first contact of the frigorifick mixture, than upon the sensible Condensation of the water, which is not likely to be so suddenly effected./

Secondly, but whereas the newly named Philosophers recite, as a constant *Phænomenon*, that after the first subsidence of the water, and a subsequent pause for a pretty while, the water will be considerably depress'd once more, before it begins to rise, we could very rarely indeed and scarce ever observe such a thing to happen, though I cannot suspect my self to have overseen it for want of attention. For my expectation of such a subsidence of the water, and its not appearing to me the first and second time, invited me to repeat the Experiment several times one after another, and to look very attentively upon the water, and the marks carefully stuck on the side of the Glass, to observe the motion of the Liquor. And this seemed the rather strange to me, because I had often formerly

observed in Trials purposely made on other occasions, that water in convenient Glasses would suffer some degree of condensation by the action of a frigorifick mixture, before it would begin to discover any ice in it. But having reiterated the Experiment, till I, and those that assisted me, grew weary, I was fain to abandon it, leaving the prosecution of it to farther Trials. For I dare not suspect, that so many eminent *Virtuosi*, as ennoble the *Florentine* Academy, could mistake, or misrelate a matter of fact, not once, but frequently, and uniformly taken notice of by them. And besides that, as I was saying, it is consonant to my own Experiments on other occasions, <sup>†</sup>\* in one of the Glasses wherein I tried this very Experiment, I observed the second subsidence to be considerable. So that I cannot but suspect, that the so differing events of their Trials and mine, as to this *Phænomenon*, may proceed either from some Peculiarity in the water they employed, or in the qualities of the Glass which the vessels I used were made of, or in the length and slenderness of the Stem, consider'd together with the grossness of our *English* Air in snowy weather, the pressure of the Air having elsewhere been shewn by me to have a great stroke in divers Condensations ascrib'd to Cold: but whether to any of these things, or any other, that which we have related is to be reduced, future Trial must determine.

Thirdly, I observed for the most part, that after that subsidence, that almost immediately attends the first rising of the water, there would be for some time, more or less, a resting of the surface of the water in the same place, which continued till the upper part of the water began to ascend upon the beginning of the glaciation of Its lower parts; and the duration of this pause or rest of the water I found to be very uncertain, being at sometimes at least twice or thrice as long as at other times, according as the frigorifick mixture did more or less vigorously operate upon the neighbouring water.

Fourthly, though if the Experiment was tried in Glasses, whose stems were of an unusual bigness, the ascension of the water in the stem upon the / glaciation of it in the globous part, was not so quick as to be very remarkable;

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yet when the stem was drawn out to such a slenderness, as was before described, the water, after having (as I lately noted) rested a while, would upon its beginning to freez beneath, ascend so hastily in the stem, as appeared strange enough, especially at the first sight: So that usually its progress upwards was very obvious, and sometimes made with such celerity, that in one minute of an hour, or much less, it would as it were shoot up several inches, and would have probably ascended much higher within half a minute more, if the slender part of the stem had been long enough to permit it.

Fifthly, but whereas the *Florentine* Academians inform, that there is a considerable Intumescence or rising of the water, that does immediately precede Glaciation, I never could satisfie my self that I observed such a *Phænomenon*. But in spite of frequently repeated Trials (both alone, and before others) and of such a degree of attention as perhaps is not often imployed even in more nice Trials, it always appeared to me, that the ascension of the water was at least accompanied, if not rather preceded, by the actual glaciation of some parts of the water that were most contiguous to the frigorifick mixture, or exposed to those portions of that mixture which were the most operative. Nor did it seem easie to me to assign any other, or at least, better reason of the ascension of the water in the slender stem, than the expansion that is wont to accrue to water, upon its being actually turned to ice. 'Tis true, that in slender stems the rising of the water will be manifest upon the production of so thin and transparent films of ice at the bottom, or some of the lateral parts of the Globe the water is contain'd in, that it has often deceived even attentive eyes, and would have deceived me too, if the newly intimated conjecture at the reason of the intumescence of the water, had not made me extraordinarily suspicious, and invited me to look upon the Glass taken out of the frigorifick mixture (and then wiped and held against the light) in so many differing postures, that though in some of them I could not, yet in others I did discover thin portions of ice, which sometimes I could



within a minute or less make visible to others, because this ice upon thawing would not unfrequently emerge to the confines of the Globe and Stem, and there become easily enough discernable to a heedful eye. And though when I guess'd, that the water was upon the point of beginning to freez, I took it out of the frigorifick mixture, to try if it would afterwards freez, or make the Liquor in the slender Pipe ascend; yet I never was so fortunate as to observe any ascension of the water in the stem, but when there was actually some particles of ice in the ball, which though I newly took out of the mixture, as soon as I could perceive the least beginning of rising in the slender part of the stem, yet I regularly / found more or less ice to have been already actually produced at the bottom or sides of the Globe. The ascension of Bubbles about the time of the waters congelation (especially if the Glass were stirr'd) I do not here solemnly take notice of, it being an usual concomitant of the glaciation of water.

Sixthly, it was remarkable, and not unpleasant in our Experiment, That not only if the Glass were taken out of the mixture, very soon after the water

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began to ascend in the stem, the thaw, by reason of the extraordinary thinness of the ice, would begin so quickly, that within about half a minute, or sometimes much less, the Liquor would begin to subside manifestly again. But when the water was sufficiently dispos'd to congelation, which it usually was, if the glass were put into the frigorifick mixture soon enough, after the total dissolution<sup>†\*</sup> of the little portions of ice newly mention'd, it would upon the contact of the frigorifick mixture, though the globe were but half buried in it, begin to glaciare in a trice: insomuch that making observation by a Minute watch, I have had the water shoot up in the stem within half a Minute, so as to discover ice in it, and within two Minutes (from first to last) to exhibit ice in most parts of the cavity of the Globe.

### *Particulars referable to the VIII. Title.*

1. Two sealed Weather-glasses, one with spirit of *Wine*, and *Cochinele*, the other with a blew Liquor made with spirit of *Mans blood*, *Copper*, and spirit of *Wine*, were immersed in water, and kept there 'till the glass, that contained the water, began to discover some ice within it; then, this water being thrown out, these *Thermometers* were remov'd into oyl of Turpentine, (substituted for the water of the same vessel) and Snow and Salt being applied to the outside, the oyl of Turpentine (whose freezing in such a Quantity and Vessel we were not afraid of) was made as cold as we could. Whereupon the Liquor in both the Weather-glasses manifestly and considerably subsided beneath the former mark, and in one of them (which had the blew Liquor) though it were but a short one, the subsidence was made half an inch; which still confirms, that the Air may impart a higher degree of Cold than is necessary to make water freez, and than is always communicated by Ice it self.

2. The *Essential Oyls*, as *Spagirists* call them, that are made of Spices, and other spirituous materials by Distillation in Water, being, by reason of Spirituousness, unapt to freez; men could not observe, what effect a degree / of Cold capable of coagulating them would produce, in reference to their taking up more or less room, when congealed, than when fluid: some Liquors having been found by experience to be expanded, others to be condens'd, by being made to congeal or to concrete by Cold. Wherefore considering that oyl of *Aniseed*, though an *Essential Oyl*, will lose its fluidity, not only sooner than hot liquors, but with a far less degree of Cold than Water it self; I thought this would be a fit subject to make trial upon, and accordingly having put a convenient quantity of this Liquor into a round Glass, about the bigness of a middle-size Orange, furnished with a suitable stem, we put a mark where the surface of the Liquor rested, as about five inches above the ball: Then putting the glass into a vessel of water, made cold by powder'd *Sal*

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*Armoniack* put to dissolve in it, we observed, that the Oyl in the stem subsiding did grow whitish,

though by inclining the glass it was easie to discern, what part was yet fluid. In a short time after this the oyl seemed totally coagulated into an opacous and very white body, (almost like *sperma Ceti*) whose upper surface was near three inches beneath the mark formerly mention'd: so that the Liquor appeared to have been not inconsiderably condensed by the operation of the Cold. Which further appeared by this, That whereas the oyl of *Aniseeds*, whilst fluid, would swim upon water, this Butter-like Concretion would sink in it. And yet, when I watched, I could observe, that upon the slow thawing of this thickned substance, there emerged from time to time several bubbles into the already fluid parts, divers of which bubbles might plainly be seen in the coagulated matter before their emersion; just before which several of them exhibited various and vivid colours, and very pleasant to behold.

### *Particulars referable to the X. Title*

1. A Flat Bolt-head, sealed up with a stem about 17. inches in length above the superficies of the water, being set in the frigorifick mixture for 8. or 10. hours, the water ascended 15. inches and  $\frac{1}{2}$  that I measured, and afterwards  $\frac{1}{8}$  or  $\frac{1}{4}$  of an inch, according to the measure of another; after which time neglecting it for one hour or more, while I was at Supper, it blew off the sealed end of the Glass quite round, and broke the bottom of it into many pieces, leaving almost all the whole body of the Pipe uncracked.
2. Water freed from Air, by standing a good while in the exhausted / Receiver, being sealed up in a round Bolt-head, whose pipe above the water was five inches or a little more to the sealed *Apex*, being set in a frigorifick mixture, exhibited an Ice very prettily shaped, and without conspicuous bubbles, in less than two hours the water came to be impelled up 4. inches and  $\frac{5}{8}$ , and seemed to reach a little above the *Basis* of the Conical and sealed part; upon its breaking with a noise the pipe was entire, and there appeared a good part of the water unfrozen under the ice, and the broken vessel seemed to smoke. Upon considering the shortness of the Conical part of the Glass, we guessed the Air to have been compressed to about the 20<sup>th</sup>. part of its former room.
3. The globulous part of a Glass-Egg of about three inches (for it wanted  $\frac{1}{10}$ <sup>th</sup>.) in *Diameter* on the out-side, was filled with water to the bottom of the stem, and then being carefully freed and sealed, was frozen from the bottom upwards, to try, whether the absence of the formerly incumbent Air would not make the ice afford larger bubbles, and consequently take up more room, than otherwise it would when the water was frozen in the Ball, and a little way in the lower part of the Stem we found, that (the remaining) water reached from the first station of the water about eight inches and  $\frac{1}{2}$ , the length of the whole Stem being a very little more than ten inches and  $\frac{1}{2}$ . Being afterwards sealed up with Air in it, and frozen, the ice reached not in it

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full four inches above the first station, though (if I mistake not) it was as well frozen this time as the former.

### *Particulars referable to the XI. Title.*

1. There was taken a strong *Cylinder* of Brass, whose cavity was two inches in *Diameter*, into which was put a Bladder of a convenient size, with a quantity of water in it, that the neck of the Bladder (which I had taken care to have oyled) being strongly tyed, the water might not get out into the cavity of the *Cylinder*, nor be capable of expanding it self any other way than upwards. Then into this *Cylinder* was fitted a Plug of Wood turn'd on purpose, which was somewhat less in *Diameter* than the *Cylindrical* cavity, that it might rise and fall easily in it. Upon the upper part of this Plug was laid a conveniently shaped flat Body, upon which were placed divers weights to depress the Plug, and hinder its being lifted up by the expansion wont to be made in water that is

made to freeze: then a frigorifick mixture being afterwards applied to the *Cylinder*, it appeared / within half an hour, or somewhat more, by a Circle that had been purposely traced on that side of the Plug, where it was almost contiguous to the Orifice of the *Cylinder*, that the water in the Bladder began to expand it self; and about two hours after having occasion to shew the Experiment to some inquisitive Persons, the Circle appeared to have been heav'd up, in my estimate about  $\frac{3}{8}$ , if not half an inch, notwithstanding all the weights, that indeavoured to hinder the Ascention, though these weights amounted to 115. Pound, which were all the determinate weights we could then procure, besides Brick, and some other things, that were estimated at five Pounds more. Nor did I doubt, that a far greater Load would not have hinder'd its Expansion.

The day after the above mentioned Experiment was made, to try the Expansive force of freezing water, the same was reiterated after the manner above delivered, but with this difference, That having procured more weights, when the Plug was lifted up  $\frac{2}{8}$  of an inch, or somewhat better, (which Plug began sensibly to rise within half, or three quarters of an hour, after the frigorifick mixture was applied,) it was loaded with a weight of two hundred Pounds, and a fifteen Pound piece of Lead, and other Bodies, as Boards, &c. to lay the weights upon; which being also weighed by themselves, came to fifteen Pounds more, so that the whole amounted to 230. Pounds; and if the hundred Pounds were both of them (as their bulk and weight invited us to guess) of that sort of weights, which are called the *Great*, a Hundred containing a hundred and twelve Pounds apiece, twenty found Pounds must be added to the summe, which would thereby be made up 254. Pounds.

2. An *Iron Barrel*, being about 14. inches long, and having about  $\frac{3}{8}$  of an inch *Diameter* at the bore, and where the greatest thickness of the metal was  $\frac{3}{16}$ , and the least  $\frac{2}{16}$ , or somewhat better, being exactly stopped at the Breech, and having a Screw of a convenient length to stop it at the other end, was filled with water, and then the Screw being put in, the Barrel was buried in a mixture of beaten Ice and Salt for about two hours or longer; at the end of

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which time being taken out, it appeared to have a crack running somewhat oblique, by beginning at a place about three inches distant from the Breech, and reaching to somewhat above six inches from the same: the crack was much more wide and gaping towards the middle of the Barrel, which appeared also distended about that part; the ice being taken out in divers pieces, and held against a Candle, seemed to have smaller bubbles than it would have had, if the water had not been pent up. But the minute bubbles were so numerous, that they made the ice more than ordinarily opacous./

3. A strong *Barrel of a Gun* of twenty four inches long, having the Touch-hole stopt, and a Plug of Iron, that was fitted to the Muzzel, forcibly driven in, after the Barrel had been filled with water, was put into a mixture of Ice and Salt, where within about three Minutes by my Watch, the lately named Plug was with noise driven out of the End it had closed before; and when the same Plug was afterward so driven in, that, to make the closure more perfect, the sides of the Orifice of the Barrel were hammer'd down upon the outward end of the Plug, yet, within about three Minutes more, the frigorifick mixture making the water expand it self, made it again drive out the Plug, and that not onely with noise, but with such violence, that we found it had broken a Deal-board, that made the nearest part of an oblong Box, (wherein the Operation was performed:) afterwards the Iron Plug, being by the help of the fire and a hammer as it were incorporated into the Barrel, the Touch-hole came to be unstopped; and though a long Iron Nail was strongly driven into it, yet the Plug being incapable to be driven out (as before,) the frigorifick mixture being again applied to the Barrel, quickly drove out the Nail; which lastly, being again forced in, and the *Commissure* being for farther security brazed over, there was now no room left for the included water to expand it self much, but by breaking the Barrel; but being my self called away, so that I could not stay to see the issue of the Experiment, I left one to prosecute it, who soon after brought me an account, that within about a quarter of an hour (by his guess) after the barrel was put into the frigorifick mixture, though of that there was scarce left enough to cover it, it burst with a noise, and blew up the cover

of the Box (wherein the Experiment was made,) and the crack, which was two inches and a half long, was wide and gaping enough to let me see, that the Barrel was of a very considerable thickness at the place where it was broken.

A new *Pewter Bottle*, holding (by guess) about a Pint, was fill'd with water, and then the Top being screwed on was put into a frigorifick mixture, wherein, when it had lain (by our estimate) about  $\frac{3}{4}$  of an hour, it was broken, not without noise, and being taken out we found in it a crack almost an inch and a half long, and in one place so broad as to amount to about  $\frac{1}{8}$  of an inch. The Bottle seem'd to be every way distended, and particularly at the bottom, which was so swell'd, that the Bottle would not stand upright upon it./

*One Particular referable to the XIII. Title.*

The old Sea-Captain told me, that out at Sea, when the Wind blew off from the *Great Banks* and Tracts of *Ice*, they could by the extraordinary highness of

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the Cold, (which would sometimes make the skin of their faces peel off) perceive, which way the Ice lay, not only long before they could see it, but sometimes when they were fain to sail twenty leagues before they could come to it.

*Particulars referable to the XV. Title.*

1. Three *Decoctions*, one of *Sage*, another of *Rosemary*, and the third of *Parsley*, were expos'd in three small earthen Pipkins to freez, and were totally turn'd into Ice, without any uncongealed liquor (that I could perceive) in the midst: nor did there appear in the Ice any resemblance of the decocted Plants, but the Ice afforded by the Decoction of *Sage* had a very uneven *superficies*, and far more rugged than the two other portions of Ice, which were neither of them smooth; and these (especially that of the *Sage*) were observed manifestly to be less hard or solid than common Ice.

2. Newly expressed Juice of *Lemmons* being set to freez in a wide-mouth-Glass, afforded an Ice very oddly figur'd, especially in one part, where it finely represented Trees, as they are in Winter, without leaves.

3. Hard *Ice* grosly beaten, having a great proportion of White Table-Salt put to it, and mingled with it, there arose from the mixture great store of whitish Fumes, as thick (at least) as common Smoak, which Fumes played up and down all the mixture, and lasted a very considerable while; and all this, though the weather was very warm, and the Experiment made in a room where there was a very good Fire.

4. *Snow-water* being put to freez in Ice and Salt, afforded an Ice prettily figured, and had the bubbles produced in it so minute, that they hinder'd / not the Globe, which the ice constituted, from being more transparent, than would have been easily expected.

5. We took a quantity (not inconsiderable) of *Ice*, about two pound or more, and having partly reduced it into small lumps, and partly beaten it into small powder, we mixt with it a convenient proportion of Bay-salt not powder'd: This mixture, when it was suffered to lay still for a pretty while, did not appear to emit any thing from the superficial parts, but yet afterwards, when it came to be stirred here or there, there would in that part arise a smoak easie enough to be discerned, if the Bason or Platter, that contained the mixture, were held between the Eye and the Light. But if the whole Body were stirred, then there would be excited so visible a smoak, as that it would not only overspread the surface of the Vessel like a Mist, but would overflow it on all sides for a good while. I use the word, *Overflow*, because indeed the Fumes did not only some of them ascend a pretty way



into the Air like smoak, but the greatest part of them by far, as soon as they were risen above the brims of the Vessel, did fall down in streams, as if it had been a liquor poured out of a Bason; so that the Fumes seemed ponderous, almost like those that one may observe, if he dip a piece of Linnen in *Aqua fortis*, and hang it up to dry; in which case the emitted steams will rather fall than rise. We also took notice,

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that those steams of our Frigorifick mixture were far more plentiful than they seemed to be; for besides those, that were manifestly spilt out of the brims of the vessel, it was easie, by looking upon the mixture in a certain position in respect of the light; it was easie, I say, to perceive, that the whole cavity of the Vessel, which was pretty deep, was all covered with those Fumes, that played upon it like the thick Mist upon a Pond, not being able to pass over the brims of the Bason. And this Ascension of Fumes, upon the stirring to and fro of the Mixture, lasted a considerable while, and probably would have lasted longer, if partly weariness, and partly business, had not called me away.

I forgot to note, that, when those Steams came out the most plentifully, I applied my face to them, to observe, if I could feel them sensibly colder than the neighbouring Air; but by reason of an Impediment I could not continue in a fit posture long enough to be sure, whether those *Effluvia* would in due time feel sensibly cold or no: and though I applied a sealed Weather-glass to the same Fumes, and the tinted spirit seemed thereupon a little to subside, yet some Circumstances make me ἐπέχειν<sup>†\*</sup> till further tryal.

6. The old Sea-Captain, that sailed so often into the Frigid Zone, answered / me, that when his Ship was immured with *Ice*, so that they could not in a long time get so much as a Barrel of Salt-water, he made Wells of the thick pieces of Ice, to receive the liquor of the thawed Ice, and found that water (though on the main Sea) to be good fresh water, potable, and fit for dressing of their meat, and other uses, so that he never feared want of fresh water in those Seas.

He also told me, that he had divers times fastned the Ship to the pieces of *Ice*, that reached under water to about thirty Fathom, and that once he lay a good while by a piece of *Ice* so thick, that it was on ground at fifty Fathom, which he clearly perceived, both by Sounding, and other ways of observing, that he acquainted me with. These deep pieces of Ice (he said) were not very high above the water, insomuch that when I told him I had found by trial, that a *Cylinder* of our *English Ice* could have but about the tenth part of it above the water it was made of, and made to float in, he answered, that that proportion agreed well enough with his observation; and added, that the great depth of the Ice proceeded from the successful Snows, which, falling from the surface of it, depress it, and often within two or three days would it self melt, so as to shrink into a third or fourth part of its former thickness, and become hard Ice. He likewise told me, that he had sailed to 82. degrees and a half of Latitude, and answered me, that he was not mistaken in observing it, having had very good opportunity to do it by more than ordinary fit Instruments, that he had carried along with him.

He told me too, that in some parts near the coast of *Greenland*, he found the Variation of the Compass to be 22. degrees, and not very long after to be scarce any at all; which strange alteration he knew not what to make of.

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He told me moreover, that the last year sailing in the height of 77. degrees in the main Sea, he was suddenly surrounded, and his Ship locked fast up, only that it was driven by and with the Ice, till the 7th of June following; and then, the Ice opening a little, he made a shift to steer through it, and pursue his Voyage, adding, that he observed, that that vast tract of Ice being once broken, the fragments of it drove towards *Hudson's Streights*.

7. The old Sea-Captain told me as strange a thing, which he had often though not carefully observed, That great Tracts of *Ice* dead the Wind, insomuch, that when he has been driven towards the Ice by stormy weather, and feared to be in great danger, when they came near it, he unexpectedly found a kind of Calm, that raised his wonder, and freed him from his fear. And at other times going out of the Ice upon an almost smooth Sea, when he had not yet gone far on it, he found that there was a Storm at distance from the ice: and mentioning this, as a very odd thing, to a *Dutch* / Navigator, who frequented those Seas for the Whale-fishing, he assured him, that he had several times observed this wonderful property of the *Ice*.

*One Particular referable to the XVII. Title.*

In *Siberia* (a Northern Province of *Russia*) the Earth is thawed in Summer but about two Foot in depth, beneath which it continues frozen, and yet over this frozen part of the ground there groweth good Corn. This I had from the *Russian* Emperour's chief Physician.<sup>†\*</sup>

*Particulars referable to the XVIII. Title.*

1. The little sealed Weather-glass being taken, was put into a Glass broader at the top than the bottom, and greased on the inside with Tallow, in which glass the ball in the inside of it was more than covered with water, and that water being frozen, notice was taken whereabouts the tinted spirit of *Wine* rested in the stem: after which the Ice being newly taken off from the Ball in the open air of an exceeding frosty morning, just upon the removal of the Ice the Liquor rose a little in the shank, as it useth to happen when a Glass-bubble filled with warm water is suddenly removed into cold; but presently after the tinted liquor, as I expected, subsided, not only as much as it had risen, but a pretty way (the shortness of the Instrument considered) below the former mark. Which may confirm our observation, that the free Air may communicate a more intense degree of Cold than Ice it self.

2. The Weather having continued for some time very cold, we placed two or three days ago a trusty sealed *Thermoscope* (that was made by the Standard-weather-glass at *Gresham* Colledge, which I therefore call the Standard-*Thermoscope*, having us'd it for some years) in a Cellar, where we had observed Beer not to freez in a very extraordinary sharp Winter; and having looked

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upon it last night, which was, as the night preceding, very frosty, the Wind being at East, we found after ten a Clock that the tinted spirit of *Wine* stood at two divisions, and about 1/8 above the freezing mark, and this morning being a hard frost, it was found to stand much at the same height. Wherefore having caused it to be removed into the free *Air* in the / Garden, it now being about nine of the Clock, is fallen to the freezing mark, and consequently is subsided above two full divisions or inches beneath its station in the warm Cellar. But nevertheless I hence observe, that the *Air* in the Cellar, notwithstanding the cold weather, is not (or is but very little) warmer than the air in my Bed-chamber is wont to be in frosty weather; for the same Weather-glass being usually kept in that chamber, the spirit of *Wine* was wont to stand about two inches above the Cypher or Freezing-mark, in the morning before the fire was made, in case there was a moderate frost abroad; and in Summer-time, when the weather was very hot, the tinted spirit has ascended to the Eighth, Ninth, and sometimes almost to the Tenth mark.

3. The last night being made extraordinary cold by Frost, Snow, and Wind, the Standard Weather-glass (before mentioned) was removed into the Garden, and left there till this morning, when the tinted Spirit appeared to be subsided above two divisions beneath the Cypher or Freezing-mark; so much greater was the Cold of the *Air*, than was absolutely necessary for the congelation of water. And yet the Coldness of this very night did not by '*Αντιπερίσταςις*<sup>†\*</sup> so increase the heat of the

Cellar, but that a vial, containing about two or three ounces of Chymical oyl of Anise-seeds, being left there till Nine of the clock this morning, was taken out without being thawed into a liquor; which argues, that the heat of the Cellar was inferior to that of the outward *Air* in moderate seasons, since oftentimes both in Spring and Autumn, oyl of *Anise-seeds* is by the warmth of the air kept in a fluid form: as this particular parcel of oyl in the same vial, wherein it was exposed, was kept by the moderate warmth of my Chamber many times this Winter.

4. This morning (being *December* 20. 1665.) a little before Ten of the clock, the weather having been frosty (bating one mild, but rainy day) for near a Fortnight, I took my sealed Weather-glass out of my Chamber-window, and having held it a while in the open *Air* in the Court, as also wetted it with water to reduce it the sooner to the Coldness of the Ambient *Air*, I caused one to pump so long, till to a younger eye than mine, the water, that came out of the Pump, seemed to begin to reek; and then I held the ball of the Weather-glass for a pretty while in the stream that came out of the Pump, and observed, that it made the tinted liquor considerably rise, and the more, the longer I kept it, till it was risen to the height of the Cross, which I made in the Frame: then carrying it up to my Chamber, though there was a good fire there, the spirit of *Wine* began to subside / again, thereby showing, that the *Air* in my Chamber was colder than the reeking Water, that had been pumped out.

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5. Another time (being *Feb.* 17. 1665/6) after it had continued frosty weather three or four days, (if I mistake not the number,) about Nine or Ten of the clock in the morning, I caused the water of a considerably deep Well to be pumped for a good while upon it, after it had been kept a pretty while in the *Air*, to bring it to the temper of that, the Pump-water raised it by degrees, but slowly enough, to between four or five Eights of an inch higher than the Pump-water at *Oxford* had been able to do. Then I carried the Weather glass to a Spring that was wont to smook in frosty weather, and was not far from the Pump, and having layed down the Weather-glass, (that my hand might have no operation upon it) so as the Ball was covered with water just at the Spring-head; after it had rested there a good while, I found the tinted Spirit but very little raised, so that in all it scarce exceeded five Eights above the height it had been brought to at *Oxford*.

Afterwards in the same place I brought the Weather-glass about Noon to the North-side of the house, to which the Pump belongs, and letting it rest against the Wall in the open *Air* for half an hour or more, I found, that though it had been that morning a small Frost, and though the Sun did not shine out, yet by the Weather-glass the *Air* was just at the same degree of warmth (if not a little greater) that the water had been at the Spring-head in frosty weather, when there was Snow upon the ground, and consequently the *Air* was then much hotter than the water had been in the Pump at *Oxford*, where yet in very cold weather it uses to smook.

*Feb.* 19. being the third day of the continuance of a moderate Frost, I held the sealed Weather-glass under the Pump, and having caused the water to be pumped for a good while upon the Ball of it, I found the tinted Spirit rise as high with the warmth of the water, as it had done many weeks before in the depth of Winter by the warmth of the water of the same Pump.

The next day, being the fourth day of the Frost, the neighbouring Spring, which (as I was informed by those I sent to see) had not, during the precedent days, smoaked, did smook this morning, as one, I sent to see, informed me, that another Spring likewise did. About Noon (the weather being fair, and the Sun shining) I imployed one to keep the Ball of the Weather-glass for a competent time covered with water just at the head of the Spring, which had smoaked in the morning; and by his Relation, which was confirmed by the height of the tinted liquor, when I saw it, it appeared to have risen higher now by near or full a quarter of an inch, than I could make / it do at the same Spring-head divers weeks ago: but note, that this day the Spring-water was a pretty deal warmer than the *Air*, notwithstanding the time and clearness of the day, as appeared by the subsiding of the tinted Spirit, when brought from the Spring to my Chamber in a frosty morning, the ground being then

covered (but not thickly) with Snow.

6. Having inquired of an Ingenious man, obliged to make some Trials about Cold, whether he had observed any thing to confirm or contradict the Suspicion I published in the *History of Cold* about the Coldness and Temperature of the *Air*; he gave me such an answer, that does notably confirm my

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conjecture. For he told me, that he had divers times observed in an exactly sealed Weather-glass, that the ting'd Spirit of Wine was higher at sometimes, when the weather was frosty, than at some other times, when it was not; and that having had occasion to keep his Weather-glass with the Ball in water, which was afterwards frozen, and continued ice for divers days, he warily brake the ice all about the Ball, and removing it thence into the *Air*, though it were in the same room, yet he found the Liquor to descend from such a mark to such a mark, and having desired to see the Instruments, I found the bigness of the Ball to be like that of a middle sized or somewhat large Crab, and the stem to be about two Foot and  $\frac{1}{2}$  long, and having had the curiosity to measure the distance between the above-mentioned marks, I found, that the Liquor, by being remov'd out of the Ice into the *Air*, had subsided a pretty deal above three inches.

*A Relation given me by an Ingenious Gentleman, lately returned out of Poland.*

7. About the 21. 22. and 23. of *December*, 1669, *old Stile*, lying within three *Polish Miles* of *Warsaw*, we saw every day the Sun accompanied with two *Parhelions*, the one Eastward, the other Westward, almost in a direct line, and distant about 8. or 10 times the *Diameter* of the Sun from it, and continued visible from near Ten to Twelve a clock, the weather being extream cold, the *Air* as clear, as possibly to be imagined, both night and day; and, when the Sun did shine, appeared as full of glittering Spangles or Particles of *Ice*. The like hath been seen since, when it hath frozen very hard; which generally happens, the *Air* being very clear, and as generally thaws, the Heavens being clouded any time together.

8. The old Sea-Captain, that sailed so often to *Greenland* to fish for Whales, assured me yesterday, being *April* 8. 75. that 18. or 19. years ago, / he sailed thither in the company of two *Dutch Ships*, whereof one was a *Hollander*, but the other of *Embden*, the names of the Masters he told me; when they were come together as far as the place where the *English* used to stay in *Greenland* (as I remember,) the Masters of the two Ships desired this Captain to give them leave to fish there with him, which, he told them, he could not possibly do, being a Servant of the *Greenland Company*, and imployed there by them, whereupon these Masters told him, that they would then go seek their Fortunes in an unknown world; and seven or eight weeks after, they came back to him miserably distress'd for want of fresh Water and Fewel, which they desired his leave to take upon the place, which being but an act of Humanity, and intrenched not upon the Rights of the Company, he willingly permitted: Whereupon they fell into discourse of their Voyage; one of the Masters told him, that coasting along the Ice in hopes to find some New Land, and some place where they might freely, as well as conveniently fish for Whales, they had at length come so far, that after the foggy and dark weather was cleared up, they took the Latitude, and found it to be 89. degrees, so that they were within one degree of the Pole.

The old Captain doubting of this, the Master brought him his Journal,

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where the Course was set down, which testified the same thing; and afterwards conferring with the Master of the other Ship, (for they sail'd in company,) he agreed in the same Relation; and the Captain hearing, that the Steers-man of one of the Ships was a *Scotchman*, he got to discourse with him, and saw his Journal too, which agreed with what the others had declared. And afterwards one of the Masters having occasion to come to *London*, and being there met accidentally by our



Captain, he brought him to some of the *Northern* Company, to whom he averr'd the foregoing Relation, of whose Truth the Captain seemed to me to be convinc'd, I asked him several Questions relating to this odd Story, to divers of which he could make no Answer, having not asked them of his *Dutchmen*; but to some few things he gave Answers, the substance whereof was, That though there were vast Regions of *Ice* towards the Shores, yet where they found themselves so near the Pole, the Sea was very open and free, so that if Wood and Water had not begun to fail them, and if they had not fear'd their other Provisions would not hold out, they might have made a Passage perhaps as far as *Japan*. That from the North-East there came a great rolling Sea, which one of the Masters, that had been at the Bay of *Biskay*, compared to that *Spanish* Sea: And that the Cold there was not extream, but such as they could well endure, and complained no more of, than they did in *Greenland*: That sailing from *Greenland* towards the Pole, they found the Compass to / vary a Point, after they had sailed some degrees Northward; Then the Variation of it was for a great while inconsiderable, and a while after that it came to be two Points. And lastly, when they came to be so near the Pole, the Declination increas'd strangely, so that at 89. degrees of Latitude they found the Variation to be four, not Degrees, but Points of the Compass, and that towards the East.

9. The old Sea-Captain told me, that they are in the Seas frequently pester'd with thick Fogs extreamly cold, which last some of them 10. or 12. hours, some a whole day, and others two or three days.

He told me, that lying an Anchor in *Bell-Sound* on the Coast of *Greenland*, near a Mountainous Rock that was very high, he and some others made a shift to get up to the top, which he judg'd to have half a Mile of Perpendicular height, and, when they came to the upper part of the Mountains, they found the Weather very clear, and the Sky very serene, and it being then *June*, the Sun shined so hot upon them that he and others stripp'd themselves, and aired their shirts and naked bodies to cool themselves, seeing all the while a thick Fog like Clouds at the bottom of the Hill; whither when they came down with store of Fowl that they had killed, they found the Fog, as they left it, very dark, and exceeding cold.

#### *A Passage taken out of the Zaars Doctors Letter.*<sup>†\*</sup>

10. Aug. 29. 1664. This Winter we stay'd at *Vologda* three months, which is North-East from *Moscou* some degrees, I expected the intense Cold which is

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usually felt there; but (as it happen'd) we had not three days of that, which we call *Winter-weather* there, notwithstanding we were there in *December*, in which Month it rain'd unusual and dangerous. The Cold, which is so much talk'd of in Books, hath been rare in these late years; for some *English* which have liv'd there thirty years since, have observed such an alteration in the Climate, that, except I had good confidence in the fidelity of their Relations, being men of known worth and sobriety, I should not adventure to tell you, That in these thirty years the Winters are become so mild, as the notable cold Weather, which uses to freez people in their way coming to Market in several postures, as they were striking their Horses, or guiding their Sledges, hath been rarely felt, only to the freezing the Noses and Cheeks of some People, which may rather be termed a *Blast*, then a settled intense Cold.

11. The warmer the Room is made by day, the thicker is the hoar upon / the glass at night, sometimes an inch thick, which I have seen. If it be a small Frost, the Nails only of the Windows, which fasten the Latten together, will be tipt with White; all the Nails of insides of Doors, and Iron-work, will be adorn'd with the Frost, and going out of the door you will endanger your breath.

The Falconers here say, Birds creep under the Snow at Nights.

Assuredly the Bears provide themselves with a Cave against the Winter. I have kept a Bear two days without meat or drink, he still sucking his Paws, making a Lather with his tongue; and, had he not smelt the meat of the house, which made him craving and clamorous, no doubt he might have been kept much longer upon his fast.<sup>†\*</sup>

12. A Wind from the Sea there causes a Thaw; so it does at *Archangel*, although it comes thither North.

I shall hereafter give you a Catalogue of our Plants, some of which are rare in *England*, but here in quantity, viz. *Lilium Convallium*, *Pyrola*, *Bifolium*, *Polygonatum*, &c. I shall a little inform you concerning the Vegetable Lamb, which *Olearius* calls *Baromets*.<sup>†\*</sup>

13. But he told me they use very little Physick, the Air being so healthful, as that tis no rare thing to see people 80. or 100. years old, especially the poorer sort, that do not indulge themselves with strong Drink.

14. The same Captain answered me, that in *Greenland* it self the North-East Winds were colder than any other, which yet he ascribed in part to the Scituation of the Countrey, those Winds blowing over vast Tracts of Ice, without Sea to mitigate the Cold they communicated to the Wind.

### *Particulars referable to the XIX. Title.*

1. Lievtenant G. *Drummond*, who for some years was Governour of *Smolensco*, told me, that he had many times Barrels little less than our Hogsheads of

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strong Beer, which being left night and day in the Sledges upon the Snow, would be frozen all about next the Cask to a considerable thickness, so that the Gimlet must pierce the Ice a great way, before the Vessel would be set a broach; but then the Liquor which came forth, would not only be much stronger, than the Beer was at first, but much more pleasantly tasted./

2. Lieutenant G[eneral] *Drummond* confirm'd what others had told me, of the great noise, like the discharge of Muskets, that they hear in the wooden houses, whose Walls are made of Fir-trees (unsquat'd, and only disbark'd,) upon very intense Frosts; and he answer'd me, the great Cracks or Flaws that appeared in the Timber after these Explosions, were barely Clefs made by the bursting of the Trees, without any splinters, or other parts of the wood thrown off from the body of the Timber.

3. The two *Swedish* Ambassadors assur'd me, that it is true, that in *Muscovy*, and some other Northern Countreys also, the Hares are as in these parts, gray, (*grise*,) but Snow-white in Winter, and that they begin to change Colour in Autumn, (and to recover it in the Spring.) And the Elder of them, Monsieur *Coyet* being asked of me, if he himself had observ'd it, he assur'd me, that he had.<sup>†\*</sup> It was likewise affirm'd by the *Swedish* Resident, who was then present, and related it to me before them, that in the Borders of *Muscovy* he had seen store of *Partridges* milk-white; when I ask'd, whether they chang'd colour in Summer, or were not rather constantly white; He answer'd me, he could not tell, it being Winter when he was there. And when I enquir'd, whether there were not some other besides Hares, that chang'd colour according to the Seasons, they all three told me, that Squirrels, which in the Summer are of the usual colour, do in the Winter turn gray, and recover their colour in the following Summer.

They were pleased also to send me word the next day by an Ingenious Gentleman, Son to Monsieur *Coyet*, that they had forgot to tell me, that whereas the River *Duna* divides *Livonia* and *Muscovy*, on one side of the River the *Hares* are of the ordinary colours, but on the other side white. So that

when the Hunters meet with any white *Hares* on this side of the River, they say tis a *Transfuga*.

4. Amongst the odd effects of Cold in *Russia*, and some other Countreys, where that Quality reigns in Winter, tis none of the least admir'd, that if a man be abroad in the Air with a *Cane* or *Staff* furnished with a metalline Head, the Cold is so intense in that compact Body, that the tip of the tongue being apply'd to the metal, they will stick, and be (as it were) glewed together, so that a man cannot sever them without great pain, and sometimes without leaving some of the skin of his Tongue behind upon the handle of the Staff, as it has been affirm'd to me by Eyewitnesses of unquestionable credit. The Reason of this odd *Phænomenon*, as far as I can conjecture, may be

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the great and sudden loss of Agitation occasion'd in the Spittle and part / of the Tongue, by the great want of Agitation in the parts of the Metal that they touch. For if a *Bowl* (for Instance) of Ivory meet in a direct line with another of the same bigness, that is already moving upon a Billiard-Table, 'twill communicate to it but a part of its own motion, and so retain the rest for it self: but if the impell'd *Bowl* were at rest, the impelling *Bowl* will communicate to it all, or almost all its motion, and lose as much it self, as may appear by its remaining quiescent in the place of the other. I must not enlarge upon this subject of Motion among Bodies, that hit against others. But to apply the Observation to my present purpose, it seems the Metalline Handle of the *Stick*, by the intensness of the Cold, has its parts so depriv'd of motion, that when those of Spittle and the Tongue have communicated to them as much of their Agitation as they can, there will not be between those three bodies Agitation enough to keep the Spittle fluid, which consequently being turn'd into Ice will stick to the two consistent Bodies it adher'd to, namely the Tongue and the Metal, and by this means will fasten them together.

In confirmation of this conjecture I shall adde some other *Phænomena*, which may be explicated by the help of it. And first we see, that in very frosty Mornings the *Ice*, that sticks to Glass-windows, often appears in the form of Trees, or otherwise odly and prettily figur'd. This is vulgarly so explain'd, as if the Cold produc'd those icy Bodies on the outside of the Glass, through which, some fancy, that the Vapours of the warm Room penetrate: But tis plain, that this *Ice* uses to be formed within the Room, as I have divers times observ'd, either by the thawing of it, or by scraping it off; So that it appears to be form'd of Vapours, which being carried to and fro by the Air, when they chance to pass along the Glass-panes of the Windows, which by the *Cold*, that reigns in the external Air in frosty Weather, have lost the wonted Agitation of their parts, these Vapours transfer so much of their motion to the Glass, that they retain not enough to keep them fluid; the Consequence of which is their being turn'd into Ice, which in very cold Countreys may be far thicker than it uses to be here: Insomuch that a Learned Acquaintance of mine assur'd me, that he had in *Russia* observ'd in some Stoves, (where tis like the Heat produc'd store of Vapours,) that the Ice on the inside of the Windows was near an Inch thick.

#### *A Note out of Martinius in his Account of China*<sup>†\*</sup>

5. In Peking elevatione Poli 42. gr. per integros quatuor menses, facto circa Novembris medium initio, flumina omnia adeó duro concrescunt gelu, ut currus, equosque, ac gravissima quaeque onera glacies tutò ferat. Hac plerumque concretio / uno fit die; cùm nox nisi pluribus, & quidem ab inferiori superficie priùs, fuit liquefactio. p. 27. 'Martinius in his account of Peking p. 27, tells us, that although the Pole be not elevated above 42. degrees, yet for four whole moneths, from the middle of *November*, all the Rivers are so bitterly frozen over, that the Ice will safely bear Coaches, and Horses, and all the heaviest burdens. This Congelation is for

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the most part made in one day, but the melting of the Ice requires many, and begins from the lower



Surface.'

*A Note taken out of Martinius Cromerus his Polonia, lib. 1. I. p, 53, 54.*<sup>†\*</sup>

6. *Tanta est enim vis frigoris in his Regionibus interdum, ut radicitus arescant arbores, & aqua ex editiori loco effusa, priusquam terram contigerit, in glaciem concreseat. Lacus quidem, & Paludes, & Flumina duobus tribusve mensibus hibernis, nonnunquam autem vel in Quintum vel Sextum usque concreta glacie, non modo peditibus, verum etiam equitibus, & curribus ac plaustis, quamvis oneratis, multis simul longo spatio pervia & secunda præbent itinera. Equidem quadam tempore ultimo die mensis Martii Vistulam in Masovia per firmam adhuc glaciem, cum curru & quadrigis, & aliquat Equitum comitatu transivi, Hæc etiam præteritâ Hyeme in Prussia glacialis piscatio in lacubus post initium Novembris cæpta, duravit per totum Martium, gelu autem per totum Aprilem.* 'So great is the violence of Cold sometimes in these Countreys, that Trees wither at the roots; and Water poured out from an high place, turns into ice before it comes to the ground. And truly the Lakes, and Marshes, and Rivers are so frozen for two or three moneths in Winter, and sometimes for five or six, that not only Footmen, but also Horsemen, and Coaches, and Waggon, though loaded, may for a long space freely and securely pass over them. Truly once I passed over the *Weissel* in *Masovia* upon the firm ice the last day of *March*, with a Coach and four horses, and a retinue of some horsemen; and this last Winter in *Prussia* the fishing in the Ice began in the Lakes after the beginning of *November*, and continued all *March*, but Frost lasted all *April*.'

*A Note taken out of Cromerus's Polonia, lib. 1. p. 68.*

*In cæteris lacubus, atque etiam in majoribus piscinis & fluminibus tempore hyberno commodiores ferè sunt piscationes, quàm æstate, pertusâ certis intervallis glacie, retique per majus foramen in aquam immisso: quod longis funibus ad perticas alligatis hominum equorumve operâ longo spatio in diversum tractum, coeuntibus rursus piscatoribus, alio foramine refertum extrahitur.*

'In other Lakes, and in the larger Fish-ponds, and Rivers, Fishing is more commodious in Winter than in Summer, the Ice being broken in some places, and a Net cast into the water through a great hole, which by long Ropes tyed to Poles, Men and Horses draw different ways for a considerable space, and then the Fishermen meeting together, take it up well fill'd at another hole.'

7. An Ingenious Physician confirm'd to me upon trial, that the *Pommel* of a *Sword*, that was expos'd to the Winter Air in *Moscou*, would stick to his Tongue that touches it therewith, and fetch off the skin, if he forcibly and suddenly pull'd it away.<sup>†\*</sup>

8. A Gentleman that came lately from *Warsaw*, told me, that in one night, or rather twelve hours, he observed this Winter the *Ice* next the surface of the water, to reach four inches directly downwards.

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9. The old Sea-Captain told me, that when he was in *Greenland*, and in those *Artick* Regions, his Appetite was so great, that he could well eat more in one day, than he could in a week or ten days here, and that accordingly he and others found themselves stronger there than here, and more prone to Venereal pleasures.

He told me also, that sailing with intention to make some discovery into the *Artick* Circle, that after having sailed a great while through a Sea exceeding blew and deep, they were as much surprized to find themselves on a suddain upon a Sea black almost like Ink, which much frightened them, none of all their Wanderers having met with a Sea of that colour. This made them, after many disputes and doubts, resolve to sound both from their Ship and Shallop, but they could find no ground at Seventy Fathom.



10. A Bottle of strong *Sack*, about two thirds fill'd, crack'd, and the Wine was frozen, viz. the phlegm, but no Ice on the top, as in water; nor does the phlegm ever ascend to the top to be frozen in an entire body.

This Sack being presently thawed lost its vigour, and so will any thing else.<sup>†\*</sup>

11. The Frost in these parts pierces the ground five foot, which the Undertakers for digging a Cellar for a Friend of mine found by woful experience, being forc'd to make their way by Fire, and sometimes by cleaving the Earth with wedges like a rock.

The Ice in *Siberia* in the River *Ob*, is said to be a Fathom and a half thick, where they have in the whole year but twelve weeks of Degelation./

The Rivers that I have travell'd over, have breathing-places for a mile, sometimes half a mile, and a quarter, out of which comes a fume like that of the *Cross-Bath* or *King's Bath* at the *Bath*. The like out of a Cellar upon the opening of the door, enough to suffocate a man, if taken unawares.

Death by Cold is not painful, especially if the Cold be very intense, as a friend of mine told me, who waiting upon his Uncle here (a Colonel) in quality of a Page, he sate upon the side of the Sledge, as Servants do, and it being very cold, he ran to get himself some heat in his feet, and afterwards return'd to sit upon the Sledg, and found himself surpriz'd with the Cold, but had not the power to prevent the danger of Suffocation by calling for help; yea rather he seem'd to be pleas'd with falling into a pleasing sleep, and so tumbled back upon his Uncles legs, which rais'd a suspicion in him, that his Cousen was frozen; and so making haste to the next Village, he rubb'd him all over with Snow strenuously enough, and afterwards brought him into a warm Room, which by degrees reviv'd him, he feeling himself afflicted and pain'd in all his limbs with such a tingling Numness, as they use to have who lean'd too hard upon their Elbows, but much more painful and amazing.

12. That *Cold* dries excessively, appears by the story of the Cheeses, and Stock-fish, which is dried in the Wind, some of which is sprinkled with Salt.

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The Earth will cleave with it, but I never observ'd it crack'd with such monstrous *Hiatus*, as *Olearius* reports,<sup>†\*</sup> yet that may be true also. Where there is Frost long before the Snow comes, the Ice in Rivers will have cracks.

About the middle of *December* being at *Yewslave* in my Lodging in a morning before day, the House being new, and expos'd to the North-east wind, it gave a crack like a Musket.

### ***Particulars referable to the XX. Title.***

1. A Seal'd Glass-bubble with Quick-silver in it weighed in the Air, and being carefully counterpois'd in Water, that to my hand, whilst I was sitting by the fire-side, felt luke-warm, did, after Snow and Salt were apply'd on the out-side of the Glass that held the water, weigh  $\frac{3}{4}$  of a Grain, or somewhat better, before or just when there appear'd a little film of Ice on the inside of the Glass containing the water, less than it did when the Bubble was first put in./

2. A globe of *Snow*, ramm'd into the Mould of just one inch *Diameter*, weighed 112. grains.

3. A Bullet of *Ice* of an inch *Diameter*, made in the same mould, amounted to 2. Drachms, 5. Grains.

4. After a long Frost and Snow, a great deal of new *Snow* being fallen last night, the Liquor in the gaged Weather-glass stands beneath the first mark, argues a more than ordinary Frost: and yet the *Mercury* in the *Baroscope* stands a near  $\frac{2}{3}$  beneath 20. inches, to which perhaps the high wind may contribute.

5. Four ounces of *Snow* made up in a lump, were counterpois'd in a pair of good Scales, and expos'd in a frosty night after eleven a clock without being taken out of the Ballance, the next morning between 0. and 10. there appear'd a decrement of 20. or 30. grains, which seem'd to have evaporated from the *Snow* it self: for though a small portion of it (probably late in the morning) were melted in the Scale, yet that Liquor was but little, and amounted not to 8. grains, which was not a Third of the weight, which the Snow-ball had lost; but supposing the decrement would be greater, if the Snow had a greater *superficies*, in respect of the bulk thereof, I caus'd the next night the following Trial to be made, not being able to assist at it my self.

There was taken ʒij [2 drachms] of *Snow*, which being made up into a kind of flat Cake, was expos'd all night, which was frosty, in the above-mentioned Balance, and the next morning about 8. a clock there appear'd to have been lost 55. grains, no water being found in the Scale; and about two hours after, the Decrement was found to be 63, grains, none of the *Snow* appearing to have melted in the Scale.

### *One Particular referable to the XXI. Title.*

The *Samojedes* cloath themselves with *Renes* skins, the hair outward, which they find to be the warmest way: and I have found a pair of *Cangies* (which

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are like high Shoes with piked toes of old *Chaucer's* fashion, which we wear without the leather) to be better than a pair of furr'd Gloves. They are only proper for the Sledge, yet one may walk in the Snow with them, which is so dry a Snow, if it be duly cold weather, as it will not wet at all, nor endamage a Scarlet cover'd all over with it, but brush or shake off like chaff.

*FINIS.*†\* /

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## **Glossary**

On the rationale of this section, including information on sources used and comment on what words are included and why, see above, vol. 1, pp. cii-ciii

acayu: i.e. acajou, the cashew tree and its nut, used in a balsam as a blistering agent

acetum minerale: 'mineral acid', i.e. one of the three acids so considered - sulphuric, nitric, and hydrochloric (muriatic); also generically, any sour or acidic liquid prepared from a mineral body

acetum radicum: 'radical vinegar', concentrated acetic acid; or more broadly in some contexts, a concentrated or highly corrosive liquor

acidulae: springs of cold mineral water, so named due to their sharp and sour taste, which was considered acid

acroamatic: privately communicated by oral teaching to chosen disciples; esoteric, secret

adnata: the mucous membrane which lines the inner surface of the eyelid

ad siccitatem: to the point of dryness

adust oil: an oil which burns, or which is prepared by burning or charring substances

advertisement: notification, instruction

aeolipile: a pneumatic instrument illustrating the force with which vapour generated by heat in a sphere rushes out of a narrow aperture, akin to the 'engine' of Hero of Alexandria (c. 60 AD)

aequitemporaneous: performed in equal lengths of time

aestiomenes: i.e. esthiomenos, pertaining to a disease which rapidly eats away the flesh

affusion: a pouring on or into

alexipharmic, alexiterial: having the properties of an antidote

alga marina: 'sea algae', i.e. sea-weed

allum: see roch-allom

aloes hepatica: a species of the aloe plant whose bitter juice was used as a purgative; considered inferior to aloes succotrina (q.v.) for this purpose

aloes succotrina: a species of the aloe plant whose bitter juice was used as a strong purgative

althea: the marsh-mallow, used in many medicinal preparations

amausen: counterfeit gems

amel: enamel

ana: 'equal amounts of each', used in medicinal recipes

anagallis: the pimpernel, which was considered to be a cure for wounds

andratomy: dissection of a human body

animadversive: percipient

antanaclasis: repetition with a different meaning

antimonium diaphoreticum: a sweat-inducing and laxative mixture of antimony oxide and potassium antimonate prepared by deflagrating antimony trisulphide (the native ore) with saltpetre in a red-hot crucible

antimony, butter of: white antimony trichloride, made by dissolving antimony trisulphide (the native ore) in hydrochloric acid and distilling it, or by distilling a mixture of the antimony ore with corrosive sublimate (mercuric chloride)

antimony, crocus of: impure antimony oxysulphide, a bright yellow powder produced by precipitating solutions of antimony from alkaline lixivias; also known as crocus metallorum (q.v.)

antimony, flowers of: any sublimate prepared from antimony trisulphide (the native ore), these flowers are generally white when the sublimation is carried out in the air (antimony oxides), but sometimes red or black (antimony sulphides) when the sublimation is done in closed vessels

antimony, glass of: a vitreous material composed mostly of antimony oxide and sulphide, obtained by roasting antimony trisulphide (the native ore) and fusing the resultant 'ash'; used as an emetic; also known as vitrum antimonii (L. Principe, "'Chemical translation" and the role of impurities in alchemy', *Ambix*, 34 (1987), 21-30)

antimony, regulus of: the pure metallic antimony, separated from the sulphur with which it is joined in the ore

antimony, sulphur of: the putative combustible component of antimony; Basil Valentine claimed to extract this substance from glass of antimony by acetic acid (Principe, 'Chemical translation')

antiperistasis: the antagonism of powers which are naturally opposed to each other, i.e. heat and cold. For the specific Aristotelian meaning, see vol. 4, pp. 459ff.

apophyses: a protuberance or process of a bone

aporrhea: a morbid exhalation, emanation or effluvium

apozem: a decoction or infusion

appensa: amulets hung around the neck as cures for various afflictions

aqua aronis: 'water of arum', a beverage made from the wake-robin or cuckoo-pint plant, both species of the genus *Arum*, which was thought to be a cure for diarrhoea

aqua cœlestis: 'water of heaven', a solution of ammoniated liquor in water; used medicinally

aqua fortis: 'strong water', a corrosive acid, usually nitric acid



aqua malvae: 'water of mallow', the distilled water of the mallow plant, used medicinally, particularly for complaints of the chest

aqua Mariae: 'water of Mary', a bain-marie (balneum Mariae) (q.v.)

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aqua mirabilis: 'the wondrous water', a cordial distilled from a mixture of various spices and aromatics with alcohol

aqua ophthalmica: 'eye water', an eye wash to relieve films on the cornea

aqua pugilum: 'water of the fighters', a liquid which Basil Valentine described in the second key of his *Zwolf Schlüssel* (Twelve Keys), which is used to volatilise gold and extract its 'tincture'; Boyle's menstruum peracutum (q.v.) is based on this (L. Principe, 'The Gold Process', in *Alchemy Revisited*, ed. Z. van Martels (Leiden, 1990), pp. 200-5, on p. 204)

aqua regia, aqua regalis: 'royal water', a mixture of nitric and hydrochloric (muriatic) acids; one of the few solvents which can dissolve gold

aqua sympathyti: 'water of osteocolla', a medicinal ointment made from osteocolla (q.v.)

aqua vitis: 'water of grape', a medicinal liquid made from the fruit and other parts of the grape-vine, used in haemorrhages

aquatilia: aquatic creatures

arch-angel: the angelica plant

archeus: the vital, immaterial, principle which Paracelsus and his followers claimed ruled over all animal and vegetable life and natural processes

arsmart water: the distilled water of the plant arsesmart, or water-pepper, which was thought to dissolve the stone

asarum: the asarabacca plant, also called nardus, whose leaves and roots are very bitter and were used as an emetic and purgative

aspera arteria: the bronchial tubes

asperated: roughened, made harsh

assa dulcis: benzoin

athanor: a digesting furnace in which a constant heat was maintained by means of a tower which provided a self-feeding supply of charcoal

aurific: producing or making gold

aurum fulminans: 'fulminating gold' (q.v.)

aurum potabile: 'potable gold', a widely-sought medicament made from gold

avolate, avolation: to fly out, evaporate

balaustium: flower of the wild pomegranate

balneum arenae: 'bath of sand', a method of heating a substance by placing its container in sand and heating the sand from below.

balneum Mariae: 'bath of Mary', i.e. bain-marie or hot-water bath, a vessel of water in which another vessel is heated; named after an alleged Jewish alchemist of the first century. See also aqua Mariae

balsam of sulphur: a balsam made from sulphur and olive oil, used largely to relieve catarrh and other chest infections

balsamum fuliginis: 'balsam of soot', an ointment used in skin diseases

balsamus samech: 'balsam of salt of tartar' (samech being Paracelsus's term for salt of tartar), used for gout and the stone

balsamum sulphuris terebinthinati: 'balsam of therebinthated sulphur', a

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balsam for chest afflictions made from sulphur and turpentine digested together.

balsamum vitae: 'balsam of life', a compound consisting of myrrh, aloes, olibanum, balsam, bdellium and a variety of aromatics and essential oils; it was used largely as a general panacea

Barbados tar: a greenish petroleum

bastard: a sweet Spanish wine

beccabunga: the brook lime plant, *Veronica beccabunga*

benjamin: i.e. gum benjamin, benzoin

bezoar minerale: an oxide of antimony used medicinally, generally made by digesting butter of antimony (q.v.) with aqua fortis (q.v.)

biddekel: i.e. binnacle, a box on the deck of a ship near the helm in which the compass is placed

blas: the general 'astral-cosmic' force which van Helmont made responsible for all motion and change in the universe (Walter Pagel, *Joan Baptista van Helmont, Reformer of Science and Medicine* (Cambridge, 1982), pp. 87-95)

blood of salamander: a name sometimes referring to the red vapours produced by spirit of nitre (nitric acid) during the latter part of its distillation

bloodstone: i.e. haematite, a native iron oxide, used against haemorrhages and fluxes

bole: any of several varieties of friable earthy clay

bole-armoniack, bolus armeniae, bolus armeneus: a red, iron-impregnated, astringent clay from Armenia, used medicinally as a styptic

Bologna stone, Bolonian stone, Bononian stone: a dense white stone, the naturally occurring form of barium sulphide. In 1603 Vincenzo Cascariolo discovered that a sample of this stone collected near Bologna, after some chemical preparation, remained phosphorescent after being exposed to the sun for a period of time

borillia: i.e. barilla, an impure alkali made from the ashes of calcined Mediterranean plants

borracha: i.e. borachio, a goatskin bag used for wine or other liquors.

bowdie: i.e. 'Bow dye', a scarlet dye developed at Stratford-le-Bow, East London

branle: to agitate, toss about

Bristol stones/diamonds: transparent rock-crystal found in the limestone deposits near Bristol, resembling diamonds in their brilliancy

Burgundy-pitch: the hot and dry resin from the spruce-fir, used as an adhesive in plasters for wounds, bruises and catarrhal afflictions

butter of antimony, butyrum antimonii: see antimony, butter of

butter of tin: see tin, butter of

cadmia, cadmian earth: calamine (zinc oxide and carbonate)

calibash: a gourd or similar large fruit that can be used as a container for liquid

calx viva: quick-lime (calcium oxide)

campanam, per: see per campanam

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candent: heated to glowing heat

caput mortuum: 'dead head', the residue remaining at the bottom of the retort after distillation

caranna: a soft and tenacious rosin derived from a West Indian tree and used in balsams and plasters

carduus benedictus: 'the blessed thistle', a bitter and astringent Mediterranean plant used medicinally as a tonic and universal antidote

carious: decayed, rotten

caryo-castinum: a medicinal concoction made of cloves

centrobarricks: of or relating to the centre of gravity, or the process of finding it

chios: the most common type of turpentine, originating from the isle of Chios

chrysopœians: those who make gold from base metals (see L. Principe, *The Aspiring Adept* (Princeton, 1998), p. 9)

chrysulca: aqua fortis (q.v.)

cinnabaris fossilis: 'mineral cinnabar', the native mineral cinnabar, mercuric sulphide

close-stool: a chamber-pot enclosed in a stool with a cover

clyssus: in Paracelsian chemistry, the reunion of chemical principles through long digestion; also used to describe the product of the detonation of nitre with any other substance

coaptation: adjustment of things to one another

cohobation: the repeated distillation of a material, done by pouring the distillate back upon the residue

colcothar: fixed residue from the destructive distillation of vitriol, generally the blue variety; usually copper oxide

colliquation: the action or process of melting together

compenetrate: to penetrate in every part

concinnity: internal harmony or congruity; mutual adaptation of parts

confectio hyacinthi: 'confection of hyacinth', an electuary made with the hyacinth plant and saffron

contemporate: moderate

contrude, contrusion: to thrust together

corrosive precipitate: see mercury precipitate

cortex Jesuiticus, cortex Peruvianus: see Jesuit's bark

crabs' eyes: concretions of carbonate phosphate of lime found in the walls of the stomach of the river crayfish, which, when powdered, were used medicinally as an absorbent

crama: a mixture

crasis: in medicine, the due distribution of the bodily humours in a healthy person; in chemistry, a mixture or the totality of the virtues of a given substance

crassum; thick, gross, fleshy

cremor tartari: cream of tartar

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crocus: any of various yellow or red powders, often obtained by calcining metals (especially iron) either alone or with sulphur

crocus martis: iron oxide, a dark red powder obtained by calcining iron sulphate or iron and sulphur

crocus metallorum: 'crocus of metals', an antimony sulphide, a bright yellow powder obtained by calcining or precipitating antimony; also known as crocus of antimony (q.v., under antimony, crocus of)

cucupia: the click beetle (cocuyo), some of whose tropical varieties are luminescent.

cucurbit: a vessel or retort, originally gourd-shaped, forming the lower part of an alembic.

dale: wooden tube or trough for carrying off water

Danzig vitriol: copper sulphate (blue vitriol)

daucus: the garden carrot, whose seeds (semen dauci) were used medicinally



deflagrable: capable of rapid combustion

deliquium, per: see per deliquium

deopilative: something that removes obstructions

dey: archaic form of 'die' or 'dye'

diachylon: a soft plaster or cement, composed of juices of herbs, metal oxides, and gums

diachylon cum gummis: 'emplaster of mucilages', or yellow diachylon, a soft plaster made by boiling together lead oxide (ceruse), olive oil and linseed, to which boiled gums such as galbanum are added.

diacorallion: 'confect of coral', a sweet medicinal powder made from white and red coral, bole, dragon's-blood, pearl, gum tragacanth, etc.

diadrom: the vibration of a pendulum

diagridium: a preparation derived from the resin of the root and bark of the scammony tree

diapalma: a drying plaster, composed of palm oil, lead oxide and zinc sulphate

diascordium: 'confect of scordium, or water-germander', a cordial medicine made with scordium, cinnamon, bistort, galbanum, gum arabic and several other ingredients

dionise: black precious stone with streaks of red, thought to be a preservative against drunkenness

discuss: to dispel, disperse

dissiltion: a leaping or springing apart; a bursting

docimastical: pertaining to the art or practice of assaying metallic ores

dorsel: a compress or poultice placed on the back

dragon's blood: the blood red resin which exudes from various East Indian trees (especially *Pterocarpus draco*), used medicinally as an astringent and as a colouring for varnishes; also, rarely, an older term for cinnabar

duelech: a Helmontian term for a type of concretion which forms from urine and causes the stone

eagle stone: aetite, a hollow nodule of clay-ironstone containing within it

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another (variable) substance; thought to have extensive medical benefits, such as facilitating childbirth and preventing abortions

effatum, effata: dictum, dicta

efformation: shaping

effumability: capable of being converted into vapour

elater: elasticity; the expansive property inherent in air or gases

elaterist: one who explains phenomena in terms of the expansive force of the air

elaterium: a laxative medicine prepared from the juice of the wild cucumber

elixir salutis: 'elixir of health', i.e. tincture of senna, a laxative preparation made with senna leaves, caraway, cardamom seeds and raisins

embryonated: found with, or embedded in, or, in terms of mineral bodies, in an embryonic or rudimentary form

emplastrum ad herniam: 'plaster for ruptures', a plaster made from galls and other vegetable matter

emplastrum de mucilaginibus: 'plaster of mucilages', i.e. diachylon cum gummis (q.v.).

emplastrum de sapone: 'plaster of soap', a plaster made from hard soap, lead oxide and olive oil

emplastrum divinum: 'the divine plaster', a cleansing plaster made from ammoniacum, bdellium, galbanum, myrrh, and other minerals and ointments

empyreumatical: having qualities as if burnt by fire, as in smell, taste, etc.

enditor: inditer, one who composes or dictates a literary work, speech or letter

English vitriol: iron sulphate (green vitriol)

engyscope: microscope

ens veneris: 'essence of Venus', a copper compound with medicinal properties mentioned by van Helmont and prepared by George Starkey and Boyle (the name alludes to the traditional association of copper with the planet Venus)

epanados: recapitulation

epiphyses: a part or process of a bone which ossifies individually, separated by a cartilage, and subsequently becomes joined to the main part of the bone

esurine: substances (especially salts) which are of fretting or corrosive quality; medicines provoking appetite.

euphorbium: the resinous gum of certain North African plants of the genus *Euphorbia*, used in many plasters and as an emetic and purgative

evanid: vanishing away

exantlate: to draw out as from a well, to exhaust; in Helmontian terms, to weaken or debilitate the corrosive power of a solvent

exsection: a cutting out or away

fixed nitre: a salt prepared from nitre which is stable (unlike nitre) in the fire; often potassium carbonate or sulphate

flowers, flores: sublimate

flowers of antimony: see antimony, flowers of

flowers of sulphur, flores sulphuris: sublimed sulphur, a light yellow powder

flower-de-luce: i.e., fleur-de-lys, the ornamental needle on a compass shaped like a lily

fluor albus: 'white flux' or 'white fluor', see white fluors

fluores: generic name for class of minerals resembling gems (e.g. fluorites) but readily fusible

fondant: flux, a substance that is mixed with a metal to facilitate its fusion

fonticulus: 'a little fountain', an artificial ulcer formed by incision or caustic, and kept open so that it may continue to discharge morbid substances

fritta: i.e. frit, a partly fused mixture of sand and fluxes ready to be melted in a crucible to form glass; also shreds or fragments

fucus: paint or cosmetic for beautifying the skin

fuliginous reeks: (1) vapours relating to soot; (2) odours emitted by bodily exhalations

fulminating gold: an easily explosive powder made by precipitating gold from its solutions with an ammonia compound

funiculus: hypothetical string of rarified matter invoked as agent in the Torricellian experiment (see vol. 1, p. 157; vol. 3, p. 10)

furor uterinus: 'rage of the womb', hysteria

gad: a pointed iron or steel bar for loosening ore, etc.

genus nervosum: a general term used to denote the nervous system

glass of antimony: see antimony, glass of

glass of lead: see lead, glass of

gleetings: a transparent mucous discharge, usually from the urethra

glyster: i.e. clyster, an enema or other sort of medicine injected into the rectum

gold, litharge of: see litharge of gold

granado: i.e. granada, the Spanish word for pomegranate

groove: a mine shaft or trench

gum arabic: gum exuded by some forms of the acacia plant, used extensively in medicinal preparations

gum dragon: the viscous substance obtained from the plant tragacanth, native to the eastern Mediterranean; it does not easily dissolve, and is therefore used in plasters, ointments, etc.

gum guttae: the gum resin derived from the stalagmitis plant

gum lacca: the dark-red resinous incrustation produced on certain trees in the East Indies by the

puncture of an insect, *Coccus lacca*, used as a scarlet dye; also known as seed-lac (q.v.).

haemantic letters: those Hebrew letters which are used in the formation of derivative words and inflexional forms

Hassian retorts: stoneware retorts capable of withstanding high temperatures for long periods

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helmet: top of a glass alembic; distillation head

hemicrania: headache confined to one side of the head

Hippocras-bag: a conical bag of cotton, linen, or flannel, used as a filter

hordeum: barley

horn silver: fused silver chloride, so called because it resembles horn

hydrarchic: pertaining to the watery realm or domain

hydrogogue: having the property of removing water or serum

hydromel: water impregnated with honey, which, when fermented, becomes mead

hydromet: (1) hydrometra, dropsy of the womb, a rare disease; (2) a hydrometer, an instrument for measuring the rapidity of the flow of water; (3) a hygrometer, an instrument for measuring humidity

hylarchic: literally 'ruling over matter', particularly as used by the Cambridge Platonists of their putative spirit intermediate between God and matter

iatrochemistry: medical chemistry, which uses mineral substances and spagyric (q.v.) techniques to develop medical remedies

idoneous: apt, suitable

illapse: the act of gliding, falling, or slipping in

incalcescent: becoming hot or warm

incogitantly: without consideration, thoughtlessly

incolumity: safety, freedom from danger.

indagation: investigation

infernal stone: lunar caustic, fused silver nitrate, a styptic substance

insessus: a hot-bath, with medicated liquid, over which a patient sits

intestinum rectum: i.e. the rectum

irroration: a light sprinkling or wetting, as by dew

Japonian earth, Japan earth: catechu, so called because this vegetable matter from East Asia was originally deemed a type of clay



Jesuit's bark: cinchona bark, so named due to its introduction into Europe in the early seventeenth century by the Jesuit, Cardinal de Lugo; a renowned cure for fever, also known as Peruvian bark

kitling: i.e. a kitten

lac sulphuris: 'milk of sulphur', a white aqueous suspension of sulphur, prepared by precipitating the sulphur out of an alkali solution

lapides cancrorum: 'crabs' stones', see crabs' eyes

lapis aquilae: see eagle stone

lapis Armeneus: 'Armenian stone', a blue carbonate of copper esteemed as a cordial

lapis calaminaris, lapis calamaris: 'calamine stone', zinc carbonate and oxide (calamine), an ore of zinc

lapis cornea: 'horny stone' or luna cornea, see horn silver

lapis haematites: 'haematite stone', see bloodstone

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lapis infernalis: 'infernal stone' (q.v.)

lapis lyncurius: 'lynx stone', i.e. amber, which was once reputed to be the petrified urine of the lynx; it was used medicinally to cleanse wounds

lapis Malacensis: 'stone of Malacca', the bezoar stone found in the gall of the Indian porcupine; it has an intensely bitter taste, and was thought to be a universal antidote

lapis manati: 'stone of the manatee', a bezoar stone extracted from the manatee; also known as oculus manati

lapis nephriticum: 'kidney stone', a compact variety of amphibole, the less valuable variety of jade, which was worn as a remedy for kidney diseases.

lapis ossifragus: 'osprey stone', another name for osteocolla (q.v.)

lapis porcinus: 'porcupine stone', another name for lapis Malacensis (q.v.); also known as oculus porcinus

lapis specularis: 'specular stone', a clear and bright stone which, when cut into thin panes, was used for glass, and when powdered, as a cosmetic.

lapis tutiae: 'stone of tutty', i.e. tutty, zinc oxide; also known as pompholix (q.v.)

lation: motion

latten: i.e. latten, iron tinned over, or tin-plated; any metal made into thin plates

lead, glass of: glass made with a large amount of lead oxide

lead, mercury of: the putative vaporous or liquid component of lead

lead, sugar of: lead acetate, made by dissolving lead oxide in vinegar

leucophlegmatitia: dropsical tendency, denoted by phlegmatic condition of the body

lignum aloes: 'aloe wood', the aromatic wood of the aloe tree; used as a cordial and in perfumes

lignum nephriticum: 'nephritic wood', so called because an infusion of it helped relieve diseases of the kidney and bladder

lily: see flower-de-luce: i.e. the ornamental needle on a compass

litharge of gold, litharge of silver: the impure lead oxide blown away during the cupellation of a gold or silver alloy, respectively

lithodendron: 'stone leaf', a kind of succulent

lixivium: water impregnated with alkaline salts, made by percolating water through ashes or other materials

London treacle: an antidotal medicine like Venice treacle and mithridate (q.v.), made from hartshorn, citron, sorrel, peony, basil and many other ingredients

ludus: a Paracelsian and Helmontian term for a type of marl believed to be efficacious against the stone

luna cornea: 'horny moon', horn silver (q.v.) (the name alludes to the traditional association of silver with the moon)

luna fixa: 'fixed moon', a metal that has the weight and chemical properties of gold, such as resistance to nitric acid, but lacks its gold colour (L. Principe, *The Aspiring Adept* (Princeton, 1998), p. 81n)

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lythargyrium auri: 'litharge of gold' (q.v.)

magistry: 'mastery', a Paracelsian term referring to a preparation of any material in which there is no separation of parts, but rather the reduction of the entire substance into a new form

mandioca: i.e. manioc, or cassava

manipulus: 'handful', a measure used in medicinal preparations

manubrium: 'handle'

marrubium album: 'white marruvium', the common horehound, whose bitter leaves were used for coughs and asthmas

martial: of or pertaining to iron (from iron's identification with the planet Mars)

mechoacan: the root of a Mexican species of bindweed, used as a strong purgative; its qualities are similar to those of the jalap root

meleteticks: rules or method of meditations

melilot plaster: a plaster made from rosin, wax, and sheep's-suet, to which the melilot flower is added

mellago: a medicine which has the consistence of honey

menstruum peracutum: a solvent developed by Boyle himself, made by distilling aqua fortis with butter of antimony (antimony trichloride); Boyle claims that it volatilises gold and can transmute a portion of that metal into silver (L. Principe, *The Aspiring Adept* (Princeton, 1998), pp. 80-6)

mercurius dulcis (sublimatus): 'sweet mercury', i.e. calomel, mercurous chloride; used as a laxative; also known as sweet sublimate

mercurius vitae: antimony oxychloride, a poisonous and violently emetic white powder made by precipitating butter of antimony (q.v.) with water; later known as algaroth, or pulvis Algarotti

Mercury: one of the principles (Tria Prima), denoting the heavy, fluid, or metallic component in any substance

mercury, running: a liquid mercury, either the element (Hg) or the principle, as above

mercury precipitate, mercurius precipitatus (corrosivus): mercuric oxide, a poisonous red powder; used as an application to wounds and ulcers; also known as red precipitate, precipitate per se, or precipitate

mercury sublimate, mercurius sublimatus (corrosivus): mercuric chloride, poisonous white powder prepared by subliming mercury, vitriol and common salt; used in various preparations to cure venereal disease; also known as corrosive sublimate, white sublimate, or sublimate

mesaraick: of or pertaining to the mesentery

meserion: the spurge-olive or widow-wail; a plant whose bark was used with mercury in treatments for syphilis and whose crushed blossoms could serve as an acid-alkali indicator

methodus medendi: the traditional system of diagnosis and therapy inherited from Galen

mina: a unit of measure of 100 drams used in Greece, Egypt and western Asia

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minium: red form of lead oxide, made by roasting lead or litharge (lead monoxide) in air; also known as red lead

miston: mixture

mithridate: a compound substance consisting of myrrh, saffron, ginger, cinammon, spikenard, and several other odoriferous spices and resins, and used as a universal antidote; also known as the Mithridate of Damocrates, or Venice treacle

monadical: pertaining to an individual elementary being, reflecting within itself the whole universe

mordicant: sharp, biting

mother: (1) a name for the womb, whose disorders were deemed to cause hysteria; (2) the mucilaginous substance formed in vinegar during fermentation; (3) the residual solution after a crystallisation

mucro: a sharp point or apex

mummmia: a medicinal preparation made from the liquor which exudes from embalmed carcasses,

used to dissolve coagulated blood; Paracelsus also uses the term to signify the spirit supposed to exist in all living bodies and to remain some time after death; also powdered mummy, used as a pharmaceutical

mundick: Cornish miners' name for iron pyrites

Muscovy glass: i.e. muscovite, a silver-grey form of mica

naker: shell-fish that yields mother-of-pearl

naturist: one who accepts the vulgar notion of nature

nihil: tutty, zinc oxide; also known as pompholix, lapis tutiae

obliquating: turning or bending aside

oculi cancrorum: 'crabs' eyes' (q.v.)

oculus manati: 'eye of the manatee', see lapis manati

oculus mundi: 'eye of the world', hydrophane, a type of yellow and transparent opal, considered a general antidote to illnesses

oculus porcinus: 'porcupine eye', see lapis porcinus

offa alba: white precipitate of ammonium carbonate described by van Helmont, obtained by mixing spirit of urine and spirit of wine (W.R. Newman, *Gehennical Fire* (Cambridge, Mass., 1994), pp. 182-3)

oil of sulphur (per campanam): see sulphur, spirit of

oil of tartar (per deliquium): see tartar, oil of

oil of vitriol: see vitriol, oil of

oleum myrrhae: 'oil of myrrh', a thick, pale yellow and volatile oil, used in mouth washes

oleum sabinae: 'oil of savin', an essential oil derived from the distilled leaves and tops of the Eurasian juniper (*Juniperus sabina*); thought to open obstructions in the uterus

oleum sulphuratum: see balsam of sulphur

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oleum sulphuris per campanam: see sulphur, spirit of

oleum tartari per deliquium: see tartar, oil of

ophthalmia sicca: 'dry ophthalmia', xerophthalmia, a thickened and abnormally dry condition of the mucous membrane covering the inner eyelids

opificer: maker, framer, fabricator

orthopnea: a form of asthma in which breathing is only possible in an upright position

os hyoides: a u-shaped bone or bones at the base of the tongue



os pubis: either of two bones which form the lower and more forward part of the pelvis

os sepiae: the internal calcareous shell of cuttlefishes (genus *Sepia*); used as an astringent and in toothpowders

oscitant: dull, sluggish

osteocolla: a deposit of carbonate of lime forming an incrustation on the roots and stems of plants, used as a treatment in setting broken bones

oxycroceum: 'an emplaster of vinegar and saffron', a plaster made from yellow wax, colphony, Burgundy pitch, an assortment of resins and gums, and saffron

panaritium: an alternative form of paronychia (q.v.)

paracentesis: the operation of evacuating the water from a cavity in the body, as in ascites or dropsy

parable: readily prepared or procured

paralipomena: things omitted in the body of a work, and appended as a supplement

pared: prepared

paronychia: whitlow, inflammation about the finger-nails caused by trapped pus

patefactions: manifestations or revelations

pearl (on the eye): cataract, an opaque thin white film over the eye

pentad, pentades: a group of five

per campanam: a method of trapping and condensing vapours in a bell-shaped vessel (*campana*); see sulphur, spirit of

per deliquium: 'by deliquescence', the reduction of a solid placed in a damp place to a liquid by its absorbing moisture from the atmosphere

pericarpia: medicines and plasters applied to the wrist as cures for various afflictions

Peruvian bark: see Jesuit's bark

phrenetis: inflammation of the brain or its membranes, attended with delirium and fever

pica: a depraved appetite for unnatural substances unfit for food

pilulae lunares: 'lunar pills', silver pills, made from silver dissolved in nitric acid, evaporated into crystals and mixed with a solution of nitre in water; used for dropsy and headaches (the name alludes to the traditional association of silver with the moon)

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pilulae mastichinae: 'mastic pills', a gentle purgative made from mastic, aloes, and other ingredients

pompholix: tutty, zinc oxide; also known as lapis tutiae, nihil; also a bubble or vesicle on the skin

pompion: pumpkin

populeon: an ointment made from the buds of the white poplar

precipitate (as a specific chemical substance): see mercury precipitate

precipitate per se: see mercury precipitate

pregravitate: to gravitate more than something else

repollent: predominating

primogeneal: original, primitive; especially referring to organisms that are simple types of their genus

pudding-pipe tree: *cassia fistula*, an Indian tree with large bulbous fruit

pugil: 'a pinch'; an eighth part of a handful (manipulus)

pyrobolist: one who makes or manages artillery or fireworks

ravings: uncontrolled thoughts

recrementitious: pertaining to dross, scoriae (q.v.), the useless part of a substance

red precipitate: see mercury precipitate

redargution: refutation

regulus: a lump of metallic material produced by the reduction of mineral ores in the fire; usually referring specifically to metallic antimony

regulus martis: 'regulus of Mars', metallic antimony, reduced from its native sulphide ore by the use of iron (the name alludes to the traditional association of iron with the planet Mars)

regulus martis stellatus: 'stellate regulus of Mars', regulus martis whose surface is covered with a striking crystalline pattern resembling a star

renitency: resistance

reverberium: reverberatory furnace, used for obtaining high temperatures

rob: thickened and condensed juice; particularly syrup of boiled fruit

roch-allom: double sulphate of aluminum and potassium, found efflorescent on the surface of bituminous schists

Roman vitriol: iron sulphate (green vitriol)

rorid: dewy

saccharum saturni: 'sugar of lead', see lead, sugar of (the name alludes to the traditional association of lead with the planet Saturn)

salamanders' blood: see blood of salamander

sal armoniack: i.e. sal ammoniac, a mixture of ammonium salts, generally predominantly ammonium chloride

sal enixum: crude potassium bisulphate, the by-product of making nitric acid from oil of vitriol and nitre

sal gem: rock salt, sodium chloride in its native mineral form, found as crystals in the earth

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sal prunellae: salt of live coal; a salt (predominantly potassium carbonate) prepared by casting a small burning coal (prunella) into melted nitre; used medicinally to soothe the throat

sal circulatum: according to Paracelsus, a salt which could reduce substances to their primordial state; used by van Helmont as a synonym for the alkahest, the universal solvent

sal tartari: see tartar, salt of

salt of tartar: see tartar, salt of

sampier: the samphire or sea-fennel plant

sandarach: realgar, arsenic sulphide, a red lustrous mineral used as a pigment

sand-furnace: sand-bath; also known as balneum arenae (q.v.)

sanguiferous: 'blood-carrying', pertaining to veins and arteries.

sanguis draconis: see dragon's blood

sarcotick: promoting the generation of flesh in wounds

searce, searced: sieve, sieved

secundines: the placenta with the membranes and umbilical cord, the afterbirth

seed-lac: see gum lacca (q.v.)

semen dauci: see daucus

sigillum Salomonis: 'seal of Solomon', a plant of the genus *Polygonatum*

slinck: the skin or flesh of a prematurely-born calf or sheep

sordes: dirt; filthy refuse or deposits

sows-, hogs-lice: sowbugs, woodlice

spagyrist: alchemist, or, more precisely, practitioner of *spagyria*, the practice of separating a material into its component principles and then recombining them into a purified, more active form (L. Principe, *The Aspiring Adept* (Princeton, 1998), p. 9)

spaud: i.e spauld, the shoulder of a human or, more usually, an animal

species: the separate materials from which compounds are formed; also an old name for medicinal powders

spirit of blood: a volatile liquid prepared by the destructive distillation of blood

spirit of hartshorn: a solution of ammonia and ammonia salts (particularly the carbonate) made from

distilling deer antlers, etc.

spirit of nitre: nitric acid

spirit of salt: hydrochloric acid or muriatic acid

spirit of urine: an aqueous solution of ammonia and ammonium carbonate prepared by the gentle distillation of putrefied urine

spirit of vitriol: see vitriol, spirit of

spirit of wine: ethyl alcohol

spiritus ardens Saturni: 'burning spirit of Saturn', impure acetone, an inflammable substance produced from the distillation of lead acetate, sugar of lead (the name alludes to the traditional association of lead with the planet Saturn)

stiriae: long pointed concretions, such as icicles or stalactites

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stuple: a little stupe, a piece of flannel with which a hot dressing for a wound is applied

Stygian water: a fuming, stinking corrosive fluid; sometimes aqua regia or another strong mineral acid; or a water made with mercuric chloride (mercury sublimate) (q.v.) or sulphur

sublimate, sublimatum corrosivum: see mercury sublimate

subtiliation: sublimation

succus pancreatis: fluid secretions of the pancreas.

sugar of lead: see lead, sugar of

sugillation: a weal, bruise, or black spot on the skin; the spots occurring in disease and in post-mortem changes

sulphur, oil of: see sulphur, spirit of

sulphur, spirit of: an acidic fluid (largely sulphurous and sulphuric acids) prepared by burning sulphur under a moistened bell jar (*campana*) and collecting the condensed fumes; also known as oil of sulphur, oil of sulphur per campanam, oleum sulphuris per campanam or oleum sulphuris

sulphur of antimony: see antimony, sulphur of

sulphur of Venus: see Venus, sulphur of

sulphur of vitriol: see vitriol, sulphur of

sulphur vivum: native sulphur

superficieculae: 'little superficies', a latinate diminutive of superficies

superonerated: overloaded or excessively burdened

sweet sublimate: see mercurius dulcis



swig: to castrate a lamb

syrup of violets: see violets, syrup of

tartar, oil of: (1) concentrated solution of potassium carbonate, made by allowing salt of tartar (potassium carbonate) to absorb atmospheric moisture (*per deliquium*), also known as oil of tartar per deliquium or oleum tartari per deliquium; (2) the empyreumatic oil distilled from crude tartar (predominantly potassium bitartrate)

tartar, salt of: potassium carbonate, or sal tartari

tartar, volatile salt of: a highly sought-after Helmontian medicine, purportedly having great solvent powers, and good for the stone

tartari volatile: see tartar, volatile salt of

tartarus vitriolatus: usually potassium sulphate; made by the reaction of salt of tartar with oil of vitriol

tenent: tenet; etymologically, a tenent is what several people believe, while a tenet is what one person believes

terra damnata: 'damned earth', i.e. caput mortuum (q.v.)

terra foliata: 'foliated earth', sometimes the crystalline form of potassium acetate

terra Japonica: see 'Japonian earth', catechu

terra Lemnia: 'earth of Lemnos', a type of terra sigillata (q.v.); Lemnian earth was renowned as an antidote to poison

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terra Samia: 'earth of Samos', a variety of terra sigillata (q.v.); Samian earth was renowned as a styptic

terra sigillata: 'sealed earth', a type of clay used medicinally for its astringent and sudorific properties

terra Silesiaca: another term for terra sigillata

terrella: a little earth, a small globe; a spherical loadstone

tersion: the act of wiping

ticket: note or memorandum

tin, butter of: stannic chloride

tin glass: bismuth

trochisch: pastil or lozenge, a medicinal substance made into a pill designed to dissolve slowly in the mouth

tunicles: small membranes enclosing bodily organs; the diminutive of 'tunic', membrane

turbith mineral, turpeth mineral: a hydrolysed form of mercuric sulphate, a lemon yellow powder used as an emetic, purgative and treatment for syphilis; made by digesting mercury with sulphuric acid and washing the resultant white powder with water

tutenag: an alloy of copper, zinc and sometimes iron

umbratile: secluded, or shadowy, unsubstantial, unreal

unguent populeon: 'ointment of poplar-buds', see populeon

urachus: a fibrous cord binding the apex of the bladder to the anterior abdominal wall and the peritoneal folds.

uvea: the posterior coloured surface of the eye

vappa: flat or sour wine

varen: i.e. vareni, or ambulo or flatus furiosus, a disease marked by a painful periodic distension of the abdomen

Venetian soap: fine hard soap made from olive oil and alkali

Venetian sublimate: mercury sublimate (q.v.)

Venice glass: a very fine kind of glass, originally manufactured at Murano, near Venice

Venice treacle: a compound mixture consisting of a wide variety of ingredients which was used as a universal antidote; also known as treacle of Andromachus

Venus, sulphur of: the putative combustible component of copper, so named because of the traditional association of copper to the planet Venus

violets, syrup of: a blue liquid, made from the flowers of violets, which changes to red with the addition of an acid substance and to green with that of an alkali

virgin wax: fresh or unused beeswax

viride aeris: the green 'rust' which forms on copper exposed to the weather

vitriol, oil of: concentrated sulphuric acid

vitriol, spirit of: sulphuric acid made by distilling one of the vitriols (either iron or copper sulphate)

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vitriol, sulphur of: the putative combustible component of vitriol

vitriolum martis: 'vitriol of Mars', iron sulphate (from iron's identification with the planet Mars)

vitrum antimonii: see antimony, glass of

vitrum Saturni: see lead, glass of (the name alludes to the traditional association of lead with the planet Saturn)

walms: a state or period of boiling

water radish: the *Sisymbrium nasturtium* plant

white fluors: 'white flux', in mineralogy, a white mineral used as a flux in the melting and refining of metals; in medicine, the whites (q.v.)

whites, the: leucorrhea, a secretion of whitish or milky mucous from the membrane lining the uterus

## Author's Notes

†@ L. Annæi Senecæ Natur. Quest. 1. 6. c. 5. [*Naturales quaestiones*, vi. 5. 3, by the Roman Stoic philosopher Lucius Annaeus Seneca (4 bc-ad 65). 'Nothing is completed at the point of its inception. This is so not only in this greatest and most complex of subjects, where, though much shall already have been done, every age will still find something to do, but in every other matter, too, the beginnings have always been far removed from the completed form.']

†@ He that desires more instances of this kind and matter, that according to this doctrine may much help the Theory of colours, and particularly the force both of Sulphureous and volatile, as likewise of Alcalizate and Acid Salts, and in what particulars, Colours likely depend not in their causation from any Salt at all, may beg his information from M. *Boyle*, who hath some while since honoured me with the sight of his papers concerning this subject, containing many excellent experiments, made by him for the Elucidation of this doctrine, &c. Dr. R. *Sharrock* in his ingenious and usefull History of the Propagation and Improvement of Vegetables, published in the yeare 1660. [This quotation from Robert Sharrock's *The History of the Propagation and Improvement of Vegetables* (Oxford, 1660), p. 77, has in fact been slightly emended, substituting 'M. *Boyle*' for 'that Noble person (in order to whose command, for all his intimations to me are such I am now writing)', and 'him' for 'his Honor'. The book in question had in fact been dedicated to Boyle by Sharrock (1630-84), a cleric and natural philosopher who also acted as editor of Boyle's *Spring of the Air* (1660), *Defence* (1662) and the first volume of *Usefulness* (1663): see above, vols. 1 and 3.

†@ See the Discourse of the Nature of Whiteness and Blackness [i.e., part 2 of this work: see below, pp. 77ff.].

†@ Since for his eminent Qualities and Loyalty Grac'd, by his Majesty, with the Honour of Knighthood. [Sir John Finch (1626-82) received his MD at Padua and became English consul there before being appointed professor at Pisa by the Grand Duke of Tuscany. Returning to England at the Restoration, he was knighted in 1661.]

†@ Exercitat. 325. Parag. 4. [*Exotericarum exercitationum liber XV de subtilitate ad Hieronymum Cardanum* (1557), by the Italian physician and natural philosopher Julius Caesar Scaliger (1484-1558). 'Its nature lies hidden, like the form of other things, in the impenetrable darkness of human ignorance.']

†@ Album quippe & nigrum, hoc quidem asperum esse dicit, hoc verò læve. de Sensu 3. & Sensil. 3. [Aristotle, *De sensu et sensibilibus* (included in the *Parva naturalia*) 442b11. 'Of white and black he says that the former is rough, the latter smooth.']

†@ Epist. 2. pag. 45. [Pierre Gassendi, *De apparente magnitudine solis humilis et sublimis epistolae quatuor* (1642). 'I should like you to consider this: when it falls on a transparent object, light seems colourless, but when it is impeded by something opaque, it looks white, and the more so the more dense or compressed it is. Thus, water is not of itself white in colour, but the ray of light reflected by it onto the eye looks white. Again, when the surface of the water is flat this reflection happens from one side only: but if the water happens to swell up into bubbles, each bubble causes a

reflection, and creates the white appearance on a certain part of its surface. Furthermore, foam formed from pure water seems to shine and look white for the very reason that it is a densely packed mass of the tiniest bubbles, each one reflecting its own ray of light, and thus creating the effect of continuous whiteness or shininess. And finally, snow seems to be nothing other than a sort of very pure foam formed from the very tiniest and most densely packed bubbles. But to elaborate these trivialities of mine any further would lay me open to ridicule.']

†@ Gent. Septen. Histor. lib. 4. cap. 13. [*Historia de gentibus septentrionalibus* (1555), by Olaus Magnus (c. 1498-1568), Archbishop of Uppsala. 'By day they travel two montane miles (which are twelve Italian ones). But by night, under the most luminous moon, they manage two or three times as far. There is no difficulty in this, as the moonlight, reflected by the snow, illuminates the fields high and low, and they can even see far ahead to avoid sudden precipices and wild beasts.' Lat. lacks this note.]

†@ Hist. Anatom. Cent. 3. Hist. 44. [*Historiarum anatomicarum rariorum centuria I-VI* (1654-61) by the Danish anatomist, Thomas Bartholin (1616-80). The Earl of Mansfeld was evidently a Danish aristocrat]

†@ Olearius Voyage de Mosco. et de Perse liv. 3. [Adam Olearius (1600-71), secretary to the Duke of Holstein, wrote *Relation du voyage de Moscovie, Tartarie, et de Perse* (1656) based upon his journeys for the Duke.]

†@ Piso Nat. & Med. Hist. de Brasil. lib. 1. in fine. [Willem Piso (1611-78), *De medicina Brasiliensi*, published as the first four books in Georg Markgraaff, *Historia naturalis Brasiliae* (1648). Piso was physician to the Dutch settlement in Brazil from 1636 to 1644.]

†@ Purchas Pilgrim. Second part, Seventh Book 3. Chap. Sect. 5. [*The Strange Adventures of Andrew Battel of Leigh in Essex, sent by the Portugals prisoner to Angola*, as found in what is usually referred to as the 7th book of the 1st part of Samuel Purchas, *Hakluytus Posthumus: or, Purchas his Pilgrimes: Contayning a History of the World in Sea Voyages and Lande Travells by Englishmen and Others*. (1625).]

†@ Purchas Ibid. [See above.]

†@ Purchas Ibid. in fin. [See above, p. 92n.]

†@ See Scaliger Exercit. 325. Sect. 9. [See above, p. 60.]

†@ Nicolaus Monardes lib. simplic. ex India allator. cap. 27. [*De simplicibus medicamentis ex occidentali India* (1574), the Latin edition of a book about drugs from America by Nicolás Bautista Monardes (1493-1588), the most famous Spanish physician of the 16th century, whose works were translated into several languages, including Latin and English. 'From New Spain we get a certain type of wood, dense and lacking knots, which has long been used in these parts to fend off kidney diseases, urinary problems and gravel. It is done in the following manner. The wood is cut up into tiny slivers and soaked in the clearest spring water, where it is left until the water is drunk. Half an hour after the wood is put into it, the water takes on a bluish colour that gradually deepens with the passage of time, though the wood is itself white.']

†@ Kircher. Art. Mag. lucis & umbræ, lib. 1. part. 3. [*Ars magna lucis et umbrae in mundo* (1646) by the Jesuit natural philosopher, Athanasius Kircher (1602-80). 'We cannot possibly pass by without mentioning a certain type of white wood from Mexico, which the natives call *Coalle* or *Tlapazatli*. Though experiments have hitherto indicated that it only colours water blue, we have found by our continual experiments that it turns water any colour, which may well seem a paradox.



A full-grown shrub of the wood, so they say, often reaches the size of a tree, with a thick trunk of knotless wood, like that of a pear tree, with leaves not unlike those of chickpea or rue, and small, longish, yellow flowers arranged

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like ears of corn. It is a cold and moist plant, albeit only a little removed from the mean temperament. The wood of this tree, shaped into a cup, imparts a deep blue colour, that of the Bugloss flower, to any water poured into it, the colour getting ever deeper the longer the water stays in it. But if you pour this water into a glass sphere and expose it to the light, you will not see even a trace of the bluish colour, and it will look clear and transparent like pure, untainted spring water. Furthermore, if you move this glass flask towards a more shaded spot, the entire liquid turns a very pleasant green. And in still shadier places it takes on a reddish colour, and, strange to say, changes colour according to the nature of objects placed nearby. But when placed in darkness or in an opaque flask it resumes its blue colour.']

†@ Herbarists are wont to call this Plant *Cyanus vulgaris minor*.

†@ Paracelsus de Mineral. tract. 1 pag. m. 243. [I.e., 'page 243 in my copy'. A reference to *De mineralibus liber* by the Swiss iatrochemist Theophrastus Bombastus von Hohenheim, or Paracelsus, published in vol. 2 of his *Opera omnia medico-chemico-chirurgica* (1658), though Boyle clearly cites a different edition. 'Otherwise, the nature of colours is similar. In brief, what I have to say on this matter is as follows: all colours derive from salt. For salt gives colour, as it gives balsam ... Now Nature herself extracts colours from the salt, giving to each species the colour that is appropriate to it.... And so whoever wishes to know the bodies of all things must needs first of all know Sulphur. Then, he who desires to know colours may seek knowledge of them from Salt. He who wishes to know the virtues, let him examine the secrets of Mercury. In this way, he shall truly draw out the foundation of investigating the Mysteries in whatever becomes visible, according to whatever guise Nature cloaks them with.' Lat. lacks this note.]

†@ See Parkinson Th. Botan. Trib. 9. cap. 26. [*Theatrum Botanicum* (1640) by the English herbalist and apothecary John Parkinson (1567-1650).]

†@ Parkinson, Thea. Bot. Trib. 4. cap. 12. [See above, p. 136n. Lat. lacks this note.]

†@ Beguinus. Tyr. Chy. Lib. 2¼ Cap. 13¼. [*Tyrocinium chymicum e naturae fonte et manuali experientia depromptum* (1610) by the French chemist, Jean Beguin (c. 1550-1620)]

†@ Libr. 2do. Cap. 34¼. [*Musæum Wormianum seu historia rerum rariorum* (1655), by the Danish physician and virtuoso, Olaus Worm (1588-1634)]

†@ See the latter end of the fiftieth Experiment [p. 183 below. For the famous Bow dye, produced by Cornelius Drebbel (1572-1633) and his successors, see Charles Webster, *The Great Instauration* (London, 1975), pp. 388-9].

†@ The Curious Reader that desires further Information concerning Lakes, may Resort to the 7th Book of Neri's Art of Glass, Englished (6 or 7 years since the Writing of this 49th Experiment) and Illustrated with Learned Observations, by the Inquisitive and Experienc'd Dr. Charles Merret. [Boyle mis-cites the name of Christopher Merrett (1614-95), MD, author of the 'Account of Freezing' that was printed at the end of Boyle's *Cold* (1665), below, p. 195. Boyle apologised through Oldenburg; see his letter of c. 2 October 1664 in *Correspondence*. Merrett was a prominent Fellow of the College of Physicians and the author of works on natural history. At Boyle's urging, he had translated Antonio Neri's *L'arte vetraria* (1612) into English as *The Art of Glass* (1662), adding numerous observations of his own, and dedicating the work to Boyle. See W. E. S. Turner,

'A Notable British Seventeenth-Century Contribution to the Literature of Glassmaking,' *Glass Technology*, 3 (1962), 201-13.]

†@ Boetius de Boot. *Gem. & Lapid. Histor. Lib. 2. Cap. 8.* [Anselmus Boetius de Boodt (c. 1550-1632), a Fleming, was from 1604 to 1612 physician to Rudolf II, for whom he wrote his *Gemmarum et lapidum historia* (1609). Boyle almost certainly cites the 3rd edition (Leiden, 1647), since that also contained the work by John de Laet cited next in the text. 'Great is the renown of the carbuncle. It is commonly thought to glow in the dark like coal, perhaps because it was called *pyropus* ['fiery eyed'] or *anthrax* ['coal'] by the Ancients. But hitherto no-one has ever been so bold as to truly claim that he has seen the gem glowing at night. Garcia d'Orta, doctor to the Viceroy of the Indies, relates that he conversed with some who claimed to have seen it, but he did not credit their tale.' Here, de Boot cites d'Orta's *Coloquios dos simplos* (1563).]

†@ Musæi Wormiani. Cap. 17. [Worm, *Musæum Wormianum seu historia rerum rariorum* (1655). See above, p. 164. 'There are some that think that the ruby is the carbuncle of the ancients, but one characteristic is lacking, namely that it does not glow in the dark like coal. However, the majority of writers hold that such a carbuncle is not to be found in nature. Even though they record that one or two can be found in the possession of the princes of the Indies, they have this only on the authority of others, not having seen it for themselves.']

†@ Purchas's Pilgrim. lib. 1. cap. 4. pag. 104. [*The first booke of Marcus Paulus Venetus, or of Master Marco Polo, a Gentleman of Venice, His Voyages*, in the 1st book of the 2nd part of Samuel Purchas, *Hakluytus Posthumus: or, Purchas his Pilgrimes: Contayning a History of the World in Sea Voyages and Lande Travells by Englishmen and Others.* (1625). The 'King of Ceylon' was evidently the great Khan.

†@ In the year 1619. [Clement Oboskin de Tobolsk and Burnash Nikomene, who set out on 6 July 1619; their account was addressed to Tsar Michael Feodorovitch Romanov (1596-1645)]

†@ Benvenuto Cellini nell Arte del Gioiellare, Lib. 1 pag. 10. [*Due Trattiti, uno intorno alle otto principali arti dell'oreficeria, l'altro in materia dell'arte della scultura* (1568), by Benvenuto Cellini (1500-71).]

†@ The Narrative in the Authors own words, is this, Ego (says he) sanctè affirmare possum me unam aureo Annulo inclusam perpetuo gestare, cujus facultatem (si gemmæ est) nunquam satis admirari potui. Gestaverat enim ante Triginta annos Hispanus quidam non procul à paternis ædibus habitans. Is cum vitâ functus esset, & ipsius suspellex (ut moris apud nos est) venum exposita esset, inter cætera etiam Turcois exponebatur. Verum nemo (licet complures eo concurrissent, ut eam propter Coloris Elegantiam, quam vivo Domino habuerat emerent) sibi emptam voluit, pristinum enim nitorem & Colorem prorsus amiserat, ut potius Malachites, quam Turcois videretur. Aderat tum temporis gemmæ habendæ desiderio etiam parens & frater meus, qui antea sæpius gratiam & elegantiam ipsius viderant, mirabundi eam nunc tam esse deformem, emit eam nihilominus pater, satisque vili pretio, qua omnibus contemptui erat, ac presentes non eam esse quam Hispanus gestarat, arbitrarentur. Domum reversus Pater, qui tam turpem Gemmam gestare sibi indecorum putabat, eam mihi dono dat, inquires; Quandoquidem, fili mi, vulgi fama est, Turcoidem, ut facultates suas exercere possit, dono dari debere tibi eam devoveo, ego acceptam Gemmam Sculptori trado, ut[printed as 'at'] gentilitia mea insignia illi, quamadmodum fieri solet, in Jaspide Chalcedonio, aliisque Ignobilioribus Gemmis, insculperat. Turpe enim existimabam, hujusmodi Gemmâ ornatus gratia, dum gratiam nullam haberet, uti. Paret Sculptor redditque Gemmam, quam gesto pro annulo Signatorio. Vix per mensem gestaram, redit illi pristinus color, sed non ita nitens propter Sculpturam, ac inæqualem superficiem. Miramur omnes gemmam, atque id præcipuè quod color indies pulchrior fieret. Id quia observabam, nunquam fere eam à manu deposui, ita ut nunc adhuc eandem gestem. [de Boodt, *De gemmis*. See above, p. 396, n. 1. 'I can solemnly affirm that I

wear one at all times, set in a gold ring, whose powers (if they are indeed the gemstone's) I have never ceased to wonder at. Thirty years ago it was worn by a certain Spaniard who lived not far from my father's house. When he died and his chattels were put on sale (as is the custom in our parts), the turquoise was displayed along with the rest. But though many had rushed there to buy it on account of the beautiful colour it had when its owner was alive, nobody now wanted to buy it, for it had completely lost its old splendour and colour, so that it seemed more like malachite than turquoise. My father and my brother were also there at the time, with a desire to own the gem, having seen its charm and beauty many times before, but now amazed to see it so ugly. Despite this, my father bought it, at a very low price, it being disdained by all, and those present believing that it was not the one worn by the Spaniard. My father came home and, thinking it unseemly for him to wear such an ugly gem, gave it to me as a gift with these words: "Since, my son, it is commonly said that to exercise its powers a turquoise must be given as a gift, I therefore give it to you." I took the stone and gave it to an engraver, to have my coat of arms engraved on it, as is the custom with jasper, chalcedony and other semi-precious stones. for I thought it an absurdity to adorn myself with such a gem when it was in fact no adornment. The engraver came and gave me back the

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gem, and I began to wear it as a signet ring. I had scarcely worn it for a month, when its old colour came back, though it was not as sparkling because of the engraving and the uneven surface. We all admired the stone, and particularly the fact that its colour became more beautiful every day. Bearing this in mind, I have hardly ever taken it off my hand, and I wear it still to this day.']

†@ Olaus Wormius in Musæ. 18°. pag. 186. [Worm, *Musæum Wormianum*: see above, p. 164. 'Anselm de Boodt relates from his own experience a highly memorable example both of the changed colour and of its chance preservation. To this, I myself could add a not dissimilar example, should anyone not believe the account sought out of Anselm.']

†@ Musæ. Worm. pag. 99. [See above, p. 164.]

†@ Arte Vetraria, lib. 7. cap. 102. [The Italian priest Antonio Neri (1576-c. 1614) revealed many carefully guarded secrets of the art of glassmaking in his little book, *L'arte vetraria distinta in libri sette* (1612). See above, p. 177n.]

†@ These were brought in and Read before the Royal Society, (the Day following) Oct. 28 1663. The Stone it self being to be shown to the Royal Society, when the Observations were deliver'd, I was willing (being in haste) to omit the Description of it, which is in short, That it was a Flat or Table Diamond, of about a third part of an Inch in length, and somewhat less in breadth, that it was a Dull Stone, and of a very bad Water; having in the Day time very little of the Vividness of ev'n ordinary Diamonds, and being Blemished with a whitish Cloud about the middle of it, which covered near a third part of the Stone.

†@ Hast made me forget to take notice that I went abroad the same Morning, the Sun shining forth clear enough, to look upon the Diamond through a *Microscope*, that I might try whether by that Magnifying Glass any thing of peculiar could be discern'd in the Texture of the Stone, and especially of the whitish Cloud that possest a good part of it. But for all my attention I could not discover any peculiarity worth mentioning.

†@ V. For it drew light Bodies like Amber, Jet; and other Concretes that are noted to do so; But its attractive power seem'd inferiour to theirs.

†@ IX. We durst not hold it in the Flame of a Candle, no more than put it into a naked Fire; For fear too Violent a Heat (which has been observ'd to spoil many other precious Stones) should vitiate and impair a Jewel, that was but borrow'd, and was suppos'd to be the only one of its Kind.



†@ XV. We likewise Plung'd it as soon as we had excited it, under Liquors of several sorts, as Spirit of Wine, Oyl both Chymical and express'd, an Acid Spirit, and as I remember an Alcalizate Solution, and found not any of those various Liquors to destroy its Shining property.

†@ XVI. Having found by this Observation, that a warm Liquor would not extinguish Light in the Diamond, I thought fit to try, whether by reason of its warmth it would not excite it, and divers times I found, that if it were kept therein, till the Water had leisure to communicate some of its Heat to it, it would often shine as soon as it was taken out, and probably we should have seen it Shine more, whilst it was in the Water, if some degree of Opacity which heated Water is wont to acquire, upon the score of the Numerous little Bubbles generated in it, had not kept us from discerning the Lustre of the Stone.

†@ I after bethought my self of imploying a way, which produc'd the desir'd Effect both sooner and better. For holding betwixt my Fingers a Steel Bodkin, near the Lower part of it, I press'd the point hard against the Surface of the Diamond, and much more if I struck the point against it, the Coruscation would be extremely suddain, and very Vivid, though very Vanishing too, and this way which commonly much surpris'd and pleas'd the Spectators, seem'd far more proper than the other, to show that pressure alone, if forcible enough, though it were so suddain, and short, that it could not well be suppos'd to give the Stone any thing near a sensible degree of Warmth, as may be suspected of Rubbing, yet 'tis sufficient to generate a very Vivid Light.

†@ We afterwards, try'd precious Stones, as Diamonds, Rubies, Sapphires, and Emeralls, &c. but found not any of them to Shine except some Diamonds, and of these we were not upon so little practice, able to fore-tell before hand, which would be brought to Shine, and which would not; For several very good Diamonds, either would not Shine at all, or much less than others that were farr inferiour to them. And yet those Ingenious Men are mistaken, that think a Diamond must be foul and cloudy, as Mr. *Claytons* was, to be fit for Shining; for as we could bring some such to afford a Glimmering Light, so with some clear and excellent Diamonds, we could do the like. But none of those many that we try'd of all Kinds, were equal to the Diamond on which the Observations were made, not only considering the degree of Light it afforded, but the easiness wherewith it was excited, and the Comparatively great duration of its Shining.

†@ It was thought needless to insert Mr. *Hobs*'s Scheme, touching this subject, because it only shews, that Wind is the cause of Cold.

†@ Sceptical Chymist. [*Sceptical Chymist* (1661): see above, vol. 2.]

†@ Chapter the fifth of that Treatise. [*Defence* (1662). See above, vol. 3, pp. 57-65.]

†@ The two Essays of the Unsuccessfulness of Experiments. [These were part of *Certain Physiological Essays* (1661): see above, vol. 2, pp. 35ff.]

†@ Another remarkable instance of the variable success of the Experiments of Cold, I have met with in an Experiment of the Learned Dr. *Merrets*, about the congealing of *oyl of Vitriol*. For though I expos'd that Liquor in small vessels of differing sizes and shapes, and even in slender glass-Pipes, seal'd but at one end, yet neither the Cold of the Air in frosty nights, that were extraordinarily sharp, nor which is more, our frigorifick mixture of Ice and Salt, would make the Experiment succeed, notwithstanding that we tri'd it with several parcels of *Oyl of Vitriol*. And yet, that the Learned Doctor by the help of Air alone (for he uses not our frigorifick mixture) did bring that Liquor, either to a true congelation, or a coagulated substance, that look'd just like Ice; both some eminent *Virtuosi*, and I my self, who had the Curiosity to examine it, can bear him witness. [On Christopher Merrett, the author of the 'Account of Freezing' found at the end of the present work, see above, p.177n.]



†@ Lib. 1. Titulo de frig. Asperitate, pag. 9. [Ch. xix of Olaus Magnus, *Historia de gentibus septentrionalibus* (1555), entitled 'De frigoris asperitate.' 'Since I was born in it, and have been as far north as the 86th degree, I consider that I am capable of showing in this chapter and in the many that follow, with greater clarity than those who write out of uncertain opinion, how fierce and terrible the cold there.']

†@ pag. 27, 28. [*Novus atlas Sinensis* (1654), by the Italian Jesuit natural historian Martinus Martini (1614-61). 'As regards the temperature of the Sky and the Land, it is colder in this clime than its latitude would suggest. For it lies scarcely beyond the forty-second degree, and yet the rivers are often all frozen so hard for four whole months that the ice can bear horses and carriages and the heaviest weights, and they may cross over in the greatest safety. Great blocks of ice are even carved out of them, for their convenience in the following Summer. Ships, in these months, are so hemmed in by the very ice that they cannot sail: wherever the cold takes them (it normally begins its assault about the middle of November), there they are forced to remain immobile for those four months: for the Ice does not loosen its grip before the beginning of March. The concretion of the ice normally takes place within a single day, though it takes several to melt.... What is quite astonishing is that this cold does not seem, or feel, so great as would impel Europeans to take a hot bath, or as would produce ice in Europe; wherefore the entire explanation for such matters must be sought in the subterranean exhalations that are found there.']

†@ See the 18. of our *New Physico-Mechanical Experiments*. [See above, *Spring of the Air* (1660), vol. 1, pp. 201-5.]

†@ In the defence against *Linus* cap.4 [See above, p. 209.]

†@ Dr. H.P. [Henry Power (1623-68), M.D., whose *Experimental Philosophy, in Three Books* (1664) included this test of Boyle's hypothesis about the spring of the air.]

†@ An Ingenious man has proposed another way of setting a Standard for Weather-glasses; namely, by observing the coldness, which is requisite to make distill'd water begin to freeze: But though the accurateness of this way may be as well as the other justly Question'd, and cannot often be put in practice, even in Winter it self, nor without trouble; yet it may also be advantageously made use of, when the cold happens to be great enough to freeze water. [Evidently a reference to Robert Hooke (1635-1703), Boyle's former laboratory assistant and at this point curator of experiments to the Royal Society, who put forward such views in his *Micrographia* (1665), pp. 38-9].

†@ Dr. Wren. Dr. Goddard. Mr. Hook. [Christopher Wren (1632-1723), LLD, was in 1665 Savilian professor of astronomy at Oxford; in 1663 Wren had presented the design of a weather clock to the Royal Society (Birch, *Royal Society*, i, 341). Jonathan Goddard (1616-75), MD, was professor of physic at Gresham College. For his interest in such subjects, see Birch, *Royal Society*, i, 338. On Robert Hooke, see above, p. 39n: for his role in relation to this book, see Introductory Note and below, p. 55]

†@ Defence against *Linus* Cap. the 5th. [See above, p. 209.]

†@ Theatr. Chynic. [sic] volum. 6. [Corrected to read 'Chymic.' in the 2nd edition: i.e. Lazarus Zetzner's collection, *Theatrum chemicum*, ed. J.J. Heilmannus (Strasbourg, 1659-61), vol. 6, which contained a treatise by Andreas Ortelius, *Orthelius commentator in novum lumen chymicum M. Sendovigii Poloni*.]

†@ *Vitrum optimè clausum ne quid exspirare posset, in loco ubi quiesceret statui, nec sine animi voluptate licebat in Pleniluniis manifesta inclusi liquoris incrementa observare, in Noviluniis vero Decrementa, &c.* [I stood a glass, very well stopped so that nothing could get out, where it might be

at rest: and it was possible at Full Moon to observe (not without a sense of pleasure) a manifest increase in the liquid enclosed therein, and a decrease at New Moon.] They are the words of *Paulus Casatus* in his *Terra Machinis mota*, Pag. 143. [Paolo Casati, *Terra machinis mota* (1658).] But since the writing of these Præliminary Discourses, the Author of them [i.e., Boyle] having consulted by the means of some Ingenious friends, the learned *Casatus*, finds, that He never made nor saw the Experiment himself, but relates it upon the authority of a certain Dutchman, whose name he adds not, and who therefore may probably be the same *Orthelius* that is mention'd by the Author of these Præliminary Discourses [see the previous footnote], who thinks it requisite to give the Reader this Advertisement, because *Casatus* himself did not, as he should have done, intimate that he deliver'd this but upon anothers credit.

†@ L'Hydrographie du P. Fournier, liv. 18. Cap. 12. [*Hydrographie, contenant la théorie et la pratique de toutes les parties de la navigation* (1643), by the French Jesuit hydrographer, Georges Fournier (1595-1652).]

†@ Defence against *Linus* Cap.3. [See above, p. 209, for a reference to *Defence*, where these issues are fully discussed. For Brouncker as the addressee of this work, see below, p. 263.]

†@ Sect. II. of the same 30. Chap. [Hobbes, *De corpore*: see above, p. 255.]

†@ See more concerning these Weather-glasses in the first of these three Discourses [in the present work, above, pp. 229ff.].

†@ See the figure among the rest of the Schemes [Plate 1, fig. 7, p. 228].

†@ pag. 58. [I.e., James, *Voyage*: see above, p. 221.]

†@ Olai Magni Gent. Sept. Hist. Lib. 11. Cap. 24. [Olaus Magnus, *Historia de gentibus septentrionalibus* (1555). 'An encounter on the ice is made in woollen shoes, not in those made of skin or oiled leather, for the strength of the cold converts whatever is unctuous into an icy slipperiness.']

†@ Olaus Magnus in *Historia Gentium Septentrionalium*, lib. 11. chap. 20. & 21. [See above, p. 274.]

†@ Cap.6. pag. 42. [Thomas Bartholin, *De nivis usu medico observationes* (1661): 'It is certain that ice formed from sea-water, if thawed, loses its brackish taste, as was discovered not so long ago by the illustrious Jacob Finck, highly accomplished senior member and professor of natural philosophy in our University, who removed some blocks of ice from our harbour.' On Bartholin, see above, p. 76.]

†@ See the Præliminary Discourses [above, pp. 229ff.].

†@ 4. Jan. 15. [Although this note appears next to the start of section 5, it is probably meant to assign a date (January 15) to the previous experiment (no. 4). The square brackets are in both cases Boyle's.]

†@ See the latter part of the next Title.

†@ In the Discourse touching the *primum frigidum* [Title XVII of the present work, pp. 364ff.].

†@ Guilielmus Fabritius Hildanus de Gangr. & Sphacelo Cap. 10. [*De gangraena et sphacelo*

*tractatus methodicus*, part of *Selectae observationes chirurgicae* (1598; originally published in French, 1597), by the German surgeon Wilhelm Fabricius von Hilden (1564-1634). 'A certain nobleman, of trustworthy character, has told me that once when he was journeying in those parts he came upon a traveller alongside the road who was rigid with cold, and practically dead. He placed him in his wagon, and took him to a dwelling-house, where the host immersed him in cold water, which brought about such a frost all over him, that his very body seemed encased in an icy (or rather an iron) suit of armour. The patient was then given, he said, a large cup of mead, a common drink among those people, spiced with cinnamon, cloves and mace, which, as he lay abed, brought him out in a sweat; and in this way was restored, though he lost the last joints of his fingers and toes. Thence we know that this method of curing frozen people is true and safe, and it is likewise proved by the greatest philosopher who frequents those regions, etc.']

†@ Barthol. de usu Nivis pag. 80. [Bartholin, *De nivis usu*: 'For this very purpose the court of Modena keeps snow tightly packed in snow cellars, where I have seen, in high summer, flesh from slaughtered animals preserved a long time from rotting.']

†@ Pag. 74. [James, *Voyage*.]

†@ Pag. 79.

†@ Barthol. de figurâ nivis pag. 79. [Boyle's note incorrectly cites Erasmus Bartholin, *De figura nivis dissertatio* (1661), a 42-page treatise bound with his brother's *De nivis usu*. The quoted passage is from p. 79 of the latter. 'He [Simler] relates that in the Rheinwald, in Rhaetia, a great mass of snow slid down the mountain and flattened the forest and the lofty pines, also hitting a body of Swiss soldiers on its way through the Alps so that 60 or more men were engulfed by the one avalanche. Buried under this heap of snow they lay hidden until summertime, when the snow to some extent melted, and the bodies were exposed, in perfect condition, to any friends or passers-by who might come looking. We ourselves beheld the sad sight.']

†@ Barthol. de usu Nivis pag. 83. ['It is to be remarked that the bodies of men killed by the cold remain in the same position, and the same rigid form, in which they were when it first came upon them. This was seen outside our own City when, on 11 Feb. 1659, our attackers were beaten back and fell amid great slaughter. Some were fixed rigid in an angry expression, some in a wide-eyed stare, some with open mouths, some with outstretched arms, menacingly proffering their sword, some lay prostrate in another place. Indeed, even after the first thaw, there emerged from the frozen sea an entire knight, astride his horse, and holding I know not what in his hands.']

†@ Capt. James's Trav. pag. 76. [James, *Voyage*.]

†@ Barthol. de usu Nivis Cap. 12. [Bartholin, *De nivis usu*, pp. 79-80.]

†@ Of the Usefulness of Experimental Philosophy. [I.e. *Usefulness*, Part I (1663), above, vol. 3, p. 265.]

†@ Purch. Lib. 3. cap. 5. Sect. 2. pag. 493. [Purchas, *Pilgrimes*.]

†@ Pag. 73. [James, *Voyage*.]

†@ Pag. 67. [James, *Voyage*.]

†@ Lib. 1. Sect. 3. Cap. 5. pag. 122. [Olaus Worm, *Musæum Wormianum seu historia rerum rariorum* (1655): 'Articles made of brass are sometimes broken by the force of the cold. Few credit

this, but Eratosthenes observed it, as did our own Jens Munk, on that exceedingly harsh voyage when he strove to find a passage into the South Sea through Christian's Strait.' On Worm., see above, p. 164.]

†@ Nicholaus Zucchius, & Melchior Cornæus [Niccolò Zucchi's anonymous pamphlet, *Magno amico nonnemo ex Collegio rom. S.J. Experimenta vulgata non vacuum probare, sed plenum, et antiperistasim stabilere* (1648) was reprinted as part of his *Nova de machinis philosophia* (1649); Melchior Cornæus, *Curriculum philosophiae peripateticae uti hoc tempore in scholis decurri solet* (1657).]

†@ It froze, so sore within the house, that the Walls & the Roof thereof were frozen two fingers thick with Ice, and also in our Cabins, where we lay all those three days, while we could not go out. Gerat de Veer in his third Voyage. [From de Veer, *Three Voyages*.]

†@ Pag. 64. [James, *Voyage*.]

†@ Feb. 4. 1661.

†@ Decemb. 11. 1662.

†@ Decemb.

†@ Decemb.

†@ Decemb. the 17.

†@ Barthol. de Nivis usu Chap.6. [Thomas Bartholin, *De nivis usu*, p. 44, quoting the Danish explorer Jens Munk, *Navigatio Septentrionalis* (1624).]

†@ In our Hydrostatical Paradoxes. [*Hydrostatical Paradoxes* was not printed until 1666: see below, vol. 5.]

†@ Ex nive copiosa glaciata compacta. [Quoting Thomas Bartholin, *De nivis usu*, p. 44.]

†@ Pag. 14. [James, *Voyage*.]

†@ Mr. Hudsons Voyage for the discovery of the North-west passage, written partly by Mr. Abacuck Pricket. [Abacuck Pricket, *A Larger Discourse of the ... Voyage of Henry Hudson*, in Purchas, *Pilgrimes*, pt. 2, bk. 3.]

†@ In the Sect. about the Temperature of the Air [Title XVIII of the present work, pp. 381ff.].

†@ New Exp. Physico-mech. Exper. 6. [See above, *Spring of the Air* (1660), vol. 1, pp. 176-80.]

†@ See the forecited place. [There Boyle cites the *Cogitata physico-mathematica* (1644) by the French philosopher and Minim friar Marin Mersenne (1588-1648).]

†@ The Appendix to the Physico-mechanical Experiments. [Apparently a reference to the 'Appendix' dealing with Hobbes's doctrine of fluidity and firmness, or else to *An Examen* (1662), of which the appendix forms part: see above, vol. 3, pp. 109ff., 166ff.]

†@ Decemb. the 13.



†@ Decemb. 13.

†@ Voyage de Moscovie & de Perse, Liv. V. [Adam Olearius, *Relation du voyage de Moscovie, Tartarie, et de Perse* (1656).]

†@ Pag. 63. [James, *Voyage*.]

†@ Pag. 86.

†@ The breadth was, I know not how, omitted in the note, but as I remember, it was about an 8. part of an Inch.

†@ Olaus Ma. Gent. Septentr. Hist. Lib. 1. Cap. 14. [Magnus, *Historia*. 'Early and mid-winter ice is so strong and rigid that a thickness or breadth of two fingers will support a man on foot; three fingers, an armed man on horseback; one and a half hands, whole regiments or companies of soldiers; and three or four hands, an entire legion, or thousands of people, as will be recorded below with reference to wars fought in Winter.' ]

†@ Purchas. Lib. 4. Cap. 13. [Purchas, *Pilgrimes*.]

†@ Purchas. lib. 4. cap. 13. pag. 813.

†@ Neither hereafter will I marvel, though the strait of *Weigats* be stopped up to the Northeast, with such huge mountains of Ice, since the Rivers *Oby* and *Jenesce*, and very many more, whose names are not yet known, pour out such a quantity thereof, that in a manner it is incredible: For it cometh to pass in the beginning of the Spring, that in places near unto the Sea, the Ice through the excessive thickness, and multitude thereof, doth carry down wood before it. And without doubt this is the cause, that about the shore of the strait of *Weigates*, so great abundance of floating wood is everywhere seen: and whereas in that strait near unto *Nova Zembla*, it is so extreme Cold, it is no marvel, if in regard of the narrowness of the strait, so huge heaps of Ice are gathered and frozen together, that in the end they grow to sixty, or at least to fifty fathoms thickness: Says the Description of the Countreys of *Siberia*, *Samojeda*, &c. extant in Purchas's third part of his Pilgrim. Lib.3. Cap.7. ['A Description of the Countries of Siberia, Samoieda, and Tingoesia', in Purchas, *Pilgrimes*, pt. 2, bk. 3.]

†@ Pag. 14. [James, *Voyage*.]

†@ Pag. 106.

†@ Purchas. lib. 4. cap. 18. pag. 837. [The figure involved is William Baffin (d. 1622), navigator and discoverer.]

†@ Pag. 17. [James, *Voyage*.]

†@ Hydrographie du P.G. Fournier, liv. 9. cap. 29. compar'd with the 22. chap. of the same Book. [See above, p. 251n.]

†@ Olai Mag. lib. 3. cap. 2. pag. 334. [Magnus, *Historia*. 'Now the Swedes and the Goths fight with just as much martial vigour on the bare ice as they do on the firmest land. Indeed, as has already been said, where in summer-time the fiercest naval encounters were once fought out, there, when the ice has frozen solid, the lines are drawn up, the ordnance set out, and bloody battles waged: so solid is the ice in supporting troops of cavalry, ranked together in close formation.' ]

†@ -- Sæpe aliàs & his annis fatalibus tam profundè congelavit (marina Aqua) ut non tantùm plaustra, sed integrum exercitum ad aliquot Milliaria Germanica secure vexerit, &c. Inquit T. Barthol. De nivis usu, pag. 43. [Thomas Bartholin, *De nivis usu*: 'Frequently, both in the past and in these wretched years, it (the sea's water) has frozen so deep as to support in safety not just wagons but a whole army for several German miles (etc.)'. In January 1658 Charles X Gustav took his army across the ice from Jutland to Fyen and on to Sjælland. Boyle's statement that this event happened 'within these three years' indicates that this part of the work was written no later than 1661.]

†@ Barthol. de nivis usu, cap.6. [Page 44. 'It occasionally happens, as Constantine Manasses in his *Annales* says happened in the reign of Theophilus, that a bitter winter turns the sea to ice to a truly great depth, and makes that watery element lose all trace of fluidity and adopt the hardness of stone.']

†@ Glycas apud Fournier, liv. 9. cap. 19. [Fournier, *Hydrographie*, quoting the 12th century Byzantine historian Michael Glycas' *Annales ... qui lectori praeter alia cognitu iucunda & utilia, Byzantinam historiam universam exhibent* (1572).]

†@ In the Evening we were inclosed amongst great pieces (of Ice) as high as our Poop, and some of the sharp blew corners of them did reach quite under us. Capt. Jam. pag. 6. [James, *Voyage*.]

†@ Olaus lib. 1. cap. 14. [Magnus, *Historia*: 'But when the ice starts to melt, at the beginning of April, no-one trusts himself on foot to the ice's thickness, still less to its strength, save at dawn: for it is made so fragile by the Sun's rays that what a short time before could bear armed horsemen can now scarcely support an unarmed man.']

†@ Olaus Magnus lib. II. & Blefkenius in Purch. lib. 3. cap. 22. [Magnus, *Historia*; and Dithmar Blefkens' account of Iceland and Greenland in Purchas, *Pilgrimes*, pt. 2, bk. 3.]

†@ I have seen also the sides lin'd with reeds longways, instead of boarding of steening.

†@ Pag. 101. [James, *Voyage*.]

†@ Pag. 65.

†@ In the Sceptical Chymist. [*The Sceptical Chymist* (1661): see above, vol. 2.]

†@ The Dialogues about heat and flame. [See above, General Introduction, p. xxxiii, and below, vol. 13.]

†@ Monsieur Bernard de Palissey au Traitté du Sel commun. [Bernard Palissy, *Discours Admirables* (1580), citing a passage from the section on salt, which might have been issued as a separate pamphlet although no surviving copies are known. This note was mistakenly omitted in the 2nd edition. See *The Admirable Discourses of Bernard Palissy*, trans. A. LaRoque (Urbana, Illinois, 1957), p. 141. Palissy (d. 1589) was a French painter, glass-blower and land-surveyor who made his fortune through discovering how to make enamel. He inaugurated a series of public lectures in Paris to expound his theories on natural philosophy, based on his experiences rather than on Aristotle, whom he claimed never to have read.]

†@ De Claves au second Livre das pierres & pierreries, Cap. 2. [Étienne de Clave, *Paradoxes ou Traitez philosophiques des pierres et pierreries contre l'opinion vulgaire* (1635).]

†@ Ibid.

†@ Monsieur de Claves livre. II. cap. 2.

†@ In the Discourses about *Antiperistasis* [see below, pp. 480ff.], the following passages are taken, out of a small narrative, consisting of about two sheets of paper of *Joh. Baptista Morinus*, published in the year 1619, and titled, *Relatio de locis Subterraneis*, annexed to a discourse (too much built on Astrological and Aristotelian grounds) of the threefold Region, that he conceives to be as well in the Earth as in the Air. [*Nova mundi sublunaris anatomia* (1619) by the French physician, mineralogist and astrologer Jean-Baptiste Morin (1583-1656).]

†@ Unde calor ille procederet petii à præfecto. Respondit, ex partibus inferioribus, inferius enim perpetuo calet. Quod responsum magis adhuc miratus, quæsi an res ita sese haberet in fodinis omnibus. Respondit ita se haberet in omnibus, saltem profundis, ut post profundum Terræ frigidaë tractum, in locum calidum descendatur. Et quod, ubicunque terra foditur post similem profunditatem, nullum amplius sentitur frigus, sed semper calor, quantumcunque profundè fodiatur. ['I asked the overseer whence the heat came. 'From the lower regions,' he replied: 'it is always hot further down.' This reply made me wonder greatly, and I asked whether this was the situation in all Mines. He answered that in all mines, at least in the deep ones, after a deep section of cold Earth one comes to a hot region, and that wherever the earth is excavated below this same depth, cold is no longer felt, but only heat, however deep one digs.']

†@ -- Percunctatus sum an quo magis acceditur ad terræ centrum, calor ille major perciperetur. Respondit, id nunquam fuisse animadversum, nisi interdum dum fodiendo occurebant venæ calidiorum Mineralium. -- Hæc Responsa non in unicâ fodinâ, & ab unico præfecto accepi; sed, &c. ['I enquired whether this heat felt greater, the closer one got to the centre of the Earth. He replied that he had never noticed, save from time to time when they came across veins of hot minerals. These answers I obtained in more than one mine, and from more than one overseer.' ]

†@ Cum descendendo calorem illum magis ac magis augeri sentirem: hujus rationem petii à præfecto, quod in nullâ adhuc fodina similem caloris intensionem percipissem. Respondit, Mineram Vitrioli paulo inferius existere, quæ calorem multiplicaret. ['As I went down I felt this heat grow more and more, and asked the overseer the reason why, since I had not noticed a similar intensity of heat in any mine hitherto. He replied that there was a deposit of Vitriol a little lower down, which intensified the heat.']

†@ It was not the Sea, nor the nearness unto the Pole, but the Ice about the land, that let and hindred us (as I said before) for that as soon as we made from the land, and put more into the Sea, although it was much further northward, presently we felt more warmth, and in the opinion our Pilot *William Barents* dyed, who notwithstanding the fearful and intollerable Cold that he indur'd, yet he was not discourag'd, but offer'd to lay wagers with divers of us, that by Gods help he would bring that pretended Voyage to an end, if he held his course Northeast from the North Cape. *Gerat de Veer in Purchas*, pag. 474.

†@ *Purchas. lib. 3. cap. 15. pag. 579.* [See above, p. 323.]

†@ Beguinus in Tyrocinio Chymico, lib. 2. cap. 1. [Jean Beguin, *Tyrocinium chymicum* (1610): see above, p. 158.]

†@ Josephus Acosta lib. 2. cap. 11. [Joseph de Acosta, *The Natural and Moral History of the East and West Indies* (1604); first published in Spanish (1590), then in French (1598). Although this work was excerpted in Purchas, *Pilgrimes*, pt. 2, bk. 5, Boyle appears to be citing the 1604 edition.]

†@ Gassendi Phy. Lib. 6. Sect. 1. pag. 399. De qualitatibus rerum -- ac addi quidem fortassis potest, præcipua frigoris semina, si quæ constant, potissimum ex frigorificis Atomis abire in halinitrum

ipsam congelet & universa à se contacta refrigeret, & abeundo in halitum creet gelidum seu frigidum ventum, sed res pendet ex variis, quæ non possunt hoc loco commemorari, Experimentis. [Gassendi, 'Physica' in *Opera omnia* (1658). 'On the Qualities of Things [heading]. ... And perhaps it may be added that the principal seeds of cold, as it were, pass in the main from the cold-making atoms into Salt-petre and other substances like themselves, when we know the salt-petre to be unresolved, even though, when penetrating water, it freezes it and cools down everything it touches, and vanishing in a vapour it makes a cold or icy breeze. But the matter depends on divers experiments which cannot be recorded here.' ]

<sup>†</sup>@ Ib[id]. pag. 400. Quomodo possunt corpuscula Nitri in aquam infusi illam præter modum adeo frigidam reddere imò, & per æstatem etiam congelare, dum nitrum nivi glacieve detritæ commistum lagenæ circumponitur, ipsaque præter corpus Lagenæ penetrant in aquam contentam. ['How the corpuscles of Nitre infused in water can make it so much colder than normal, and indeed freeze it in summer-time, if nitre mixed with snow or crushed ice is placed around a flask, and they [the corpuscles] pass into the water held within the body of the flask.' ]

<sup>†</sup>@ Aug. 1. [This note was mistakenly omitted in the 2nd edition.]

<sup>†</sup>@ In the third Preliminary Discourse [of the present work, pp. 253ff.].

<sup>†</sup>@ Purchase lib.4. pag. 542. [Purchas, *Pilgrimes*.]

<sup>†</sup>@ Purchase pag. 811.

<sup>†</sup>@ These voyages are extant in Purchase lib. 1. cap.13. and this passage is in pag. 560.

<sup>†</sup>@ Purchase. pag. 564.

<sup>†</sup>@ Purchase pag. 415. [Giles Fletcher (1549?-1611), LLD, acted as special ambassador to Russia in 1588-9. His *Of the Russe Common Wealth; or, Manner of Government by the Russe Emperour* (1591) is abridged in Purchas, *Pilgrimes*, pt. 2, bk. 3. Contrary to what Boyle says, this is the first reference to Fletcher in this work, although he is mentioned several times subsequently.]

<sup>†</sup>@ Purchase pag. 414. [Quoting Fletcher again.]

<sup>†</sup>@ Pag. 62. [James, *Voyage*. The Swedish ambassador has not been identified]

<sup>†</sup>@ Pag. 61. [James, *Voyage*.]

<sup>†</sup>@ Pag. 46.

<sup>†</sup>@ Pag. 64.

<sup>†</sup>@ Pag. 66.

<sup>†</sup>@ Pag. 69.

<sup>†</sup>@ A Sceptical Disquisition of Antiperistasis. [See below, p. 464.]



<sup>†</sup>@ Pag. 702. [Jonas Poole, *A Voyage set forth by ... Sir T. Smith and the rest of the Muscovie Company to Cherry-Island*, in Purchas, *Pilgrimes*, pt. 2, bk. 4.]

<sup>†</sup>@ Pag. 473, 474. [de Veer, *Three Voyages*, in Purchas, *Pilgrimes*.]

<sup>†</sup>@ Acosta lib. 2. cap. 9. pag. 101. [Acosta, *History* (above, p. 374.)]

<sup>†</sup>@ Pag. 109.

<sup>†</sup>@ Purchase. pag. 578. [Purchas, *Pilgrimes*.]

<sup>†</sup>@ See James voyage, pag. 61. & 81. and elsewhere.

<sup>†</sup>@ Purchase pag. 569.

<sup>†</sup>@ Josephus Acosta lib. 2. pag. 111, 112. [Acosta, *History* (above, p. 374).]

<sup>†</sup>@ Purchase pag. 571.

<sup>†</sup>@ Pag. 414. [See above, p. 387n.]

<sup>†</sup>@ Purchase. pag. 415.

<sup>†</sup>@ Voyage de Moscovie & de Perse, livre 3. p. m. 117, 118, 119. ['Pages 117-9 in my copy' of Olearius, *Relation du voyage*. The figure referred to in the next sentence has not been identified.]

<sup>†</sup>@ Pag. 119.

<sup>†</sup>@ Pequin.

<sup>†</sup>@ Lib. 1. cap. 6. [*De medicina Ægyptiorum libri quatuor* (1591) by the Italian physician, Prosper Alpinus (1553-1617).]

<sup>†</sup>@ Pag. 81. [James, *Voyage*.]

<sup>†</sup>@ Pag. 9. [Alpinus, *De medicina Ægyptiorum*. 'In winter, night air is found to be quite cold: soon after sunrise it warms a little, more so at midday; but when night falls it becomes cold again. Thus the air may truly be called inconsistent, and it is in this very inconsistency that the many illnesses abroad in the city at that time have their origin.' ]

<sup>†</sup>@ The weather was snowy and foggy, freezing our rigging, and making every thing so slippery, that a man can scarce stand. And all this with the wind Southerly, says Captain James (page 104.) in his Journal 26. of August. [James, *Voyage*.]

<sup>†</sup>@ Ab his ventis aerem alteratum, esse causam cur pestis illa dissolvatur, multi illorum Affirmant. Quod etiam non videtur penitus à veritate alienum, quando id multis etiam rationibus nobis persuaderi possit, in primisque, &c. Prosp. Alpin. lib. 1. De medicina Ægypt. cap. 18. [Alpinus, *De medicina Ægyptiorum*. 'Many of them affirm that the change effected in the air by these winds is the reason why the plague ceases. This does not seem wholly divorced from the truth, it being possible to adduce

many reasons in its support, especially ... (etc.).' Boyle discusses this phenomenon twice elsewhere: in *Usefulness*, Part 1 (1663), above, vol. 3, p. 450; and in Title 40 in *General History of the Air* (1692), below, vol. 12.]

<sup>†</sup>@ Ibid. lib. 1. cap. 6.

<sup>†</sup>@ Ibid. lib. 1. cap. 7. pag. 11. ['A most hot air holds sway by reason (as is said) of the sky itself, because this city is only six degrees distant from the Tropic of Cancer. Owing to this short distance, when the sun reaches that tropic and is closer to their zenith, the air becomes powerfully hot, and unless the Aetesian winds then blow from the north, a most vehement scorching heat is felt which can scarce be supported by our people. When they [the winds] come, our foreigners take refuge underground, where they remain until the winds' ferocity has died down and ceased. Almighty God has joined these inconveniences with other good things, for when these very hot winds have abated, there at once begin to blow others, from the North, offering immediate solace to tired and scorched bodies. For if the first set of winds persisted very long, no-one could live in that region.']

<sup>†</sup>@ Josephus Acosta, lib. 3. cap. 9. [Acosta, *History* (above, p. 374).]

<sup>†</sup>@ Lib. 3. cap. 2. p. 120.

<sup>†</sup>@ Lib. 3. cap. 3.

<sup>†</sup>@ Part 1. cap. 2. [William Wood, *New Englands Prospect* (1634).]

<sup>†</sup>@ Captain James's voyage, pag. 52, 53.

<sup>†</sup>@ Purchase lib. 3. cap. 5. Sect. 2. pag. 495.

<sup>†</sup>@ Captain James 65. [James, *Voyage*.]

<sup>†</sup>@ Purchase lib. 1. cap. 4. pag. 74. [Marco Polo's voyages are extracted in Purchas, *Pilgrimes*, pt. 2, bk. 1.]

<sup>†</sup>@ Olear. lib. 3. p. m. 117 ['Page 117 in my copy' of Olearius, *Relation du voyage*. For a similar report from Samuel Collins, see above, p. 387.]

<sup>†</sup>@ Purchase pag. 491. [de Veer, *Three Voyages*, in Purchas, *Pilgrimes*.]

<sup>†</sup>@ Purchase pag. 491. [Boyle's reference earlier in the paragraph is to the *Commentary on Scipio's Dream* by the neo-Platonist Ambrosius Theodosius Macrobius (fl. 400)]

<sup>†</sup>@ Purchas. pag. 491.

<sup>†</sup>@ Captain James's voyage, 64.

<sup>†</sup>@ Livre 3. p. m. 117. ['Page 117 in my copy' of Olearius *Relation du voyage*]

<sup>†</sup>@ Livre 3. 116.

<sup>†</sup>@ Purchas. lib. 3. pag. 415.

<sup>†</sup>@ Purchas. pag. 497. [De Veer, *Three Voyages*, in Purchas, *Pilgrimes*, pt. 3.]

†@ Pag. 64. [James, *Voyage*.]

†@ Pag. 497. [Purchas, *Pilgrimes*.]

†@ Pag. 56. [James, *Voyage*.]

†@ Pag. 52.

†@ Purchas. lib. 3. cap. 5. pag. 496

†@ Pag. 65.

†@ Purchas. pag. 497.

†@ And 'tis from very Northern Countries, that we usually receive very dark colour'd Furs, and the skins as well of black Foxes as of white ones.

†@ Lepores coloris & pellis mutatione anni tempestates sequuntur, ac hiberno tempore albis pilis vestiti, æstivis mensibus eosdem cinereos habent. Livoniæ nova descriptio, Pag. 303. ['Hares mark the seasons with a change of colour and pelt: in winter they have coats of white hair, but in the summer months they have ashen ones.' Probably from *Regnorum Sueciae, Gothiae, Magnique Ducatus Finlandiae, ut et Livoniae ... descriptio nova* (1656), by Martin Zeiller (1589-1661).]

†@ Pag. 46. & Pag. 89. [James, *Voyage*.]

†@ Num. 35. [J.B. van Helmont, 'Gas aquæ', in his *Ortus medicinae* (1648). 'Fill a large glass flask with pieces of ice, and fully close the neck with a Hermetic seal, i.e. by melting the glass there. Then place the flask on some scales, with a weight set on the other side, and you will see that after the melting of the ice the water is nearly one eighth part heavier than the ice was. This can be done a thousand times with the same water...(etc.)' ]

†@ -- Hinc gelidam congelatamque aquam graviores esse non congelata expertus est Jo. Manelphus, Com. in 4. Meteor. Aristot. Inquit Tho. Bartholin de Nivis usu cap. 12. [Thomas Bartholin, *De nivis usu*, p. 82: 'From which Jo. Menelphus discovered that water is heavier cold and frozen than unfrozen.' Joannes Manelphus was the first professor of philosophy at La Sapienza in Rome, and wrote a number of commentaries on Aristotle. However, that on *Meteorologica* appears lost.]

†@ If it had not been for the negligence or mistake of one, that I ordered in my absence to freez and thaw the same water, divers times one after another, I might have added the success of that Experiment, which I was sorry to miss of, because it might possibly have afforded an useful hint about a way to correct stinking water in some Climates or seasons.

†@ Olearius, livre 3. pag. 168. [*Relation du voyage*. The other figures referred to in this paragraph have not been identified.]

†@ In the Section touching the duration of Ice. [See section XVI in this work, above, pp. 357ff.]

†@ Of the imperfection of Physicks. Maistre Bernard Palissy. [Boyle here evidently refers to one of the components of his *Usefulness of Natural Philosophy*: see below, vols. 6, 13. On Palissy, see above, p. 365. The passage Boyle cites on the mountains of the Ardennes is almost certainly from the *Discours Admirables*, but we have been unable to locate it. Boyle calls Palissy 'illiterate'

because he knew neither Greek nor Latin.

†@ Lib. 1. pag. mihi 23. [Magnus, *Historia*: 'page 23 in my copy'. 'There are also iron mattocks, made in a certain way, that with great ease break thick, hardened ice such as does not give way to other iron implements. Other hatchets, though mixed with steel, break in extreme cold like glass at a mere blow against the ice, or a sturdy tree: but the aforesaid mattocks or iron spears remain most strong.']

†@ As it afterwards did at the least.

†@ Tam cito illa congelabat, ut eximerem ex eo crustam unam aut alteram antequam non calefacta vel levissime concrevisset. pag. 572. ['It froze so rapidly that I lifted one or two layers of ice off it before the unheated water froze even lightly.' Bérigard, *Circulus pisanus*. On Cabeo, see above, p. 383.]

†@ Quare ferventem aquam adhibuisse oportet qui asserit eam esse minus gelabilem, præcipuè salsam. pag. 571. ['Therefore he must have used boiling water, who asserts that it is less freezable, especially salted.' Bérigard, *Circulus pisanus*. In the 1st edition, this and the previous author's note are accidentally reversed.]

†@ Here the Postscript ends.

†@ Purchas lib. 4. cap. 19. pag. 844. [Purchas, *Pilgrimes*.]

†@ Lib. 3. cap. 22.

†@ Volentes igitur piscari sub glacie duo magna foramina latitudine 8. vel 10. pedum, centum & quinquaginta vel 200. passibus à se invicem directa distantia, aperiunt, inter quæ 30. vel 40. minora foramina, latitudine unius pedis & semis, ab utroque latere distantia 30. pedum intermedia constituunt, tum per ea, &c. Olai. Mag. lib.20. [Magnus, *Historia*. 'Those who intend to fish under the ice cut out two large holes, 8 or 10 feet across, and 150 or 200 feet apart, and make 30 or 40 smaller holes in between them, a foot and a half across, at an average of 30 feet from each end, etc.']

†@ Olaus Mag. Titulo, De cursu glaciali, pro piscibus. Quæ (*Anguillæ*) si totaliter glacie constrictæ fuerint simul omnes respiraculum ab aere non habentes pariter suffocatæ moriuntur. [Ibid.: 'If they (i.e. eels) were completely locked in by the ice, without an air hole, they would at once all die of suffocation.']

†@ Præmittendum est quod generaliter omnes lacus, & stagnales Aquæ in mense Octobri incipiunt congelari, glaciesque aucto frigore in plerisque locis tantum condensari, ut ubi venæ lacus & stagna viventis aquæ non intrant, pisces suffocati tempore resolutionis glaciei inspiciantur, verum ne hæc suffocatio tam dispendiosa fiat, diligentîâ piscatorum continue glacies ipsa perfringitur ne congeletur. Olai. Magni. lib. 1. Titulo de transitu glaciali, &c. [Ibid. 'It must be stated at the outset that all lakes and pools of water, in general, start to freeze in October, and that when it gets colder the ice is in many places so tightly packed that, where no currents of living water enter the lake or pool, the fish are found suffocated when the ice thaws. So that this suffocation does not turn out too costly, however, the fishermen carefully and continually break the ice, lest it freeze over.' ]

†@ Pag. 82.

†@ Pag. 47.



†@ Purchas lib. 3. cap. 5. pag. 487 [de Veer, *Three Voyages*, in Purchas, *Pilgrimes*.]

†@ Purchas lib. 3. cap. 1. pag. 415. [See p. 387n.]

†@ Of the usefulness of Experimental Philosophy. [*Usefulness*, Part 1 (1663): above, vol. 3, p. 265]

†@ Of the Unsuccessfulness of Experiments. [This formed part of *Certain Physiological Essays* (1661): see above, vol. 2, pp. 35ff.]

†@ Rem vero adeo obscuram exemplis similibus illustrabo Brassicæ: aqua congelata Brassicam representat, spiritibus vegetalibus à frigore concentratis. Tho. Bartholin de usu Nivis, pag. 17. ['But I shall illumine such an obscure subject with the similar examples of the cabbage: the water when frozen provides a representation of the cabbage, since its vegetable spirits are concentrated by the cold.' Thomas Bartholin, *De nivis usu*. Contrary to what Boyle says, there is no such passage on p. 17.]

†@ Pag. 573. [Bèrigard, *Circulus pisanus*. Few have noted why manifold images of plants sometimes are seen within the ice in glass vials full of water frozen right up to the surface. This occurs when the salt of various plants have been put into vials, for when there will be the salt of any plant and of wormwood, the ice in its lixivium clinging to the glass displays jagged leaves of that plant [wormwood]; similarly, in another vial, the leaves of the plant whose salt had been added to its own water will be seen. And lest anyone should attribute this to chance, the same images always occur in water quite often melted and refrozen, so that therefrom you would say that the multiple spirit of salt is to be placed among natural principles.]

†@ In the unpublished Section of the usefulness of Experimental Philosophy. [The second volume of *Usefulness* was published in 1671: see below, vol. 6.]

†@ Of the unsuccessfulness of Experiments. [I.e. part of *Certain Physiological Essays* (1661): see above, vol. 2, pp. 35ff. For the previous reference to this essay, see above, p. 448.]

†@ Where a wonderfully piercing, though not sensibly violent cold, does sometimes suddenly kill men, and yet preserve their Bodies untainted whole years together. [Acosta, *History* (above, p. 374). Boyle had discussed the atmospheric conditions in this and other mountainous areas in *Spring of the Air* (1660), above, vol. 1, pp. 285-6.]

†@ Pag. 36. [James, *Voyage*.]

†@ See the History of fluidity, Sect. 19. [A reference to 'The History of Fluidity and Firmnesse', in *Certain Physiological Essays*, above, vol. 2, pp. 135-6.]

†@ -- Cœpimus in hanc fodinam per gradus valde strictos profunde admodum descendere, per regionem certè frigidissimam, quam solis vestibus metallicis opertus, multo frigidior sensi, &c. p. m. 130. ['Page 130 in my copy'. From what follows it is clear that Boyle is quoting from Jean-Baptiste Morin, *Nova mundi sublunaris anatomia* (1619), the work cited on p. 367. 'In this mine we began a fairly deep descent, in very brief stages, through an exceedingly cold region, which I, dressed only in miners' clothes, found much colder (etc.).']

†@ -- Inquam descendi mense Julio, quo anni tempestas vigeat calidissima, siccissimaque. p. m. 130. ['I went down in July, when the weather was the hottest and driest of the year. Page 130 in my copy.']

†@ Pag. 142, & 143.

†@ Pag. 125.

†@ Exhalatio aere levior per ipsum puteum ascendit magno impetu, in ejus summitate adhuc sensibiliter calida ipsa æstate, licet supremam terræ regionem tunc frigidissimam permeat. Pag. 128. see also pag. 125. ['The smoke, being lighter than air, goes up the shaft with great speed, and is still sensibly hot when it reaches the top, even in summer, though it passes through the exceedingly cold upper region of the Earth.']

†@ In Polonia vero montes profundissimi salis sunt, præsertim in Vielisca & Bochna ubi videndi causâ transcensis scalis, vidi in profundioribus locis laboratores nudes ob calorem, ferreis instrumentis eruere opulentissimum Thesaurum salis, veluti Aurum & Argentum ex Mineris inexhaustis. Olaus Mag. lib. 13. p. 382. [Magnus, *Historia*: 'But in Poland there are very deep salt mountains,

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especially in Vielisca and Bochna, where I went down the ladders in order to take a look, and saw workmen in the deeper parts, naked because of the heat, hewing out a magnificent stock of salt, like gold or silver, from undiminished deposits.']

†@ Pag. 63.

†@ Pag. 58.

†@ Pag. 59.

†@ Hancque naturam, lacum similem, prope Metropolin Nidrosiensem Regni Norvegiæ, habere compertum est, eo præcipue Argumento, quod in mediis frigoribus nunquam congelatur. Lib. 2. [Magnus, *Historia*: 'And this nature is certainly possessed by a similar lake near Trondheim, a great city of the kingdom of Norway, particularly in the respect that it never freezes, even in the midst of winter.']

†@ Joseph. Acost. Hist. Ind. pag. 174. [Acosta, *History* (above, p. 374).]

†@ P.m. 136. ['Page 136 in my copy' of Morin, *Nova mundi* (above, p. 367.)]

†@ Quia verò in Comitatum Zoliensem, dum aquas persequimur, ventum est, non possum præterire hiatum terræ iisdem in locis famosum ob pestilentes expirationes, quibus Aves supervolantes, & quævis alia animantia extinguere constat, manifesto eorum experimento, qui, &c. Pag. 74. [A reference to *De admirandis Hungariæ aquis* (1551) by Dr Georg Werner. 'Now since in our quest for waters we have come to the County of Zol [?], I cannot leave without a mention of the gap in the Earth there, famous for its pestilential exhalations, which kill birds and any other living things that pass over it. This is established by the purposeful experiment of those who ... (etc.)' ]

†@ Ibidem est sub dio fons calidarum cæteris amplior, quem Purgatorium vocavere, ea nimirum ratione, quod, quemadmodum proditum est in purgatorio pœnas nocentium pro noxarum modo, alias acerbiores alias mitiores, ita quædam insunt Aquæ hoc in fonte discrimina, nam quâ in eum à Danubii ripâ aditus est, subfrigida primum, mox tepida, & quo in eum penetraris altius hoc magis calet. In recessu vero interiore tam est calida, ut ferri non possit. Est etiam is calor haud dubie aquæ hujus proprius; nam alia, quæ dixi, temperamenta verisimile est à Danubio accedere, qui crepidinem hujus fontis lambit, & cum vel modicè excrescit, totum inundat, neque tamen ita restringit, quin

caleat. Quin intra ipsam ripam, qua Danubio perennis cursus est, calidæ ebulliunt, ubi qui altius mergi volunt lavare consueverunt. Pag. 57. 'In the same place there is an open-air hot water spring larger than the rest, which they call "Purgatory", for the following reason. Just as in Purgatory they say the guilty are punished more harshly or more leniently according to their crimes, so there are different sorts of water in this spring: for at the part closest to the bank of the Danube, it is at first rather cold, then warm, and then, where one gets to the deepest part, it produces a great heat. Indeed, in its inner depths it is too hot to bear. This heat, furthermore, is undoubtedly a property of the spring water itself: for the water of other degrees of warmth I have mentioned is very likely to derive from the Danube, which laps at the edge of this spring, and indeed completely floods it when its level rises the slightest amount, though without quenching its heat. In fact even within the very banks which mark the Danube's regular course, there boil up hot springs, where those who wish to go in quite deep have been accustomed to wash.']

†@ Neque in ripâ tantum eruuntur Calidæ, sed etiam intra amnem, si fundum ejus pedibus suffodias. Calet autem immodicè, nec sunt Idoneæ balneis, nisi temperentur, quod Admixture frigidæ de proximo haustæ in proclivi est. Pag. 65. 'Nor do hot waters bubble up only at the river's edge, but even in the middle of the current, if you burrow into the bottom with your feet. But it is exceedingly hot, and not ideal for bathing, unless it be tempered by an admixture of cold water drawn from nearby.'

†@ Nec prætereundum hic puto Lacum esse LX. milliarum in longitudine, & XX. in latitudine Italicorum, *Veter* appellatum in Regno Ostrogothorum, quæ talis est naturæ, quod cum tempestuoso vento congelatus fuerit, & tempus resolutionis immineat, vehementissimo strepitu incipit fundo ebullire & commoveri, magna violentiâ perumpere in parvas rimas, vel scissuras, quæ fiunt in glacie, & has in modico temporis spatio faciens valdè latas, licet pro tunc glacies in spissitudine habuerit, plus quam unum, vel duo brachia. Lib. primo, pag. 23. [Magnus, *Historia*. 'One must not, I consider, omit mention of a lake in the kingdom of the Ostrogoths, sixty (Italian) miles long, twenty wide, and called Lake Veter. Its nature is such that when it has been frozen by the cold winds, and the thaw is imminent, it starts to bubble up and shake, with a ferocious cracking sound, and split with great violence along little rifts or fissures that appear in the ice, tearing them wide open in a moment, though the ice may then still be more than one or two cubits thick.' ]

†@ Dr. Sam. Collins. [see above, p. 64n.]

†@ See the second Preliminary discourse, that accompanies the History of Cold [above, pp. 240ff.].

†@ My backwardness to admit a *fuga Contrarii*, may be somewhat confirmed by what I lately learned from the English Extraordinary Ambassador (the Earl of *Carlisle*) into *Russia*, newly returned thence. For meeting the other day with an opportunity of asking his Lordship a few Questions (which he was pleased to answer with his wonted civility) about the Cold in *Muscovy*: I was informed by one of his answers, That his Excellency had there the curiosity to observe some Bottles of choice and strong Wine, that were vehemently frozen, and the opportunity to take notice, that the liquor was quite congeal'd throughout, and turned into solid ice, whence he rationally inferred, that the spirituous parts of the Wine did not in these Bottles (for ought he acknowledg'd, that in greater vessels, that may sometimes hold true, which is said of the production of spirit of Wine by congelation) retire to the Center, and remain there unfrozen; and his Lordship ingeniously persued the Experiment, and confirmed the conjecture, by causing the ice taken out of the broken bottles to be thawed by degrees into several vessels, by which means he found, that the liquor afforded by the exterior parts of the resolved ice, was very little, if at all less strong, then that which was obtained from the internal parts of the same ice; from which Observation *Carneades* would argue, That at least 'tis not universall, but in particular cases, and therefore probably by accident, or upon particular accounts, that the Concentration of the spirits of Liquors is consequent upon being exposed to Cold. [Boyle speaks of Charles Howard, 1st Earl of Carlisle (1629-85), ambassador

extraordinary to Russia, Sweden, and Denmark in 1663-4. Howard was married to his cousin Anne, daughter of Edward, Lord Howard of Escrick, who had at one time been the intended wife for Boyle himself. See R.E.W. Maddison, *The Life of the Hon. Robert Boyle* (London, 1969), p. 55. This note must have been added at the last minute, since Howard returned to Russia on 30 January 1665. See Loewenson, 'The Works of Boyle and "The Present State of Russia"' (above, p. 64n.), p. 481 note 47.]

†@ I presume he means *Cornelius Drebell*. [Maignan, *Cursus philosophicus*, citing Cornelius Drebbel, probably his *Tractatus de natura elementorum* (1628), which discusses atmospheric pressure in connection with a toy 'perpetuum mobile.' This note was mistakenly omitted in the 2nd edition. 'Using a very accurate thermometer, completely unadjusted from winter to the following summer, and (in accord with Drebel's precepts) filled with water in such a way that it cannot escape or decrease, I have found in the aforesaid excellent wine cellars that the greatest cold there was in a very hot summer did not equal that which there was in winter, as stated in the above experiment: for in the glass thermometer tube, which was four hands long and divided into eight degrees and parts of degrees, the water climbed to seven and a half degrees in winter, but in summer scarcely went over six degrees, even though the cold that summer felt much greater.']

†@ Artic[le]. 6. [Paragraphs 3 to 7 are all taken from Hobbes' *Elements of Philosophy, concerning Body* (1656).]

†@ Artic. 7. [On the absence of the figure, see above, p. 501.]

†@ Quoted by Paul. Neworantz. De Purpurâ, Cap. 12. [The identity of this work, citing Avicenna, is obscure.]

†@ Doctor S. Ward (now the worthy Bishop of Exeter) and Dr. J. Wallis (the learned Savilian Professor of Geometry.) [Seth Ward (1617-89), DD, held academic posts at Cambridge and Oxford until 1660, when he became Bishop successively of Exeter and Salisbury. His book, *In Thomae Hobbii philosophiam exercitatio epistolica* (1656), was a wide-ranging critique of Hobbes. John Wallis (1616-1703), DD, Savilian professor at Oxford from 1649 until his death, feuded with Hobbes for more than twenty years until the latter died. See J.F. Scott, *The Mathematical Work of John Wallis* (London, 1938), ch. 10.]

†@ Chap. 28. Sect. 2. at the beginning [i.e., of Hobbes' *Elements of Philosophy, concerning Body* (1656)].

†@ See the VI. Section of the History of Cold [above, pp. 288ff.] .

†@ Euri, Austrique venti à Meridie loca Arenosa summoque calore inflammata transeuntes atque Ægyptum spirantes tantum caloris æstus, pulverumque & inflammatarum Arenarum evehunt ut ignitas fornacis flammæ, nec non pulveribus obscurissimas nubes eo asportasse videatur. *And elsewhere*, -- Prima æstatis parte calidissimâ inæqualissimaque ob vehementissimum Meridionalium Ventorum calorem, &c. *Prosper Alpinus* de Medicina Ægyptiorum. [Prosper Alpinus, *De medicina Ægyptiorum libri quatuor* (1591). 'The east and south winds, travelling from the south over regions which are sandy and inflamed with the greatest heat, and blowing into Egypt, spread abroad so great a rage of heat, dust, and burning sands that it appears to have carried away thence the burning flames of a furnace and clouds very dark with dust... Early summer is hottest and most unforgiving, on account of the ferocious heat of the south winds.'

†@ From 9. till noon, there blows a wind with such extreme heat from the sands, that it swallows up a mans breath, and stiflenth him. -- The King of *Chermain* sent an Army of sixteen hundred horse, and five thousand foot, against the Lord of *Ormus*, for not paying his Tribute, which were all



surprized and stifled with that wind. Marcus Polus in Purchas's Pilgrims, lib. III. p.m. 71. ['Page71 in my copy'. Excerpts from Marco Polo are printed in Purchas, *Pilgrimes*, pt. 2, bk. 1.]

†@ See this difficulty more largely handled in the first Preliminary discourse [to this work: above, pp.229ff.].

†@ In the new Experiments touching the Spring of the Air. [*Spring of the Air* (1660): see above, vol. 1, p. 207.]

†@ See the IX. Title of the History of Cold [above, pp. 308ff.].

†@ See the IV. Section of the History of Cold [above, pp. 278ff.].

†@ See the Examen of Antiperistasis [above, pp. 459ff.].

†@ See the IX. Title of the History of Cold [above, pp. 308ff.].

†@ Pag. 673. [See above, p. 450, where Boyle cites Bèrigard, *Circulus pisanus*.]

†@ According to the goodness of the Lime, of which if it be very strong, two parts may serve the turn, and which, if it be too weak, may make the Experiment miscarry.

†@ To prevent which, I usually cause the Orpiment to be beaten, wrapp'd up in divers papers, or some other way secur'd from Avolation, and from harming the vessel wherein 'tis pounded.

†@ Which for this use 'twill suffice to make by dissolving Gum-Arabick in a little fair water.

†@ This is known in the shops by the name of Red-Lead, and is here specified, as being cheap and easie to be procur'd, though I suppose that other Calxes or powders of the same metal, if they be not sophisticated, may serve the turn.

†@ If you write first with the black Ink, and then with the solution, the Lines must be made somewhat distant, that those which express your secret may have room between the others, and therefore the better to avoid suspicion, I chose rather to write first with the invisible Ink, and then over that with the black; as if I had writ on an ordinary piece of white paper, by which means I could write the black Lines as close as I pleased.

†@ That learned Gentleman Mr. H. Oldenburg, Secretary to the Royal Society. [Oldenburg was the 'publisher' of this work. See above, pp. 205-7 and Introductory Note.]

†@ For those that concern Divinity belong not to this Catalogue.

†@ This is pointed at in the third Page of the following Account, where mention is made of an Honorable Person, &c.

†@ See the Publisher's Advertisement to the Reader [Oldenburg's advertisement at the beginning of the work; see also above, Introductory Note].

†@ Among which I am since informed, that he had tried divers, before he saw my Papers.

†@ So one of the chief Passages of the *Examen* of *Antiperistasis* is much confirmed by the Forty Fourth and Forty Fifth Pages (of the following Papers) which contain an Account of a Trial made

by the command of the *Royal Society*, to whom it was proposed by the Author of the *Examen* [i.e., Boyle] with a request, that they would be pleased to order it to be made. [See below, pp. 538-9: there does not appear to be a report of this experiment in the Society's minutes.]

<sup>†</sup>@ The Art of Pottery. [This work never appeared. Merrett's interest in glassmaking was well known. See above, p. 177n.]

## Editor's Notes

<sup>†</sup>\* *RBHF*, p. 29.

<sup>†</sup>\* Sharrock, *The History of the Propagation and Improvement of Vegetables* (Oxford, 1660), pp. 31, 77; see also above, vol. 2, p. 337, vol. 3, p. 371.

<sup>†</sup>\* Birch, *Royal Society*, i, 323.

<sup>†</sup>\* Fulton, *Bibliography*, p. 43.

<sup>†</sup>\* Boyle, *Experimentorum novorum physico-mechanicorum continuatio secunda* (Oxford, 1680), sig. a1, and below, vol. 9.

<sup>†</sup>\* See below, vol. 9. In the list of Boyle's works in the Latin edition of *Hydrostatical Paradoxes, Colours* was originally given as published in 1666, but this is altered in ink in some copies to 1664. See below, vol. 5, p. xxi. It is perhaps revealing that Boyle's letter to Oldenburg of 29 December 1667 seems to imply that he thought the book had been published in 1661.

<sup>†</sup>\* See below, vol. 11.

<sup>†</sup>\* *Stationers' Company Register*, ii, 335.

<sup>†</sup>\* Birch, *Royal Society*, i, 404.

<sup>†</sup>\* *Term Catalogues*, i, 24.

<sup>†</sup>\* *Correspondence*. See also Oldenburg to Boyle, 25 August, 1 September and Boyle to Oldenburg, 29 August 1664. For the speculation that Oldenburg might have executed the translation, see *Oldenburg*, ii, 210n.

<sup>†</sup>\* Boyle to Oldenburg, c. 16 September 1665; cf. Oldenburg to Boyle, 18 September, 10 October, 21 November 1665; Boyle to Oldenburg, 11, 18 November 1665: *Correspondence*.

<sup>†</sup>\* There may also have been a 24° version of this: see Fulton, *Bibliography*, p. 46.

<sup>†</sup>\* See above, General Introduction, p. lxxviii.

<sup>†</sup>\* See *Correspondence*, s.v. 1664; the letter survives in British Library MS Sloane 548, fol. 21.

<sup>†</sup>\* Joseph Glanvill, *Plus Ultra* (London, 1668), p. 98.

<sup>†</sup>\* A. R. Hall, 'Further Optical Experiments of Isaac Newton', *Annals of Science*, 11 (1955), 27-43, on pp. 27-9; B.J.T. Dobbs, *Foundations of Newton's Alchemy* (Cambridge, 1975), pp. 123-4.

†\* Samuel Pepys, *Diary*, ed. William Matthews and Robert Latham (11 vols., London, 1972-83), viii, 188, 236-7, 247.

†\* See Debus, 'Solution Analysis prior to Robert Boyle', *Chymia*, 8 (1962), 141-61; id., 'Sir Thomas Browne and the Study of Chemical Indicators', *Ambix*, 10 (1962), 29-36; Marie Boas [Hall], *Robert Boyle and 17th-century Chemistry* (Cambridge, 1958), pp. 133ff.; id., 'Introduction' to the Johnson Reprint facsimile of the work (New York, 1964), esp. pp. xviii-xx; and William Eamon, 'New Light on Boyle and the Discovery of Colour Indicators', *Ambix*, 27 (1980), 204-9.

†\* Michael Hunter, 'How Boyle Became a Scientist', *History of Science*, 33 (1995), 59-103, on p. 97 and n. 51, and below, vol. 13.

†\* See above, vol. 2, p. 207, and below, vol. 13.

†\* See L. D. Patterson, 'Thermometers of the Royal Society', *American Journal of Physics*, 19 (1951), 523-55; 'The Royal Society's Standard Thermometer, 1663-1709', *Isis*, 44 (1953), 51-64; W. E. K. Middleton, *A History of the Thermometer* (Baltimore, 1966), chs. 2-3.

†\* See Collins to Boyle, 1 September 1663; see also 20 November 1663: *Correspondence*. Boyle possibly met Collins at court on 29 December 1662: see *Occasional Reflections*, below, vol. 5, p. 173. On Collins see L. Loewenson, 'The Works of Robert Boyle and "The Present State of Russia" by Samuel Collins (1671)', *Slavonic and East European Review*, 33 (1955), 470-85, though Loewenson was not aware of the existence of the Boyle Papers.

†\* Birch, *Royal Society*, i, 378.

†\* See Loewenson, art. cit., p. 481n; see also *Forms and Qualities*, below, vol. 5, p. 288.

†\* See *Correspondence*, 1664-5, passim. For a brief summary, see Marie Boas Hall, 'What happened to the Latin edition of Boyle's *History of Cold?*', *NRRS*, 17 (1962), 32-5.

†\* It was just at this time that the engraved Faithorne portrait of Boyle reproduced as the frontispiece to vol. 1 of this edition was being finished, but there is no reason to think that it has any connection with this book.

†\* Birch, *Royal Society*, ii, 2, 5. No copies of this issue have been located.

†\* *Phil. Trans.*, 1 (1666), 8-9, 46-52.

†\* Birch, *Royal Society*, ii, 28. The inscription to the Royal Society's presentation copy of the work gives the same date.

†\* *Stationers' Company Register*, ii, 355 (in the description of the book, Merrett's Christian name is incorrectly given as Charles: cf. below, p. 372.)

†\* *Term Catalogues*, ii, 7.

†\* See below, vol. 9.

†\* Birch, *Royal Society*, ii, 492-5, where the text is printed from the version in the Society's Register Book (see Table); iii, 175.

†\* Hall, 'What happened to the Latin edition?'; Oldenburg to Boyle, 27 July 1668, *Correspondence*.

†\* This may be deduced from the fact that Hyde states that the work that Wase was translating comprised 49 pp. in quarto in its English edition: this must be a reference to the appendix to the 1683 edition, which comprises 29 pp. and with which Merrett's adjacent 20-page 'Account of Freezing' was perhaps conflated due to a misunderstanding. See *Correspondence*. For a translation of 'Examen of Antiperistasis' in hand L, see Table.

†\* Glanvill, *Plus Ultra* (London, 1668), pp. 96-7.

†\* See *Correspondence*.

†\* Margaret Cavendish, Duchess of Newcastle, *Observations upon Experimental Philosophy* (London, 1666), sig. b1v and passim. The Duchess's allusion to the fashion for including a list of the author's writings may also have been aimed at Boyle.

†\* M. Grilli, 'Robert Boyle: contributi alla fisica del calore', *Physis*, 24 (1982), 489-517; R.-M. Sargent, *The Diffident Naturalist: Robert Boyle and the Philosophy of Experiment* (Chicago, 1995), pp. 193ff.

†\* Steven Shapin, *A Social History of Truth* (Chicago, 1994), esp. ch. 6.

†\* See *Correspondence*. For a further letter in which queries were raised about the text, see Oldenburg to Boyle, 3 December 1664: however, in this case, emendations were not suggested.

†\* Boyle probably means his nephew Richard Jones (1641-1712), 3rd Viscount and 1st Earl of Ranelagh, the son of his sister Katherine, at whose home Boyle lived from 1668 until his death in 1691. Throughout the body of the work Boyle calls his nephew 'Pyrophilus', a sobriquet he had used in two other works ostensibly written for Jones, *Certain Physiological Essays* (1661) and *The Usefulness of Natural Philosophy* (1663-71): see above, vols. 2-3 and below, vol. 6.

†\* Evidently the Oxford group of natural philosophers.

†\* Aristotle, *Metaphysics*, 982b12; Plato, *Theaetetus*, 155d.

†\* See above, p. 5n.

†\* See below, p. 168.

†\* 'things are more easily moved once set in motion'.

†\* For Boyle's 'Dialogues concerning Flame and Heat', see above, General Introduction p. xxxiii, and below, vol. 13.

†\* An allusion to Francis Bacon, 'Experiments in Consort touching the production of Cold', *Sylva sylvarum* (1627), cent. i. 69-75.

†\* Henry Oldenburg (1618-77), Secretary to the Royal Society. For his role in the publication of this work, see Introductory Note.

†\* This error, listed in the errata, was not corrected in the 2nd edition, which repeats this warning. Lat., however, corrects the mistake. Here, it has been corrected so that the passage referred to



appears on p. 79.

†\* Lat. lacks this sentence.

†\* In cases where these necessitate the transference of text, or where authorial interference seems to be involved, attention has been drawn to the changes made in notes at the relevant points in the text. Otherwise, they have been silently corrected. In the 2nd edition, all but the first of the Errata were omitted, but only certain of the errors were corrected.

†\* Boyle often abbreviates 'Pyrophilus' to 'Pyro:'. For the identity of this person, see above, p. 5n.

†\* See part 1 of *La dioptrique* by René Descartes (1596-1650), the French mathematician and philosopher; this formed one of the *Essais* appended to the *Discours de la méthode* (1637).

†\* Probably Katherine Jones, Lady Ranelagh. See above, p. 5n.

†\* Citing a work by the Italian physician Epifanio Ferdinandi (1569-1638), probably his *Centum historiae, seu observationes et casus medici* (1621). The figure referred to in the previous paragraph has not been identified.

†\* It is unclear which work Boyle means; a possibility is 'Vitiated Sight', appended to *Final Causes* (1688), below, vol. 11.

† The square brackets are Boyle's.

†\* More information about John Vermaasen, the blind organist from Maestricht whose story follows, is found in Oldenburg's letter to Boyle of 10 October 1665 (too late to have been incorporated into *Colours*): see *Correspondence*. Oldenburg learned about Vermaasen from a M. du Son, evidently the man who later made a water pump for Finch's house and who may have been a source of some of Finch's information. See M.E. Rowbottom, 'Some Huguenot Friends and Acquaintances of Robert Boyle (1627-91)', *Proceedings of the Huguenot Society of London*, 20 (1959-60), 177-94, on p. 188.

†\* On views on colours before Boyle, see below, pp. 52-3, 56-7.

†\* Literally, 'corpuscles of colour'.

†\* The Epicurean stance was expounded by the French natural philosopher Pierre Gassendi (1592-1655) in his 'Physica', i. 6, ch. 12; this appears in his *Opera omnia* (1658), vol. 1. For the view that colour is an inherent property, see Aristotle, *De sensu*, 439a7-440b27.

†\* Boyle's vague reference is probably to Gassendi's 'Physica' (see above, p. 52n) rather than the work quoted on pp. 61ff. below, *De apparente magnitudine solis humilis* (1642).

†\* On Aristotle's view, see above, p. 52n.

†\* See Plato, *Timaeus*, 67c-68d.

†\* For Descartes' views, see his *Dioptrique* (above, p. 29n), books i and vi.

†\* I.e., the next part of the work.

†\* See above, p. 57.

†\* Lat. lacks bracketed phrase.

†\* Xenophon, *Anabasis*, Book IV, ch. 5; in fact, Cyrus was already dead, and Cheirisophus was leading the army.

†\* This was Samuel Collins (1619-70), physician to Tsar Alexis Romanov from 1660-9 and author of the posthumously and anonymously published book *The Present State of Russia* (1671), written in the form of a letter to an unnamed friend who has since been identified as Boyle. See Collins to Boyle, 1 September 1663, in *Correspondence*, and L. Loewenson, 'The Works of Robert Boyle and "The Present State of Russia" by Samuel Collins (1671)', *Slavonic and East European Review*, 33 (1955), 470-85.

†\* Thomas James, *The Strange and Dangerous Voyage of Captain Thomas James in his intended Discovery of the Northwest Passage into the South-Sea* (1633).

†\* Here Lat. has 'trium quatuorve passuum', 'three or four paces'.

†\* See above, p. 62.

†\* Literally, in its least parts.

†\* See above, p. 62.

†\* See Experiment I, below, p. 77.

†\* See above, p. 61.

†\* Gassendi, *De apparente magnitudine*: 'Bodies that are by their nature black are rightly held to be made up of particles whose tiny surfaces are rough and do not easily reflect light outwards.'

†\* It is not clear to whom Boyle here refers. This is also true of the figure mentioned at the start of sect. 9.

†\* See above, pp. 40ff.

†\* Clearly Boyle intended to use the plural here, as he did in the Contents on p. 17.

†\* The experiment described here is probably taken from 'Of Fermentation', a short treatise by Boyle's friend, the Oxford physiologist Dr Thomas Willis (1621-75), that was first printed in Latin as part of Willis' *Diatribae duae medico-philosophicae quarum prior agit de fermentatione* (1659). See A.G. Debus, 'Sir Thomas Browne and the Study of Colour Indicators', *Ambix* 10 (1962), 29-36, on p. 32.

†\* Here the 'Errata' alert the reader to the fact that the last part of Experiment IV was printed erroneously on pp. 142ff.: see above, p. 24. We have moved that paragraph forward. Although this was not actually corrected in the 2nd English edition, Lat. has the correct order.

†\* It is not clear to which of his writings Boyle here refers.

†\* 'burned things are black, but thoroughly burned ones are white'.

†\* 'thoroughly burnt'.

†\* Pierre Belon, *Les observations de plusieurs singularitez et choses memorables, trouvées en Grece, Asie, Indée, Egypte, Arabie, et autres pays estranges* (1553).

†\* In both the 1st and 2nd English editions, this paragraph contained another two sentences that, according to the 'Errata', properly belong on p. 79 above, where we have moved them: see above, p.24.

†\* Here Lat. has a footnote not found in either English edition: '*Bismutum* aliàs dicitur *Marcasita argentea*.' ('Tinglass is also called silver Marcasite.')

†\* Literally, 'to dryness'.

†\* The Antwerp cartographer Abraham Ortelius (1527-98), whose comprehensive atlas of the world, *Theatrum orbis terrarum* (1570), was often translated. Ortelius also wrote several works on classical studies and geography.

†\* Prince Pyotr Semyonovich Prozorovsky and Ivan Afanas'yevich Zhelyabuzhsky, on a diplomatic mission from the Tsar, were received by Charles II on 29 December 1662. The splendour of the occasion, which Boyle witnessed, led him to write the 5th reflection in the last section of *Occasional Reflections* (1665), where more details are given: see below, vol. 5, p. 173. The physician whom Boyle mentions here was Samuel Collins; see above, p. 64n. The square brackets are Boyle's. The surviving MS version of this section of the text (BP 17, fol. 66) lacks the passage in square brackets and the sentence that follows it, which is consistent with what Boyle says about it having been added later.

†\* Olaus Magnus, *Historia de gentibus septentrionalibus*. See above, p. 64.

†\* Dr Samuel Collins, see above, p. 64n.

†\* Olearius, *Relation*. See above, p. 85.

†\* Here Lat. adds two paragraphs not found in either English edition (pp. 159-60).

Sed occasione illius, quod de Animalium albescentibus pilis diximus, ut ad considerandum Te invitem, quousque & in quibus casibus adventitia pilorum Albedo ex Textura Cutis & Qualitatibus succi alimentarii dependeat, subnectam unum alterúmve, quod mihi in mentem modò venit. Unum est, quòd passim (licèt non semper) in equorum tergoribus videmus istas partes, quas Ehippium sauciavit & exoriavit, postmodum albos pilos emittere; unde quidam pro secreto habent, ad figuras stellatas equorum frontibus formandas, partem affectam pomis assatis, utprimùm ab igne sustuleris, adurere, ut super cicatrice pili proximè nascendi albescant.

Et videmus, Senium, uti afferre capillicidium solet, ita in canum, quod remanet, vertere. Et diligens *Piso* narrat, in *Brasilia*, ubi Medicinam faciebat, uti Indigenæ sunt valde longævi, crebrò centesimum annum supergredientes, ita nunquam eos calvescere vel canescere. Alterum est, quòd videtur nihilominus, nigrum capillitium multò minus durare, quàm nigredinem cutis, quam præsumere possumus esse ætate solâ propè indelebilem, ex ista narratione, quam *Marcgravius* (jamjam rursùm citandus) in libro suo, ab ipso *Pisone* edito, nobis tradit de *Æthiopibus*, quos in *Brasilia* viderat, his verbis: *Multus vidi Æthiopes Senes, barbâ densa & totaliter canâ, ut & capillis capitis canis. Lepidum spectaculum! atra barba, quæ incanuit, in nigra cute videtur, quasi farina esset adpersa.*

But I shall take the opportunity afforded by those remarks that I have made on the subject of the hairs of animals turning white to subjoin one or two things that now occur to me, so that I can invite you to consider to what extent in such cases the process of whitening that affects the hairs results from the texture of the skin and the nature of pigments in food. One of these points is the following: that we do very often (although not always) see that those parts on the backs of horses which the harness [or, horse-cloth] has wounded and stripped of skin, later emit white hairs; and then some people claim the following secret recipe for forming star-like shapes on the foreheads of horses, that one should burn the relevant part with roasted apples, taken immediately from the fire, so that the hairs that will grow again most closely above the scar should turn white.

And we see that old age, while it tends to cause men's hair to fall out, also tends to turn white the hair that remains. And that careful author Piso tells us that in Brazil, where he used to practise medicine, while the Indians are very long-lived, often surpassing their hundredth year, so also do they never go bald or white-haired. Nevertheless, it seems to be the case that the production of black hairs lasts much less long than the blackness of the skin, which we can presume to be almost immune to the unaided efforts of age, according to the following information, which Marcgravius (whom we must now cite yet again) gives us in his book, published by that same Piso, on the subject of the negroes [lit., Ethiopians] whom he saw in Brazil. These are his words: 'I have seen many aged negroes who have a thick and entirely white beard, and also have white hairs on their heads. An amusing sight! The black beard, which has turned white, can be seen on the black skin, as if it had been sprinkled with flour'.

On Piso and Marcgrave, see below, p. 89n.

†\* I.e. the traveller and poet George Sandys (1578-1644). However, the 'late... Critick' has not been identified.

† See Genesis 9, 25.

†\* From *Airs, Water, Places*, 13, from the Hippocratic corpus.

†\* Presumably a reference to the *Introduction to Aristotle's Logic* by Porphyry (232-c. 305), a disciple of Plotinus.

†\* Neither this figure, nor the one referred to in the next sentence, has been identified.

†\* Here Lat. adds a passage not found in either English edition (pp. 166-8).

Atque hîc ausim tibi considerandum proponere, quòd, cùm per Experimentum decimum pateat, Liquorem arte paratum (Solutionem scilicet Argenti) a cute imbibitum, posse intra pauca minuta eòusque Texturam ejus mutare, ut quandam ipsi nigredinem conciliet; sic ab alia quadam re, quam jamjam Tibi sum commemoraturus, est concludere, succum simplicem & naturalem, quique magis est excolor, posse, licèt tardiusculè, eandem operationem in cute edere. Vir quippe fide dignus, mihi notus, qui, in *America* cùm esset in eo plurimùm erat, ut Rariora investigaret, fructum mihi quendam monstrabat, inde allatum, *Limonio* haud adeò absimilem, & quamvis, cùm eam videbam, aridus esset, adeoque ineptus mihi ostendendi, quod voluissem ipse experiri, ipse tamen jugiter affirmabat ex sua ipsius experientia, Quòd si fructûs illius succum manui incuteret, quamvis Liquor claritate esset aquæ par, primitûs colore purpureo cutim tingeat; sed si rarsùm sufficientem Liquoris ejusdem quantitatem, ei superfunderet, color in Succineam Nigredinem intendebatur; Et, quod mirùm est, nullâ arte laborève eluebatur; at intra 8. vel 10. dies de semetipso peribat. Et quamvis hic vir afferebat ea, quæ satis me inducebant ad fidem ipsi hac in re adhibendam; attamen, probabilitatis firmandæ gratiâ, præter rem non fuerit adjicere, occurrisse mihi descriptionem ejusmodi fructûs *Americani* in nupera quadam de istis partibus Narratione, à bonæ notæ Authore



facta (erat quippe, ut memini, vel *Marcgravius*, vel *Piso*;) Atque uti descriptio sat bene congruit, sic Author succo hujus fructûs easdem ferè operationes adscribit, quas amicus meus fructui, quem ipse videbam, tribuebat; nisi quòd Author ille non observat, primam cutis humectationem, Liquoris adminiculo factam, purpureum duntaxat vel similem talem obscurum colorem producere: quo tamen in puncto, Amici mei narratio probabilior videtur.

Atque hoc, particularibus illis junctum, quibus id hoc loco associamus, quædam fortè monita Tibi suggeret, quæ, cùm aliis nunc rebus sim occupatus, haud conabor nunc exequi, cùm revertendum mihi sit ad eam Dissertationis meæ partem, ubi *Gallici* illius *Missionarii* Observationem commemorabam.

And here I would venture to propose the following for your consideration: that, as appears by the tenth experiment, when the liquid that has been carefully prepared (that is, the solution of silver) is imbibed by the skin, it can within a few minutes change its texture to such an extent that it can lighten some of its blackness. Thus we may conclude from another thing, which I have already told you, that a simple and natural juice, which is particularly colourless, can, albeit quite slowly, carry out the same operation on the skin. Indeed, I know a trustworthy gentleman who, when he was in America, was particularly concerned to investigate rarities; he showed me a certain fruit, brought from that place, that was not very dissimilar to a lemon. And although, when I saw it, it was dry, and thus not fitted for showing me the things on which I myself wished to experiment, nevertheless he kept assuring me, from his own experiments, that if this fruit touched its juice upon one's hand, although the liquid was as transparent as water, it stained the skin at first with a purple colour. But if a sufficient quantity of the same liquid is again poured over it, the colour is changed to an amber-like blackness. And, amazingly enough, it can be removed by no amount of skill or effort; but within eight or ten days it goes away of its own accord. And although that man gave me sufficient evidence to lead me to think that he should be trusted in this matter, nevertheless it would not be out of the way to add, in order to strengthen our belief in it, that I have come across a description of this kind of American fruit in a certain recent treatise about those parts, written by a reputable author (who was, indeed, as far as I recall, either *Marcgravius* or *Piso*). And as the description fits well enough, so also the author ascribes almost the very same properties to the juice of this fruit, as my friend attributed to the fruit that he had himself seen. The only difference is that the author does not observe that the first wetting of the skin by the liquid produces a purple or such-like dark colour. On this point, however, my friend's report seems the more probable one.

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And this fact, joined to those particulars which I here add to it, might perhaps suggest certain things for you to take note of, which, since I am now taken up with other matters, I shall not now try to follow up, when I should be returning to that part of my treatise, in which I was discussing the observation of that French missionary.

On *Marcgrave* and *Piso*, see above, p. 89.

†\* Here Lat. adds a further passage not found in either English edition (pp. 170-1).

Ac æquè præmemoratum Testimonium *Andree Batteli*, quàm conjectura nostra, cujus gratiâ id afferebamus, confirmari potest narratione Authentici Autoptæ *Marcgravii*, Medici *Coloniæ Belgicæ* in *Brasilia*, ubi Regionis istius historiam ordiebatur, quam dolendum est ab ipso non esse ad finem perductam. *Vidi hîc* (inquit) *plenè albissimum Juvenem octodecim annorum, pilis crispis albissimis, & superciliis albis, cute albissimâ, naso plane more Æthiopum, qui natus hîc à patre & matre Nigritis*. Et quod valde est notabile, idem Author, recensens differentia genera hominum, quos diversarum gentium mixti congressus in *Brasilia* produxêre: *Vidi* (inquit) *Africanam fæminam, non nigram, sed ruffâ planè cute, ac pilis ac capillis ruffis. Ex qua regione esset, non potui resciscere; nam Linguam ejus non intelligebant cæteri Nigritæ*.

And the previously-noted testimony of Andrew Battel, as well as my own conjecture, for the sake of which I mentioned it, can be confirmed by the narration of a man who truly saw it with his own eyes, Marcgravius, the doctor of the Dutch colony in Brazil, where he began the history of that region, which one must regret that he was not able to complete himself. 'I have seen here,' he says, 'a thoroughly white [or, albino] youth of eighteen years, with very white curled hairs, and white eyebrows, and a very white skin; his nose is clearly of negro form, and he was born here of negro parents.' And, which is most striking, the same author, when he surveys the different types of people, whom the mixed unions of diverse races have produced in Brazil, says: 'I have seen an African woman who was not black, but had quite reddish skin, and red hair on head and body. I could not discover from which region she came, for the other negroes did not understand her language.'

On Battel, see above, pp. 91-2; on Marcgrave, see above, p. 89.

†\* The 'Errata' notes that this should be altered from 'yet with the Solution of Pot-ashes and other Lixivate Salts', a change that seems likely to have emanated from Boyle rather than a compositor. See above, p. 24.

†\* For Aristotle's views, see above, p. 52.

†\* See above, p. 81.

†\* Lat. has 'duorum circitur et dimidii', 'about two and a half inches'.

†\* See the plate on p. 103, which faces this page in both English editions.

†\* See above, p. 107.

†\* Like a peacock's tail, variegated.

†\* It is not clear to which of his works Boyle here refers.

†\* The principle of extraversion occurs in the tract, 'Gas aquae', in *Ortus medicinae* (1648) by the Flemish natural philosopher J. B. van Helmont (1579-1644).

†\* The square brackets are Boyle's.

†\* Here Lat. has three sentences not found in either English edition (pp. 268-9).

Ac præter alios flores, quorum nomina mihi non occurrunt, examinatis itidem eodem modo albis foliis Florum de duplici genere diverso prunorum, parum ea alterata fuêre per Sal Acidum, Alcalizati verò ope elegans flavum exhibuêre. Eodémque eventu de Florum *Mali Armeniaci* foliis Experimentum capiebamus: Flores verò Mali *Persici*, uti colore sunt, rosas propemodum *Damascenas* referente, sic modo supra tradito versati, Salis Alcalizati adminiculo colorem subviridem, & ope Spiritûs Acidi, Rubrum exhibebant. Sed neque hic neque ille (inprimis viridis) tam erant luculenti & vividi, ac in compluribus floribus aliis eâdem methodo produxeramus.

And leaving aside those other flowers, whose names I cannot recall, when I examined in the very same way the white petals of the flowers from the two different kinds of plum-tree, they were very little affected by acid, but when treated with an alkali they showed an attractive yellow colour. And I made an experiment, with the same result, using the apricot blossoms. Peach blossoms, as well, being very similar in colour to damask roses, when treated in the manner described above, showed a somewhat greenish colour with the addition of an alkali, and a red colour when acid was applied.

But neither the one nor the other (in particular, the green) was of as bright and strong a hue as that which I have produced in many other flowers by the same method.

†\* At this point, Lat. has a lengthy passage not found in either English edition (pp. 273-8).

Attamen, quoniam hujus generis Experimenta usui esse possunt Medicis & Chymicis, eorum duo vel tria subnectam, prout ea in Collectaneis meis consignata sunt. Quamvis Violarum vulgarium, recenter decerptarum, & albæ chartæ fortiter affrictarum, partes expressæ in Rubrum verterentur, Acido, & in Viridem, Alcalizato Sale, uti accidere solet Syrupo Violarum; tamen quando, Curiositatis ergò, albarum Violarum folia eodem modo cum iisdem Liquoribus examinabam, ea pars, quæ Spiritu Salis erat humectata, vix erat sensibilibiter alterata, nisi quòd parùm magis ad pallorem transibat; & ea, quæ liquore lixiviato humectata erat, mox vertebatur (uti de aliis Floribus albis observavimus) in saturum Flavum, Viridi dilutiori nonnihil approximantem. Neque hîc omittam hanc circumstantiam; quòd observans in Violis hisce albis, circa foliorum alborum fundum, esse solere genus quoddam parvi noduli (quo enim nomine apud Botánicos veniat, non memini) colore non multum Violarum vulgarium colori, nisi quòd pallidior est, absimili; harum quasdam etiam compressas albæ chartæ affricabam; & quamvis *Alcali* non multùm videbatur colorem ipsarum ad viridem mutare; Spiritus tamen Salis mox partem, quam humectabat, in Russum Gilvúmve colorem mutabat. Eo fortassis effectus Alkali fuisset manifestior, si satis magna harum coloratarum Floris partium copia mihi suppetiisset ad Experimenta mea repetendum.

Primulæ Veris purpureæ, chartæ affrictæ, Acidi Salis beneficio, rubræ fuerunt redditæ, & mediante Alcalisato in cænosum viride conversæ; foliis verò flavarum sive vulgarium Primularum Veris, duplicium juxtà ac singularum, chartæ similiter affrictis, idem Spiritus Acidus nonnisi *diluere* colorem videbatur, Alkali verò ipsum in saturum Flavum sive Limonium mutabat.

Similem differentiam manifestam comperi inter diversa genera *Auricularum Ursi*: Cùm enim Flores, qui erant colore purpureo, vel simili tali, chartæ albæ affricti, illico in rubrum verterentur per Spiritum Salis, & in Viride per Alkali; Isti, qui colore erant Limonio, vel Flavo saturiori (experiebar enim de utroque genere) simili chartæ affricti, colorem suum, mediante Solutione Alcalisatâ, nonnisi in saturiorem Flavum mutabant, & mediante Spiritu Acido eum duntaxat dilutum nanciscebantur.

Sic in Tulipis deprehendebam, chartam, affrictu foliorum, ex iis, quæ rubræ erant, acceptorum, infectam, fuisse Spiritu Salis in lucidius gratiúsque Rubrum versam, & mediante Alkali in Viride; cùm, si folia ejusdem florum generis affricaremus, quorum aliqui colore erant luculenter Flavo, alii pallente Limonio, alii Albo, charta, ipsorum succis imbuta, colorem suum non mutabat adhibito eodem Salino Liquore, cujus in ipsum operationes erant istis similes, quas nuper referebam ipsum in Flavas *Ursi Auriculas* edidisse. Sed videri poterit notabilius aliquantò, fieri posse, ut differentes Floris ejusdem partes, mediante eodem Sale exhibeant colores differentes, observationi illi congruenter, quam de differentibus Floribus jam tradidimus.

Licèt enim in Tulipis flavis succus Foliorum, ut modò notabam, Rubrum non exhiberet Salis Acidi adminiculo, nec Cæruleum, mediantibus Lixiviatis: quinimò, quamvis erectæ partes, quæ intra Tulipas proveniunt, circa cauliculum, qui Semen dicitur continere, in plerisque Tulipis Flavis colore sint pallentiori, attamen cùm repererim Tulipas quasdam Flavas, in quibus hæ Excrescentiæ, perinde ac Foliorum ima pars, colore erant obscuro, deprehendi, hæ cùm comprimerentur, & Spiritu Acido humectarentur, illico eas Rubrum, & cum Sale fixo, viride exhibuisse. Atque in ipsa etiam Tulipa Alba, ubi Excrescentias hasce colore paululùm intensiori reperiēbam, Experimentum eodem capiebam successu. Et memini, in floribus similiter Pomi, quamvis Foliorum color, mediantibus duobus istis, toties à me commemoratis, Salium generibus, non fuerit aliter mutatus, ac jam de aliis coloribus Albis observavimus, Semina tamen (uti vulgò appellantur) in candidissimis illis Floribus contenta, cùm sint subrubra, mediante Spiritu Acido & Sale Lixiviato, in charta, super qua comprimebantur, jucundum Rubrum atque Viride exhibebant. Sed, quod est omnium maximè

notabile, quòdque fortassis omnium maximè conducit, ut ostendatur, differentes ejusmodi Colores Florum, qui merè habentur accidentales, posse ex insigni in ipsorum Textura disparitate proficisci, hoc est, Quòd pluries, quàm semel bísve, fui expertus, idque in floribus differentibus, diversas partes unius ejusdémque Folii diversis Texturis, secundùm distinctos ipsius colores, præditas esse posse. Etenim selecto Tulipæ folio, partim Albo, partim Rubro, istisque partibus cum cura à se invicem forfice divisus, comperi, quòd, cùm alba pars folii, albæ chartæ affricta, nonnisi dilueretur humectando eam Spiritu Salis, nec nisi flavum consequeretur colorem, adminiculo *Alcali*; charta è contra, Rubris ejusdem folii partibus, ipsi affrictis, infecta, in jucundum Rubrum vertebatur, & humectata cum *Alcali*, gratum sed aliquantum è cæruleo Viride exhibebat: Idémque præstabat

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color Purpureus sive Violaceus, quem facili negotio à gemini coloris Excrescentiis, intra florem illum prognatis, defricabam. Atque successu haud dissimili alia nonnulla Tuliparum folia etiam examinabam, quorum singula partim rubra, partim flava erant, quæque etiam obscuri coloris excrescentias intra sese productas continebant.

Tantùm ex Notis nostris; quibus expeditis, ut ad id progrediamur, quod in promptu erat afferre, quando Violarum diversimodè coloratarum mentio istas inserendi mihi dabat occasionem.

However, since experiments of this sort can be of use to physicians and chemists, I shall subjoin two or three of them, since they are recorded in my notebooks. Although when ordinary violets have been freshly picked, and are rubbed vigorously on white paper, the parts that are pressed out of them are turned red by acid and green by alkali (as tends to happen with syrup of violets), nevertheless when, for the sake of curiosity, I examined the petals of white violets in the same way with the same liquids, that part that had been moistened with spirit of salt was scarcely altered to any perceptible degree, except that it became a little paler; and that part which had been moistened with liquid of lye [*liquor lixiviatus*; alkali] was soon turned, as we have observed in the cases of other white flowers, to a dull yellow, quite similar to a thinnish green.

Nor will I here omit the following circumstance: I notice that in these white flowers, there tends to be some sort of small nodule (I do not remember by what name it would be known to the botanists) of a colour that is not very dissimilar to that of ordinary violets, except that it is paler. Some of these I pressed together and rubbed on white paper, and although the alkali did not seem to turn their colour much towards green, nevertheless the spirit of salt soon changed that part that it had moistened to a reddish-orange colour. And perhaps the effect of the alkali would have been more obvious, if I had had a sufficiently large supply of these coloured parts of the flower with which to repeat my experiments.

Purple spring primroses, when rubbed on paper, were made red by the assistance of acid, and when treated with alkali were turned into a dirty green. But as for the petals of yellow, or ordinary, spring primroses, both single and bipartite, when rubbed on paper, that same acid seemed to have no effect on them except to dilute the colour, while the alkali changed them into a dull yellow or lemon colour.

I observed a similar clear difference between different types of *auriculæ ursi* [lit., bear's ears]. For when those flowers that were purple, or of a similar colour, were rubbed on white paper, they were straightaway turned to red by spirit of salt, and to green by an alkali. Those which were of a lemon colour, or a deeper yellow (for I experimented with both sorts), when rubbed on the same sort of paper, changed their colour, by the mediation of an alkaline solution, merely to a deeper yellow; and with the acid, it merely happened to become diluted.

Thus, I found in the case of tulips, that paper which had been affected by rubbing with petals taken from those that were red was turned by spirit of salt into a brighter and more pleasant red colour; and by the addition of alkali, to green. While, if I rubbed petals of the same types of flower, of which some were bright yellow in colour, others a pale lemon colour, and others white, the paper



that had been imbued with their juices did not change its colour with the addition of that same salt solution, which acted on them in the same way as it did upon the *auriculae ursi* that I have just mentioned above. But something a little more notable could be seen: that it was possible for different parts of the same flower, when the same salt was applied to them, to show different colours; and this fact is consistent with the observation that I have just made on the subject of different flowers.

For while, in the case of yellow tulips, as I have just now noted, the juice of the petals does not produce a red colour when acid is added to it, nor a blue, with the addition of lyes [i.e., alkali]; nevertheless, although those erect parts which appear inside tulips, around the stalk that is said to contain the seed, are in most yellow tulips of a paler colour, I did however find some yellow tulips in which these excrescences, and also the lowest part of the petals, were dark in colour; and when these were pressed, and wetted with acid, they immediately became red, and when they were moistened with a fixed salt [i.e., alkali], they showed a green colour. And also in the white tulip itself, when I found these excrescences had a slightly more intense colour, I carried out this experiment with the same results. And I remember that it is similar, in the case of the flowers of the apple-tree; although the colour of the

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flowers, when affected by those two types of salt that I have so often mentioned [i.e., acid and alkali], were not otherwise changed, than in the way that we have observed with other white colours, nevertheless the seeds (as they are commonly called [i.e., pollen] that are contained in the whitest of those flowers, when they are slightly reddish in colour, show a pleasant red and green colour on the paper on which they are pressed, when treated with acid and salt of lye [i.e., alkali] respectively.

But the most notable fact of all, and one which perhaps most of all tends to show that these kinds of different colours in flowers, which might be taken as merely accidental, can in fact arise from an important difference in their textures, is the following: that I have found by experiment, on more than one or two occasions, and in the cases of different flowers, that various parts of one and the same petal can be equipped with differing textures, according to the distinct colours to be found in it. For when I took a tulip petal that was partly white and partly red, and carefully separated these two parts from each other with scissors, I found that, while the white part of the petal, rubbed on white paper, could merely be diluted by wetting it with spirit of salt, and was merely changed to a yellow colour by the addition of an alkali, nevertheless the other sheet of paper, stained with the red parts of the same petal, was turned [by acid] to a pleasant red colour, and when wetted with alkali, showed an attractive but rather bluish green. The same results were also shown by the purple or violet colour, which I was easily able to rub out of the twin-coloured excrescences that were produced inside that same flower. And I have also examined several other tulip petals with quite similar results; each one of these was partly red, and partly yellow, and each contained dark-coloured excrescences that had been produced within itself.

So much for my notes; now that I have covered these points, I can carry on to the subject that I was on the point of introducing, when the mention of differently-coloured violets gave me the opportunity to insert these notes.

<sup>†\*</sup> Citing a work by the Southern Netherlandish botanist Carolus Clusius (1524-1609), probably his *Rariorum plantarum historia* (1601).

<sup>†\*</sup> It is not clear to what work Boyle here refers: it is apparently not *Sceptical Chymist* (1661), above, vol. 2.

<sup>†\*</sup> It is not clear what work Boyle here refers to.

†\* Here Lat. has a passage not found in either English edition (pp. 294-6).

Sed (*Pyrophile*) magis fortè notabile existimaveris, ejusmodi colorum degenerationes posse quandoque in succis Animalium observari; cùm istius generis Exempla non sint, quòd quidem Ego novi, ab iis, qui de Coloribus tractant, notata; una tamen ejusmodi Instantia nobis suppeditatur, à parvo quodam *Americano* sive pisciculo sive Insecto marino, quem nostratium nonnulli *Muricem* piscem appellitant, licèt ejus structura, quantum quidem Ego poteram judicare ex uno, quem videbam, mortuo, satis esset peculiaris. Quamvis enim longitudinis esset, unum inter & duos pollices mediæ, & quantum conjiciebam, pollice latior, tegumentum tamen ipsius multò magis referre videbatur tegumentum Insecti, quàm aliarum concharum; Constabat quippe multis testis duris, sibi invicem superinnatis, squamarum instar in istius generis Insectis, quæ in officinis *Millepedes* appellantur. Jam verò de hoc pisce *Murice* vir valdè curiosus & fide dignus mihi asseverabat, quòd cùm in *Indiis* ad littus maris unum ipsorum vivum tolleret, succus ejus, qui manum ipsius inficiebat, primùm apparuerit in ipsa Viridis, deinde Cæruleus; undè cùm degenerasset in Purpureum, tandem desiverit in eleganti Rubro: quæ de spontanea Colorum degeneratione narratio geminam vix habet; maximè si has præterea adjiciamus circumstantias, quòd scire mihi cupienti, idémne succus ullam rem aliam fuisset infecturus, quàm cutim manus, cujus peculiarem texturam & calorem suspicari quis posset ad operationem illam contribuere, asseverabat, quòd strophiole suo succum abstersurus invenerit, non modò succo illud similiter fuisse infectum, sed & colorem in Linteo durâsse non eluendum: quam postremam circumstantiam, licèt valdè mira videatur, prohibeor existimare incredibilem, ex eo quòd Princeps quidam, qui rerum Cognitione juxtà ac Sceptro Monarchico est *Augustus*, mihi aliquando dignabatur, audente me aliquid, occasione datâ, ipsi offerre circa purpuram, narrare de Pisce ignoto, capto in mari *Britannico*, & à Piscatoribus, qui eum ceperant, nec tamen, cujus generis esset, noverant, ad Majestatem suam pro re rara allato. Succus quippe piscis Linteum ab ipso humectatum colore inficiebat purpureo vel Coccineo, qui elui non poterat. Atque alibi annotavi, cognatum quendam meum manicam mihi monstrâsse indusii sui, quam piscis cujusdam, ibidem à me notati, succo infecerat; qui succus, quamvis ab initio subviridis, postea, cùm eum eluere conabatur, in colorem vertebatur coccineum, quem adhuc, me ipsum vidente, tenebat, licèt indusium illud crebrò lavatum gestatúmque fuisset.

But, my dear Pyrophilus, you will perhaps think it even more noteworthy that the degeneration of this sort of colours can sometimes be observed in the juices of animals. Indeed, as far as I know, no examples of this nature have been noted by those who write on the subject of colours. I have been supplied, however, with one instance of this phenomenon, by a certain small American fishlet or marine insect, which some of our people tend to call the murex-fish. One must admit that its structure, as much as I could judge from one dead specimen that I have seen, is strange enough. For although it is 1½ inches in length, and, as far as I could guess, more than an inch wide, nevertheless its covering seems to be much more akin to the covering of an insect, than to that of other shellfish. It consisted, indeed, of many hard scales overlapping each other in turn, like the scales of that sort of insect which is known by the technical name of 'millipede'. Now, on the subject of this murex-fish, a truly curious and trustworthy gentleman told me that when, in the Indies, one of these creatures was brought ashore alive, its juice, which dyed his [i.e., the informant's] hand, first appeared on it as green, then as blue; thence, after it has degenerated to purple, it finally ends in an attractive red colour. This information about a spontaneous degeneration of colours has scarcely any parallel; especially so, if we add these following additional circumstances. When I wished to know whether this same juice would dye anything else apart from the skin of one's hand, since someone might suspect that the hand's own unique texture and heat might assist this operation, he asserted that when he was about to wipe the juice away from his shirt-cuff, he found that it had not only been similarly stained with the juice, but that the colour remained in the linen and could not be washed away. This last circumstance, although it does indeed seem quite amazing, I am prevented from thinking unbelievable by something that a certain Prince (who is truly an Augustus, as much in his understanding of the world as in his royal power) once deigned to tell me, when I was making so bold as to offer him some information on the subject of purple, having been invited to do so; he

told me of an unknown fish that had been caught in British waters, and had been brought as a rarity to his Majesty by the fishermen who had caught it, but who did not know what kind of creature it was. Indeed, the juice of this fish dyed linen that had been moistened with it a purple or scarlet colour, which could not be removed. And I have noted elsewhere, that a relation of mine showed me a sleeve of his garment that he had dyed with the juice of a certain fish, which I described in that place; this juice, although it was greenish to begin with, was later, when he tried to clean it off, turned to a scarlet colour, which it still had, when I saw it, although the garment had been frequently washed and worn since then.

Boyle may have seen the murex in the repository of the Royal Society: see Nehemiah Grew, *Musæum Regalis Societatis* (London, 1681), pp. 124ff. The 'certain Prince' was probably Charles II.

†\* Pierre Gassendi, *Animadversiones in decimum librum Diogenis Laërtii, qui est de vita, moribus placitisque Epicuri* (1649).

†\* See pp. 111-13 above.

†\* 'Trifling amount'.

†\* In fact, p. 134 in the 1st edition, p. 77 in this one.

†\* Perhaps a reference to *The Sceptical Chymist*, above, vol. 2.

†\* We cannot identify this pamphlet, but the story of the water drinker was taken from *De fermentatione* by Thomas Willis. See above, p. 78n.

†\* It is not clear who is here referred to. However, Richard Jones ('Pyrophilus') had himself met Francis Mercurius van Helmont (1614-99), son of the famous chemist, in September 1658 at Frankfurt-am-Main while touring the continent with Henry Oldenburg. See Oldenburg to Boyle, 10 September 1658, in *Correspondence*.

†\* The 'famous Spagyrist' is Alexander von Suchten in his *Antinomii mysteria germina* (1604).

†\* Beguin, *Tyrocinium chymicum*: see above, p. 158.

†\* The substance which van Helmont called 'Paracelsus' ludus' was a mineral, perhaps boracite or magnesium borate, found near Antwerp (J.R. Partington, *History of Chemistry* (4 vols., London, 1961-70), ii, 226).

†\* Here Lat. has three sentences not found in either English edition (pp. 366-7):

Atque cum Lapide Calaminari, qui non videtur merus lapis, Experimentum hujusce generis sumpsit, de quo Chymici quidam gratias mihi agerent. Etenim, monstrare Tibi Solutionem possum, quam de hoc Minerali paraveram, beneficio Spiritûs Salis (modo quadantenus peculiari extracti) adeò intensè tincti, ut pro eleganti Solutione Auri, cui etiam, quoad gustum, aliquatenus affinis est, habita fuerit, nec immeritò haberi possit. Sed in transitu, Chymici isti, qui Mineralia illa corpora concludere solent Auro fæta, in quæ agit *Aqua Regis*, maxime si *Menstruum* ab iis obtinuerit colorem ullatenus Flavum, multùm aberrare poterunt. Etenim, ut alia particularia alii loco reservem, de industria sum expertus, ramenta *Martis* facilè in *Aqua Regis* (quam Ego communiter ex *Aqua forti*, una cum pauco Spiritu Salis, paro) dissolvi, Solutionémque, quæ chartam albam Saturo Flavò inficiebat, largiri.

And I have begun an experiment of this nature (for which some chemists may thank me) with calamine, which does not seem to be a mere stone. For I can show you a solution which I had prepared from this mineral, by the addition of spirit of salt (though of a somewhat peculiar sort) that was so powerfully tinged, that it might have reasonably been taken for an elegant solution of gold, to which, as far as taste goes, it was somewhat similar. But, in passing, I note that those chemists may have been very much mistaken, who have tended to conclude that those mineral bodies are born from gold, on which the *Aqua Regia* can act, especially if the *Menstruum* made from them shall have a colour that is in any way yellow. For, while keeping the other details for another place, I have had practical experience of the fact that iron filings can easily be dissolved in *Aqua Regia* (which I prepare as a mixture, made from Aquafort, with a little spirit of salt), and that the resulting solution dyes a white piece of paper with a deep yellow colour.

†\* It is not clear which of his other writings Boyle refers to here and in the next sentence.

†\* The German chemist, J.R. Glauber (1604-70), author of *A Description of New Philosophical Furnaces* (1651) and other works.

†\* Probably a reference to the work described in p. 177n.

†\* Sir Robert Moray (1608-73), soldier, diplomat and a leading figure in the early Royal Society, who maintained an extensive correspondence with his friend Christiaan Huygens. For the MS of the 'Short Account' which exists in Leiden see Introductory Note; however, this covering letter survives only in this printed text, the version of which in Huygens, *Oeuvres complètes*, (22 vols., The Hague, 1888-1950), iv, 482-93 is taken from Birch's edition.

†\* Christiaan Huygens (1629-95), Lord of Zulichem in Holland, pursued original studies in physics, mathematics, astronomy and optics. He visited London in 1661 and again two years later, when he became a Fellow of the Royal Society. Clearly Boyle intended that Moray forward to Huygens this letter about the interaction of light and matter.

†\* I.e., 'Mr Clayton', who is almost certainly to be identified as John Clayton (c.1620-c.1710), lawyer, virtuoso and Fellow of the Royal Society, knighted in 1664. He visited Italy and France in 1663. See E. & D. S. Berkeley, *John Clayton: Pioneer of American Botany* (Chapel Hill, 1963), pp. 9-13. The editors of Huygens' *Oeuvres complètes*, iv, 483n, wrongly identify the figure in question as Sir Robert Clayton (1629-1707), London merchant and alderman.

†\* In its early years, the Royal Society met at Gresham College in the City of London.

†\* I.e., Charles II, who had obtained the diamond for his collection.

†\* See Birch, *Royal Society*, i, 329-30.

†\* Quoting from pp. 110f in *De gemmis et lapidibus libri II* by the Dutch mineralogist John de Laet (1593-1649), which was appended to the 3rd edition of the work by de Boodt just cited. 'Since Carbuncles were called *Pyropi* and *Anthraces* by the Ancients, it was widely held that they glowed in the dark, like coal: but so far this has never been observed in any gemstone, in spite of what is rashly claimed by some.'

†\* Literally 'the nature of things'.

†\* The Italian adventurer Ludovico Barthema wrote an account that was abridged as *The Travels of Lewis Barthema or Vertoman* in the 1st part of Samuel Purchas, *Hakluytus Posthumus: or, Purchas*



*his Pilgrimes: Contayning a History of the World in Sea Voyages and Lande Travells by Englishmen and Others.* (1625).

†\* A work by the Portuguese botanist Garcia d'Orta (c.1500-68), cited in the quotation on p. 396, epitomised by Clusius as *Aromatum et simplicium ... historia* (1567).

†\* Perhaps John Winthrop (1606-76), governor of Connecticut and virtuoso, who was in London in 1662 when he became a Fellow of the Royal Society. However, Connecticut hardly constitutes 'the Indies'. On the other hand, it cannot be the Francis Willoughby who was governor of Barbados, as suggested by the editors in Huygens, *Oeuvres complètes*, iv, 488n, since he was not an FRS.

†\* I.e., Selim II (1524-74), son of Soliman the Magnificent, whom he succeeded in 1566.

†\* This phenomenon was discussed at the Royal Society on 11 and 18 November 1663: see Birch, *Royal Society*, i, 329, 333. On Moray, see above, p. 187.

†\* Which of the sons of Willem Boreel this was is unclear. R. E. W. Maddison notes that Huygens reported seeing 'Boreel' (otherwise unidentified) at Gresham College in April 1661. He thinks this was Johan Boreel (1627-91), who was often in England and himself later served as ambassador to Charles II. We are inclined to agree. His brother Adam Boreel (1603-66), a theologian and New Testament scholar who wrote several works, also spent time in England, and BP 12, 13 and 15 comprise his unpublished treatise, *Jesus Nazareus Legislator*. See R.E.W. Maddison, 'Studies in the Life of Robert Boyle, 1: Robert Boyle and Some of his Foreign Visitors', *NRRS*, 9 (1951), 1-35, on p. 4.

†\* John Clayton; see p. 187n. MSS of this 'Short Account' exist at the Royal Society and in Leiden; see Introductory Note. The Royal Society version is endorsed 'Read before the Society Oct. 28 1663', but otherwise both versions lack the notes throughout the text and to the Postscript in the printed version, including the sub-title to the Postscript.

†\* Followed by 'before' in MSS.

†\* Followed by 'not' in MSS.

†\* Followed by 'that immediately' in MSS.

†\* Followed by 'far' in MSS.

†\* MSS have 'or', with 'either' after 'differing' four words earlier.

†\* MSS have 'neer hand' [?].

†\* Followed by 'white and well' in MSS.

†\* MSS have 'least'.

†\* Followed by 'though' in MSS.

†\* For the latter two components, see below, pp. 459ff., and 499ff., Boyle's examination of Aristotle's theory of cold, and of the views on cold of Thomas Hobbes (1588-1679). For Boyle's earlier controversy with Hobbes, see above, vol. 3, pp. xiff, 109ff.

†\* It has not proved possible to identify this figure, who is also referred to in *Notion of Nature* (1686), below, vol. 10.

†\* The *Origin of Forms and Qualities* was printed the following year, 1666: see below, vol. 5.

†\* Although Henry Oldenburg made a complete Latin translation that was at least partly printed, no copies are known to exist, and no part of his translation is known to have survived in manuscript. See Introductory Note, and M. B. Hall, 'What Happened to the Latin Edition of Boyle's *History of Cold?*', *NRRS*, 17 (1962), 32-5.

†\* Henry Oldenburg, see above, p. 12.

†\* Francis Bacon, 'Experiments in Consort touching the production of Cold', *Sylva Sylvarum* (1627), cent. i. 69-75.

†\* See above, General Introduction, p. xxxiii and below, vol. 13.

†\* The minutes of the Royal Society, as published in Birch, *Royal Society*, do not in fact contain a specific mandate to Boyle to publish his studies of cold. However, see the brief note prefixed to 'The Experimental History of Cold', below pp. 263-4.

†\* See, for instance Bacon, *Historia ventorum* (1622), in his *Works*, ed. James Spedding, R. L. Ellis, and D. D. Heath (14 vols., London, 1857-74), ii, 27.

†\* Probably a reference to Stanton St John, near Oxford, where Boyle spent part of his time at this point.

†\* This figure has not been identified.

†\* Duarte Lopes, *A Report of the Kingdom of Congo* (1597).

†\* This figure has not been identified. It seems unlikely that it was Henry Stubbe (1632-76), who was royal physician in Jamaica from 1662 to 1664.

†\* Gerrit de Veer, *The True and Perfect Description of Three Voyages ... by the Ships of Holland and Zeeland* (1598; Eng. trans., 1609) was reprinted in Samuel Purchas, *Hakluytus Posthumus: or, Purchas his Pilgrimes: Contayning a History of the World in Sea Voyages and Lande Travells by Englishmen and Others* (1625), pt. 2, bk. 3.

†\* James, *Voyage*.

†\* Except in two cases where authorial interference seems to be involved and notes have been included at the relevant point in the text, these have been silently corrected.

†\* For earlier theories of cold, see below, pp. 364ff.

†\* See Plate 1, fig. 1 [p. 228].

†\* 'idiosyncrasy'.

†\* See Plate 1, figs. 2-4 [p. 228].

†\* It is not clear to which of his writings Boyle here refers.

†\* See Plate 1, fig. 2 [p. 228].

†\* See above, p. 234.

†\* See Plate 1, fig. 3 [p. 228]. Boyle here refers to the thermometer brought from Italy by Sir Robert Southwell (1635-1702) and improved by Robert Hooke. See Introductory Note. The Florentine virtuosi were the Accademia del Cimento, for whose work on such subjects see W.E.K. Middleton, *The Experimenters* (Baltimore, 1971), pp. 166ff.

†\* Boyle does not in fact discuss this matter elsewhere.

†\* I.e., Thomas Hobbes, *De corpore* (1655), translated into English as *Elements of Philosophy, concerning Body* (1656), the text of which Boyle's next paragraph paraphrases.

†\* It is not clear to whom Boyle here refers.

†\* See Plate 1, fig. 5 [p. 228].

†\* See Plate 1, fig. 6 [p. 228].

†\* See Plate 1, fig. 1 [p. 228].

†\* William, 2nd Viscount Brouncker (1620-84), was first President of the Royal Society, in which he played a dominant role in its early years. His main area of interest was mathematics, and in 1653 he published a translation of Descartes' *Musicae compendium* with extensive notes.

†\* The square brackets are Boyle's.

†\* The square brackets are Boyle's.

†\* It is not clear to whom Boyle here refers.

†\* The square brackets on this page are Boyle's.

†\* On Samuel Collins, see above, p. 64n.

†\* See book 1 of Descartes' *Les météores*, one of the *Essaies* appended to his *Discours de la méthode* (1637). Boyle's earlier experiment in favour of this hypothesis is not otherwise known.

†\* I.e., Charles II, patron of the Royal Society.

†\* See further below, p. 361.

†\* Followed in 1st edition by 'it', a compositor's error, as Boyle pointed out in a letter to Oldenburg of 23 December 1665: *Correspondence*.

†\* These and the previous square brackets are Boyle's.

†\* The square brackets on this page are Boyle's.

†\* The square brackets on this and the previous page are Boyle's.

†\* The square brackets are Boyle's.

†\* James, *Voyage*.

†\* One of these figures is presumably Samuel Collins: see above, p. 64n. The physician referred to in the following section has not been identified.

†\* Dr Samuel Collins; see p. 64n.

†\* I.e., Gerrit de Veer, *Three Voyages*, and James, *Voyage*.

†\* See above, pp. 298-9.

†\* A measure of size is apparently missing here, as Oldenburg pointed out in a letter to Boyle of 3 December 1664: *Correspondence*. However, it is not clear what it should have been.

†\* All these square brackets are Boyle's.

†\* The square brackets on this and the next page are Boyle's.

†\* See below, pp. 412-13.

†\* For Hobbes' views, see below, p. 504.

†\* Altered from a full-stop in the 1st edition, as suggested in Boyle to Oldenburg, 23 December 1665: *Correspondence*.

†\* The original text had '4 inches and 2/9'. Following the errata, we have replaced this by 'four inches'.

†\* The square brackets here and on the previous and following page are Boyle's.

†\* Niccolò Zucchi and Melchior Cornaeus; see above, p. 301.

†\* The square brackets are Boyle's.

†\* Boyle here refers to *On Floating Bodies* by the Greek mathematician Archimedes (c.287-212 bc), and *De Beghinselen der Weeghconst* (1586) by the Flemish hydrostatician Simon Stevin (1548-1620). The square brackets around the previous paragraph are Boyle's.

†\* James, *Voyage*.

†\* de Veer, *Three Voyages*.

†\* Title IV of the present work, pp. 278ff.

†\* The 'Noble Virtuoso' in question has not been identified.

†\* The square brackets here and earlier on the page are Boyle's.



†\* Boyle later tried this experiment another way, which he intended as a substitute for this one. See below, p. 422.

†\* See above, p. 277n.

†\* See Gassendi, 'Physica', ii. ii, ch. 3, in his *Opera omnia* (1658), vol. 1.

†\* de Veer, *Three Voyages*.

†\* Presumably Dr Samuel Collins; see above, p. 64n. See also pp. 274-5, 299ff.

†\* Boyle here refers to the demonstration of 'Boyle's Law' in *Defence* (1662), above, vol. 3, pp. 57-61.

†\* Mersenne, *Cogitata physico-mathematica*, see above, p. 327n.

†\* See above, vol. 3, p. 59.

†\* The square brackets here and on p. 333 are Boyle's.

†\* Presumably Dr Samuel Collins; see above, p. 64n.

†\* The square brackets on this page are Boyle's.

†\* Presumably Dr Samuel Collins; see above, p. 64n.

†\* John Barclay, *Ioannis Barclaii Argenis* (1621); the first English edition was *Barclay his Argenis, or the Loves of Poliarchus and Argenis* (1625).

†\* Erasmus Bartholin, *De figura nivis dissertatio* (1661), bound with his brother Thomas' *De nivis usu* (see above, p. 277).

†\* The square brackets are Boyle's.

†\* The square brackets here and on p. 346 are Boyle's.

†\* Presumably Dr Samuel Collins; see above, p. 64n.

†\* The square brackets opened on p. 347 are Boyle's and should evidently close here.

†\* The square brackets on this page are Boyle's.

†\* 'Fulcrum'.

†\* Presumably Dr Samuel Collins; see above, p. 64n.

†\* Replacing 'cemented together by cold water' as instructed in the 'Errata', evidently an authorial change (see above, p. 225, though this wrongly gives the passage to be replaced as 'congealed by cold water').

†\* See above, pp. 320-1.

†\* Accounts of James Hall's voyages to Denmark and Greenland are found in Purchas, *Pilgrimes*, pt. 2, bk. 4.

†\* Boyle here fails to provide any clue about his apparently published source, which might well be Purchas.

†\* de Veer, *Three Voyages*.

†\* Virgil, *Georgics*, i. 236 (deriving from Eratosthenes): 'rigid with dull-blue ice and black rains'. The preferred MSS of Virgil, however, read 'caeruleae': 'dull-blue and rigid with ice and black rains'.

†\* Olaus Magnus, *Historia*, ch. 14.

†\* See above, p. 353.

†\* This figure, who is more than once cited in the course of the work, has not been identified.

†\* One of the brothers Bartholin, mentioned many times in this part of the work, see above, p. 342n.

†\* This demonstration appeared as experiment 14 in 'Heat and Cold' in *Mechanical Qualities* (1675), below, vol. 8. The liquor in question was oil of vitriol.

†\* The second plate, which is bound opposite this heading, accompanies the description given by John Evelyn (1620-1706), the well-known virtuoso who had made Boyle's acquaintance in the 1650s.

†\* S[tratum] S[uper] S[tratum], i.e., layer on layer.

†\* The square brackets are Boyle's.

†\* de Veer, *Three Voyages*.

†\* For Boyle's passage of the Alps, see *RBHF*, pp. 18-19.

†\* The dispute to which Boyle refers stemmed from the views of Aristotle as expressed in his *De generatione et corruptione*, 330a30-330b22. On recent theories concerning the role of nitre in this connection, see R. G. Frank, *Harvey and the Oxford Physiologists* (Berkeley and Los Angeles, 1980), pp. 117ff. A long appendix to this section is found at the end of this work, beginning on p. 591 below.

†\* Translated by Boyle on the following line as 'principal subject', to which an alternative might be 'primary substrate'.

†\* Probably a reference to *De primo frigido* by the Greek biographer and philosopher, Plutarch (c. ad 46-120).

†\* Evidently a reference to Gassendi's 'Physica' in his *Opera omnia* (1658). See further below, p. 377.

†\* See above, p. 366, and below, p. 377.

†\* Fournier, *Hydrographie*. See above, p. 251n.

†\* 'phenomenon'.

†\* James, *Voyage*.

†\* See above, p. 366, and below, p. 377.

†\* James, *Voyage*.

†\* See above, p. 366.

†\* Perhaps a sarcastic reference to the efforts of Walter Charleton (1620-1707), an English physician, to gain a hearing in England for the atomism of Gassendi by acting as if he were Gassendi's personal spokesman. His *Physiologia Epicuro-Gassendo-Charltoniana* (1654), is an amplified translation of Gassendi's *Animadversiones in decem librum Diogenis Laertii* (1649).

†\* These and the square brackets that follow are all Boyle's.

†\* A reference to John Philoponus (late 5th cent.-late 6th century ad), an Alexandrian philosopher who set out a major critique of Aristotle's physics and cosmology in works such as *In physica* (517); he also discussed the views of the Greek philosopher Democritus (c. 460-370 bc). Boyle's subsequent discussion on various views of the shape of 'frigorific corpuscles' is largely taken from Gassendi's discussion of the same topic in 'Physica', i. vi, ch. 6, in his *Opera omnia* (1658), vol. 1.

†\* As well as Gassendi (see above, pp. 366ff.), Boyle here cites the Italian Jesuit natural philosopher, Niccolo Cabeo (1585-1650), author of *Meteorologicorum Aristotelis commentaria* (1646).

†\* The square brackets are Boyle's and were presumably intended to close here.

†\* This figure has not been identified.

†\* Dr Samuel Collins; see above, p. 64n.

†\* This refers to Samuel Collins, which means that he must have been in Russia when the Russians took Smolensk from the Poles in 1654, six years before he became physician to the tsar. But the famous siege of Smolensk by the Poles occurred in 1611. Boyle may be confusing these two battles, or he may be confused about the source of this information. For a similar report from Adam Olearius, see below, p. 402.

†\* The square brackets are Boyle's and were presumably intended to close here.

†\* de Veer, *Three Voyages*.

†\* Martinus Martini, *Nova atlas Sinensis* (1654).

†\* Purchas, *Pilgrimes*.

†\* Olearius, *Relation du voyage*. See below, p. 402n.

†\* Genesis 31, 40.

†\* James, *Voyage*.

†\* For Cartesian views on cold, see above, p. 277. For Prosper Alpinus, see above, p.394.

†\* See, e.g., below, p. 403.

†\* See above, p. 387n.

†\* See above, p. 395n.

†\* The square brackets are Boyle's. For Dr Samuel Collins, see above, p. 64n.

†\* Thomas Bartholin, *De nivis usu*, quoting the Danish explorer Jens Munk, *Navigatio septentrionalis* (1624).

†\* Apparently also from Fletcher's account in Purchas, *Pilgrimes*.

†\* This figure has not been identified.

†\* 'Sieur de Beauplan', i.e. Guillaume Levasseur. Boyle probably cites the 2nd (enlarged) edition, *Description d'Ucranie, qui sont plusieurs Provinces du Royaume de Pologne* (1660). See Loewenson, 'The Works of Boyle and "The Present State of Russia"' (above, p. 64n), p. 483 n. 53. The 'Polonian Lord' is possibly the figure referred to in para. 22, above.

†\* See below, pp. 427-8.

†\* For Boyle's travels in Savoy, see *RBHF*, p. 17.

†\* See above, p. 380.

†\* The square brackets are Boyle's.

†\* These square brackets, and those around the first part of para. 9, are Boyle's.

†\* A phrase denoting the air-tight sealing of a vessel.

†\* The square brackets at the start of this paragraph are Boyle's; they were presumably intended to close here.

†\* Santorio Santorio (1561-1636), professor of theoretical medicine at Padua. His *De medicina statica libri octo* (1614), was a work for which Boyle had considerable respect.

†\* Clearly this is erroneous: it was the ninth experiment in section XI, not the tenth, that Boyle intended to replace with this one. See above, p. 328.

†\* See above, pp. 364ff.

†\* Bartholin, *De nivis usu*, citing *Meteorologicorum Aristotelis commentaria* (1646) by Niccolo Cabeo: see above, p. 383. For Gassendi, see above, pp. 366ff.



†\* That is, the section this appendix accompanies: see above, pp. 364ff.

†\* Purchas, *Pilgrimes*, pt. 2, bk. 4. See above, p. 353.

†\* A discussion of frozen birds flying again is to be found in the 'Strange Reports' appended to *Experimenta et Observationes Physicæ* (1691): see below, vol. 11. Of the figures mentioned in the subsequent section of the text, the second could be the one referred to on pp. 406-7, above.

†\* Aristotle, *Meteorologica*, I.12.348b30. On Cabeo and Bartholin, see above, p. 423.

†\* *Circulus pisanus ... de veteri et peripatetica philosophia* (1643; enlarged edition 1661), by the French natural philosopher Claude Guillermet de Bèrigard (1578-1663).

†\* Substituted for 'leaving' in the 1st edition, as instructed by Boyle in his letter to Oldenburg of 23 December 1665: *Correspondence*.

†\* 'Unless they cut air holes in the ice, all that there is in the river or pool will at once die.'

†\* Dr Samuel Collins; see above, p. 64n.

†\* Bartholin, *De nivis usu*.

†\* This figure has not been identified.

†\* Through the French caballist Jacques Gaffarel (1601-81), Boyle evidently refers to Étienne de Clave, author of the work cited above in p. 367n. The surviving draft of this paragraph (BP 10, fols. 157-8) offers no further information.

†\* Josephus Quercetanus, or Joseph Duchesne (1544-1609), a prolific French Paracelsian. It is not clear whom Boyle means by 'the notedst Chymist in *England*' in the next sentence.

†\* Bèrigard, *Circulus pisanus* (see above, p. 433), p. 573. 'I should like to demonstrate, by means of a remarkable experiment, how penetrative certain corporeal spirits are. Write some letters on a sheet of paper in white vinegar, so that no trace of them is visible, and shut the paper up inside the first pages of some hefty tome. Then take another piece of paper, soak it in that stinking water in which orpiment has been dissolved, and when it is dry place it among the last pages of the same book, lightly compressed. You will immediately see conspicuous letters on the former sheet of paper, just as if they had been drawn from the ink.' As Boyle notes in the following sentence, there are some critical omissions in this recipe. For a complete version, see below, pp. 515-16.

†\* Here MS (BP 10, fol. 158) has an unpublished paragraph:

Wee have somtimes thought fit to try whether Followed by 'spi', deleted. †1 or noe Followed by 'well sented' deleted. †2 spirituous & strongly sented liquors being turn'd into Ice that Ice would have all its parts soe lock'd up as to hinder the Followed by 'Diffusion', deleted. †3 Avolation of the odoriferous steames[.] But we were not satisfy'd by the few tryalls we made that <the> Cold of this Climate would quite imprison or suppress Odors or <quite> hinder Ice it selfe from affecting the Nostrills[.] But we would gladly have had the Experiment very watchfully tryd in colder countrey's both because the sharper Cold may there keep in the oderiferous Effluvia better & because alsoe we had a Jelousy that in an Aire noe colder then ours the very approch of the face & the steames issueing out of the Nose might Followed by 'resolve', deleted. †4 dissolve some of the superficial

parts of the Ice or relax the Pores of it & soe both free & excite the Corpuscles of smell that were imprison'd there.

†\* This does not appear to be recorded in the minutes as published in Birch, *Royal Society*. It is also unclear which of his other writings Boyle refers to later in this sentence.

†\* See above, p. 337.

†\* Clearly Boyle meant section VI, not XVI. See above, p. 289.

†\* See above, p. 400. The '*American Physician*' has not been identified.

†\* See above pp. 231-3.

†\* See above, p. 395. The 'learned Physician' has not been identified.

†\* 'The History of Fluidity and Firmnesse' formed part of *Certain Physiological Essays* (1661): see above, vol. 2, pp. 115ff.

†\* It is not clear to what authors Boyle here refers.

† See *Spring of the Air* (1660), above, vol. 1, p. 210.

†\* Substituted for '3/8' in the 1st edition, along with 'above' for 'almost' later in the sentence, as instructed by Boyle in his letter to Oldenburg of 23 December 1665: *Correspondence*.

†\* See note on previous page.

†\* The phenomenon of antiperistasis is the intensification of a quality by sudden contact with its contrary. See Aristotle, *Meteorologica*, I.12.348a-349a.

†\* The interlocutors of this dialogue are the same as those of Boyle's *Sceptical Chymist*: see above, vol. 2, p. 207.

†\* Samuel Collins, see above, p. 64n.

†\* For this experiment by Aristotle, see *Meteorologica*, I.12.348b31-3.

†\* 'Now courage is needed, Aeneas, now a stout heart.' Virgil, *Aeneid*, vi. 261.

†\* 'Stomachs are warmer in winter'. Hippocrates, *Aphorisms*, i. 15.

†\* Santorio Santorio, *De medicina statica libri octo* (1614). See above, p. 421.

†\* A reference to a work of Libertus Fromondus, possibly his *Meteorologicorum libri sex* (1627).

†\* 'bulimia'.

†\* Prosper Alpinus, *De medicina Ægyptiorum libri quatuor* (1591), of which there were many subsequent editions. See above, p. 394n.

- †\* Bartholin, *De nivis usu*. The virtuoso referred to in the previous sentence has not been identified.
- †\* Dr Samuel Collins; see above, p. 64n. The 'learned *Polander*' could be the same figure referred to on p. 407.
- †\* The square bracket is Boyle's. The opening bracket was accidentally omitted, and it is not clear where it should be placed.
- †\* I.e., J. B. Morin: see the above author's note, and above, p. 367.
- †\* Lucretius, *De rerum natura*, vi. 848; cf. Ovid, *Metamorphoses*, xv. 309, Arrian, *Anabasis*, iii. 4.2, Quintus Curtius Rufus iv. 7. 22, Pliny, *Historia naturalis*, v. 5. 36.
- †\* François Pyrard, *Voyage de F. Pyrard de Laval. Contenant sa navigation aux Indes Orientales, Maldives ...* (1619); an abridged translation was printed as *The Voyage of F. Pyard to the East Indies* in Purchas, *Pilgrimes*, pt 2.
- †\* The reference is obscure. However, see above, p. 410n. It is not clear whether the reference to the observatory of the astronomer, Tycho Brahe (1546-1601), on the island of Hveen was found in Boyle's source or was added by him.
- †\* On Morin, see above, p. 367.
- †\* This figure has not been identified.
- †\* Boyle's informant must have been his brother, Roger Boyle, Baron Broghill (1621-79). The reference to the ships as 'royal' suggests that the event took place in the 1640s, but it is apparently not otherwise recorded.
- †\* Neither the ditch nor the grotto have been identified. The latter cannot be the cave referred to in *Usefulness*, above, vol. 3, p. 420] The identity of Boyle's informants concerning the former is also unclear.
- †\* Dr Samuel Collins; see above, p. 64n. The square brackets are Boyle's.
- †\* Martinus Martini, *Nova atlas Sinensis* (1654).
- †\* Sir Thomas Smith (1558?-1625), the first governor of the East India Company and grandson of a founder of the Muscovy Company, acted as special ambassador to the tsar in 1604-5. On his return he published *Sir Thomas Smith's Voiage and Entertainment in Rushia* (1605), which was excerpted in Purchas, *Pilgrimes*, pt. 2, bk. 4.
- †\* Dr Samuel Collins; see above, p. 64n.
- †\* Literally, 'flight from a contrary'.
- †\* A reference to Niccolò Zucchi. See above, p. 301n.
- †\* The reference is obscure.
- †\* On Morin and the Polish nobleman, see above, pp. 367, 407.

†\* *Cursus philosophicus* (1653), by the French natural philosopher, Emanuel Maignan (1601?-76). On Olaus Magnus and Martini, see above, pp. 64, 234.

†\* See above, p. 491.

†\* Emanuel Maignan, *Perspectiva horaria, sive de horographia gnomonica, tum theoretica, tum practica, libri quatuor* (1648), a treatise on sundials and optics.

†\* *New Experiments, Physico-Mechanicall, touching the Spring of the Air* (1660): see above, vol. 1, pp. 201-5. Boyle cites the same experiment on p. 235 above.

†\* As will shortly be clear, Boyle wrote this work in answer to certain arguments in Thomas Hobbes' *De corpore* (1655), translated into English as *Elements of Philosophy, concerning Body* (1656): see also above, pp. 255ff.

†\* Boyle refers to the plate ('Scheme') that accompanied the section from Hobbes that he prints as paragraph 4 on pp. 504-5.

†\* Boyle noted that the printer had accidentally omitted the words 'not only' at this point in a letter to Oldenburg of 8 July 1665: *Correspondence*.

†\* The square bracket at the beginning of this paragraph is Boyle's; the closing bracket was accidentally omitted.

†\* This figure has not been identified.

†\* See above, p. 450.

†\* A reference to *Colours*, part two, which carried the title 'Experiment[s] in Consort, touching Whiteness & Blackness': see above, pp. 61ff. On p. 26 of that work, Boyle called this essay 'as well Speculative as Historical'.

†\* This catalogue was printed here in the 1st edition. We have followed Fulton, *Bibliography*, p. 51, in assigning it pp. 846-7. In the 2nd edition, it was replaced by a different catalogue (see above, Introductory Notes); this is generally found before p. 267 of that edition, although in some copies its location is different.

†\* Boyle wrote this preface to the following tract by Merrett, on whom see above, p. 177n.

†\* Merrett's account of his experiments on freezing were read at the meeting of the Society on 30 December 1663 (Birch, *Royal Society*, i, 350-64).

†\* I.e., where the Royal Society met.

†\* Below, p. 539: in both original editions of *Cold*, Merrett's tract was paginated separately from Boyle's work.

†\* I.e., the 'repository' of the Royal Society. Thomas Henshaw (1618-1700), a virtuoso who played a prominent role in the early Royal Society, presented a series of experiments on freezing at the Society's meetings on 3 and 10 December 1662 (Birch, *Royal Society*, i, 132, 139-41): however, his queries do not appear to survive.



†\* Bartholin, *De nivis usu*.

†\* 'phenomena'.

†\* I.e. Boyle, whose air-pump is referred to in the next paragraph: see above, p. 521.

†\* 'phenomenon'.

†\* 'phenomena'.

†\* A major blood vessel near the heart.

†\* Robert Hooke, *Micrographia* (London, 1665), scheme VIII, figure 1, facing p. 88. At this point Hooke was curator of experiments to the Royal Society, under whose auspices the book was published.

†\* Josephus Quercetanus, see above, p. 449n. Boyle there cited Quercetan's view that the seminal virtues of burned plants survive in the ashes, the view that Merrett puts to the test here.

†\* A phrase denoting the air-tight sealing of a vessel.

†\* 'piercing cold', Virgil, *Georgics*, i. 93.

†\* This is changed in the 2nd edition to 'Mr. Hook', clearly in error. Merrett undoubtedly meant to refer to the German savant Theodore Haak (1605-90), a charter member of the Royal Society. See below, pp. 540ff., where Merrett says more about frozen solutions of soot, and where he unambiguously credits this work to Haak, although the 2nd edition again has 'Hook'. On Haak, see Pamela R. Barnett, *Theodore Haak, F.R.S. (1605-90)* (s-Gravenhage, 1962).

†\* A puzzling statement, as no plates were bound with Merrett's tract in either edition of *Cold*, and Haak never published his own account of this work.

†\* Wilhelm Fabricius von Hilden: see above, p. 292n.

†\* See above, p. 64n.

†\* Bartholin, *De nivis usu*.

†\* See *Micrographia* (above, p. 531), p. 220, where Hooke relates his finding that light is refracted more by water than by ice.

†\* 'phenomena'.

†\* On Descartes' *Dioptrique*, see above, p. 57.

†\* Bartholin, *De nivis usu*. Birch places inverted commas around the italicized passages in the following paragraphs, which may cause readers to take them for quotations from Bartholin, whereas Merrett paraphrases Bartholin in roman type, adding his own comments in italics.

†\* It is not clear why Merrett refers to 'the same Authors' in the plural, when he has cited a single work. It may be a typographical error, or it may reflect the fact that Erasmus Bartholin's little tract

*De figura nivis dissertatio* was bound with his brother's longer work *De nivis usu*.

†\* The shop of the bookseller Octavian Pulleyn was at the Sign of the Rose in St Paul's Churchyard. Although made at the behest of the Royal Society, these experiments do not appear to have been reported back to it.

†\* See above, p. 534n.

†\* Apparently a reference to John Barclay's poem *Argenis, or the Loves of Poliarchus and Argenis*: see above, p. 341.

†\* This matter is dealt with by the German physician and natural philosopher, Daniel Sennert (1572-1637), in his *Hypomnemata physica* (1636).

†\* See above, pp. 415ff.

†\* Francis Bacon, *Sylva sylvarum* (1627).

†\* This preface was added to the 2nd edition of 1683. It was clearly intended to precede the 'Appendix' that follows, which was itself added to the 2nd edition. However, in many copies of the work, it is found bound elsewhere, often following the title-page to the book as a whole; it is here that Birch prints it in his edition. Both this preface and the 'Appendix' are separately paginated from the remainder of the work.

†\* Boyle became a member in 1675.

†\* Samuel Collins: see above, p. 64n.

†\* This figure has not been identified.

†\* William Drummond (1617?-88), 1st Viscount Strathallan, served in the army of Tsar Alexis Romanov from 1655-65 with permission from Charles II (then in exile).

†\* Jean-Baptiste Du Hamel, *De corporum affectionibus tum manifestis tum occultis libri duo* (1670). The first secretary of the Académie Royale des Sciences, Du Hamel (1623-1706) made contacts with English intellectuals when he visited England on a diplomatic mission in the late 1660s. 'Last year I came upon the *History of Cold* by the illustrious and learned Mr Boyle, in which he has dealt thoroughly with a subject almost untouched by Philosophers, with such care and erudition that one could hardly hope for anything of greater accuracy.'

†\* Israel Conradt, *Dissertatio medico-physica de frigoris natura et effectibus* (1677). A letter from Conradt to Boyle of April 1672 outlining his work on cold is printed in *Correspondence*. 'The last of this learned band is Mr Boyle, the delight and ornament to our age, to whom all our attention is now rightly due. Indeed, I have left my discussion of him to last especially so that, having made a brief and unfettered digression, and to some extent set aside the list of headings given above, I might without weariness or artful brevity subject to further investigation both what has already been partly dealt with and what still remains to be said, under the leadership (so to speak) of the man I have taken as my guide for this whole treacherous and icy path.'

†\* 'He who has truly appreciated Cicero may be sure he has progressed far': Quintilian, *Institutio Oratoria*, x 1.112, substituting *placuerit* for *placebit* in the original.

†\* Probably Boyle's servant and amenuensis John Warr (d. 1715), whom Boyle named as executor in his will.

†\* The square brackets are in the original.

†\* On Drummond, see above, p. 547n. In this 'Appendix' (apart from the preface, which Boyle did not write), his name is always spelt 'Drummon'; we have added the final letter in each case.

†\* The surgeon has not been identified. The extraordinary ambassador was Charles Howard, Earl of Carlisle. See above, p. 496.

†\* Dr Samuel Collins, whose time in Russia overlapped with Howard's. See above, p. 64n. A surviving MS (BP 28, fols. 259-63) which contains these two entries also contains several others from this 'Appendix', suggesting that all may have come from notes on a conversation or correspondence with Collins. See Introductory Note.

†\* This entry is found in the same MS mentioned in the previous note, which suggests that Dr Samuel Collins was the source.

†\* This entry and the next are found in the same MS mentioned on p. 551n., again suggesting that Dr Samuel Collins was the source.

†\* This must be the unidentified captain from the Hudson's Bay Company mentioned on p. 547.

†\* Presumably Samuel Collins, who is credited with similar reports on pp. 274-5 and 299ff. of the main work.

†\* William Drummond. See p. 547n.

†\* A copy survives in the Society's Register Book, and it is printed in Birch, *Royal Society*, ii, 492-5. See above, Introductory Note. The square brackets are in the original.

†\* I.e. members of the Accademia del Cimento: for their experiments on such topics, see above, p. 246.

†\* These experiments were apparently not published.

†\* So in MS: the printed text has 'dissolutions'.

†\* 'hold back'.

†\* Samuel Collins: see p. 64n.

†\* 'antiperistasis'.

†\* Samuel Collins: see p. 64n.

†\* This paragraph, and those numbered 12 and 13 which follow, are found in the MS mentioned in p. 551n, suggesting that Dr Samuel Collins was the source of all the information in items 10 to 13 here.

†\* A reference to Adam Olearius, *Relation du voyage de Moscovie, Tartarie, et de Perse* (1656), a work often cited by Boyle in the main body of this work.

†\* Peter Julius Coyet (1618-67) served as secretary of a Swedish embassy to Moscow in 1647. In 1666 he came to England with his son, Wilhelm Julius Coyet (1647-1709), who became MA at Oxford the next year. See Loewenson, 'The Works of Boyle and "The Present State of Russia"'(above, p. 64n.), p. 482 n. 48.

†\* Martinus Martini, *Novus atlas Sinensis* (1654).

†\* *De origine et rebus gestis Polonorum libri xxx* (1558) by Marcin Kromer, Bishop of Ermeland.

†\* Presumably Samuel Collins: see above, p. 64n.

†\* These two paragraphs, and the first two in the next section, are found in the MS mentioned in p. 551n, which suggests that Samuel Collins was the source of all the information in sections 10 to 12 here.

†\* Olearius, *Relation du voyage*.

†\* In the text of the Appendix, we have silently corrected one error noted in the list of Errata to the 2nd edition as a whole which appears on sig. D2v of that edition: on p. 1, l. 5, 'of them' has been deleted after 'the top'.

## Notes to Editor's Notes

†1 Followed by 'spi', deleted.

†2 Followed by 'well sented' deleted.

†3 Followed by 'Diffusion', deleted.

†4 Followed by 'resolve', deleted.