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Robert Boyle's mechanical account of hydrostatics and pneumatics: fluidity, the spring of the air and their relationship to the concept of pressure

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Abstract This article in an attempt to identify the precise way in which Robert Boyle provided a mechanical account of the features that distinguish liquids and air from solids and from each other. In his pneumatics, Boyle articulated his notion of the 'spring' of the air for that purpose. Pressure appeared there only in a common, rather than in a technical, sense. It was when he turned to hydrostatics that Boyle found the need to introduce a technical sense of pressure to capture the fluidity of water which, unlike air, lacked a significant spring. Pressure, understood as representing the state of a liquid within the body of it rather than at its surface, enabled Boyle to trace the transmission of hydrostatic forces through liquids and thereby give a *mechanical* account of that transmission according to his understanding of the term. This was a major step towards the technical sense of pressure that was to be adopted in Newton's hydrostatics and in fluid mechanics thereafter.

1 Introduction

The development of the sciences of hydrostatics and pneumatics in the seventeenth century is typically discussed, for example by Pierre Duhem, in terms of the introduction of the concept of pressure.[1] Considerations of pressure loom large in the more recent, and detailed, engagement with Boyle's pneumatics by Shapin and Schaf-

[1] See Duhem (1905).

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fer (1985). From the point of view of the position argued in this paper, focus on the concept of pressure can be a misleading and inappropriate way of understanding developments in seventeenth-century hydrostatics and pneumatics. Moreover, those that have adopted such a focus have given insufficient attention to the precise import of the term 'pressure' and the changes it underwent in the seventeenth century and thereafter. The uncritical assumption that seventeenth-century figures such as Boyle employed the term 'pressure' in something like its modern sense has blinded historians to many of the subtleties involved in the evolution of a theorisation of the features that distinguish liquids and air from solids and from each other that gradually emerged in the period that separated the relevant work of Simon Stevin and Isaac Newton.

Boyle built on the work of his predecessors to articulate technical notions of the 'spring of the air' and fluidity. The term 'pressure' was used extensively by Boyle to be sure, but his early uses of it employed a common sense of that term rather than a technical sense. As far as his pneumatics was concerned, his notion of spring gave Boyle what was required from a technical point of view. He gave serious attention to hydrostatics, as distinct from pneumatics, only after his reading of Blaise Pascal's treatise *The Equilibrium of Liquids* in 1664. Recognising that liquids do not possess a significant 'spring' Boyle came to realise that hydrostatics required a precise characterisation of 'fluidity' to distinguish liquids from solids. It was that task that took Boyle some way beyond the common sense notion of pressure to a more technical one.

There was a productive interplay between developments in hydrostatics and pneumatics that is brought out in the following study. When Stevin wrote his *Elements of Hydrostatics* in 1586, he was able to take a sophisticated and mathematically formulated science of weight as his model of a theorised science. Since liquids and solids alike possess weight, a theory of liquids as distinct from solids required that liquids be characterised in a way that pinpointed a feature or features of them distinct from weight. This was a step that Stevin did not succeed in taking.[2] It was the behaviour of air, rather than liquids such as water, that most directly signalled the need to transcend weight. The resistance of a syringe to the motion of the plunger and the expansion of an inflated bladder as air is forced into it do not lend themselves to an explanation that appeals only to weight. Following Evangelista Torricelli's experiment demonstrating the support of a mercury column by the atmosphere, there were anticipations of Boyle's attempt to characterise the nature of air by adding spring to weight. In the light of such developments, Pascal came to realise that liquids needed to be theorised in a way that distinguished them from both air and solids, focusing on the continuity and fluidity of the former for that purpose. This theoretical innovation, spelt out in his *Equilibrium of Liquids*, was not matched by the companion treatise, *On the Weight of the Mass of the Air*, which did not move significantly beyond considerations of weight and fluidity insofar as it lacked a concept playing the role of Boyle's 'spring'. Boyle's attempts to give a theoretical account of both pneumatics and hydrostatics, as distinct from each other and the science of weight, are the main focus of concern in this paper.

I find it useful to characterise the causes involved in Boyle's hydrostatics and pneumatics, such as weight, pressure and the spring of the air, together with the

[2] I have analysed Stevin's hydrostatics from this point of view in Chalmers (2015).

explanations provided by invoking them, as 'intermediate' causes and explanations. Boyle himself occasionally used that term to capture the sense in which such causes differed from the ultimate causes of material phenomena which seventeenth-century mechanical philosophers located in the shapes, sizes and motions of corpuscles of universal matter.[3] Intermediate causes could be identified and explored by experiment and could serve their explanatory function whether or not they themselves could be explained at a deeper level. As we shall see, Boyle insisted that his pneumatics, for example, no more required that the spring of the air be explained than the ancient science of simple machines such as the balance required that weight be explained.

In the following section, I discuss more fully the sense of 'mechanical' involved in Boyle's quest to identify the intermediate causes of hydrostatic and pneumatic phenomena. I prepare the way for my account in Sect. 4 of Boyle's introduction of the concept of 'spring' into pneumatics by surveying, in Sect. 3, the work of his predecessors on which he could draw. Section 5 contrasts the modern concept of pressure with a common sense of that term which dates from antiquity. This makes it possible for me to critically evaluate, in Sect. 6, where to situate Boyle's usage of 'pressure' in his pneumatics in the spectrum between the two extremes. I argue that Boyle's pneumatics involved and required no significant move beyond the common sense of pressure. This no longer remained the case once Boyle turned his attention to hydrostatics. Section 7 captures the way in which Boyle moved beyond the common sense of pressure to give an account of how pressure is transmitted from point to point through the body of liquids. Boyle's theoretical innovations complemented those made by Pascal in ways described in Sect. 8. The main thrust and significance of my analysis is summarised in the concluding section.

2 Boyle on mechanism and intermediate causes

Boyle made one of his most forthright expressions of the sense in which he regarded his accounts of hydrostatics and pneumatics to be 'mechanical' in *An Hydrostatical Discourse* composed in 1672 as a response to criticism from Henry More. Defending his appeals to the weight and spring of the air in his pneumatics, Boyle wrote:

> And since such kinds of explanations have been of late generally called mechanical, in respect of their being grounded upon the laws of the mechanics, I, that do not use to contend about names, suffer them quietly to be so: And to enti-

[3] Boyle used the term 'intermediate' causes in 'A Proemial Essay' (1661) which was an introduction to *Certain Physiological Essays* which reported on some of Boyle's early experimentation other than that in pneumatics. It is reproduced in *The Works of* Boyle (1999), Ed. Hunter and Davis, Volume II, pp. 9–34. (Hereafter, I identify sources in this collection simply as *Works* followed by a roman numeral representing the volume and the page number, e.g., *Works* II, p. 23.) Boyle characterised intermediate causes more fully in a manuscript fragment dealing with final causes and the uses of experiment, dating from around 1688 and reproduced in Boyle (1990), Vol. IX, f40-41, reel 5, frame 250. 'Of the subordinate or intermediate causes or theories of natural things, there may be many: some more and some less remote from the First Principles and yet each of them capable to afford a just delight and useful instruction to the mind. And these we may call the cosmographical, the hydrostatical, the anatomical, the magnetical, the chemical and other causes or reasons of phenomena as those which are more immediate (in our way of estimating things) than the general and primordial causes of natural effects'. See also Boyle (1990), Vol. VIII, f184, reel 5, frame 189.

> tle my now examined explication to be mechanical, in the usual sense of that expression, I am not obliged to treat the cause of gravity in general; since many propositions of Archimedes, *Stevinus* and those others that have written of statics, are confessed to be mathematically or mechanically demonstrated, though those authors do not take upon them to assign the true cause of gravity but take it for granted, as a thing universally acknowledged, that there is such a quality in bodies that they treat of. And if in each of the scales of an ordinary and just balance, a pound weight, for instance, be put; he that shall say, that the scales hang still in equilibrium because the equal weights counterpoise one another; and in case an ounce be put into one of the scales, and not in the opposite; he that shall say that the loaded scale is depressed, because it is urged by a greater weight than the other, will be thought to have given a mechanical explanation of the equilibrium of the scales, and their losing it; though he cannot give a true cause, why either of those scales tends towards the centre of the earth.[4]

Boyle is here making explicit what was implicit in the acceptance and deployment of the science of simple machines, dating from the works of pseudo-Aristotle and Archimedes and developed with increasing sophistication up until the dawn of the scientific revolution.[5] The science of weight, involving the transmission of forces via contact action, was explanatory and useful and regarded as such in spite of the fact that gravity or weight and, one might add, the rigidity of balance arms and the near constant length of pulley strings were appealed to as causes which themselves remained unexplained. In the remainder of the article, I use the term 'mechanical' in the sense outlined here and referred to by Boyle as 'the usual sense of the word'.[6]

Boyle insisted that there is merit in knowledge that explains by appealing to unexplained intermediate causes.

> And though it must not be denied that it is an advantage as well as a satisfaction to know in general how the qualities of things are deducible from the primitive affections of the smallest parts of matter, yet whether we know that or no, if we know the qualities of this or that body they compose, and how 'tis disposed to work on other bodies or be brought on by them, we may without ascending to the top of the series of causes perform things of great moment and such as without the diligent examination of particular bodies would, I fear, never have been found out a priori even by the most profound contemplators.[7]

Knowledge of intermediate causes is to be found by 'diligent examination of particular bodies' that is by experimenting, rather than by a priori contemplation. Such

[4] *Works* VII, p. 148.

[5] For an appreciation of the degree of sophistication reached by developments in the science of weight prior to the seventeenth century, see Renn and Peter (2012).

[6] As I have stressed elsewhere, this use of mechanical 'in the usual sense of the word' differs from the sense involved in 'the mechanical philosophy' characterised by the insistence that phenomena be traced back to the interaction of corpuscles of universal matter characterised solely in terms of their shape, size and motion. The distinction between mechanism in the strict, philosophical, sense and in a more common sense is spelt out in Chalmers (1993, 2002, 2009, 2012).

[7] *Works* II, p. 24.

knowledge, which is not otherwise available, is of great moment and not to be despised since it is explanatory.

The standpoint adopted by Boyle and described in the foregoing is clearly implicit in *Spring of the Air* (1661), where Boyle insists that his business 'is not—to assign the adequate cause of the spring of the air but only to manifest, that the air has a spring, and to relate some of its effects'.[8] When, 5 years later, Boyle turned his attention to hydrostatics, he insisted that that field qualified as 'philosophy'.

> Hydrostatics is a part of philosophy, which I confess I look upon as one of the ingeniousest [*sic.*] doctrines that belong to it. Theorems and problems of the art, being most of them pure and handsome productions of reason duly exercised on attentively considered subjects, and making in them such discoveries as are not only pleasing, but diverse of them surprising, and such as would make one at first wonder by what kind of ratiocination men came to attain the knowledge of such unobvious truths. — For there are many things, as well as the more familiar, as of the more abstruse phenomena of nature that will never be understood by those that are strangers to the hydrostatics.[9]

This passage appears early in a work that proceeds to describe phenomena explored by *experiment* and explained by appeal to the weight and fluidity of water. Here Boyle uses the term 'philosophy' in a way that later generations were to use the term 'science'. If philosophical knowledge is distinguished from that of artisans by the fact that the former, unlike the latter, identifies causes which facilitate explanations of phenomena, then Boyle can insist that hydrostatics is to be deemed part of philosophy, in spite of the fact that the causes and explanations involved are intermediate rather than ultimate ones.

In modern terminology, one can summarise Boyle's stance in the following way. In order to convert pneumatics and hydrostatics into sciences on a par with the science of weight, it was necessary to move beyond weight, which solids, liquid and air share in common, by developing theoretical characterisations of the ways in which liquids and air differ from solids and from each other. It was the (intermediate) concepts of spring, fluidity and pressure that made it possible to identify the pushes involved in the transmission of forces through liquids and air by contact action, thereby affording *mechanical* explanations of hydrostatic and pneumatic phenomena.

The foregoing remarks help us to appreciate Boyle's dissatisfaction with the treatise on hydrostatics compiled by Simon Stevin in 1586.[10] That work was modelled on the mathematical treatises of Euclid and Archimedes and presented hydrostatics as a body of theorems derivable from postulates sufficiently unproblematic to be granted at the outset.[11] Boyle was prepared to accept that Stevin had demonstrated that the

[8] *Works* I, p. 166.

[9] *Works* V, p. 194.

[10] *The Elements of Hydrostatics* in Stevin (1955), pp. 393–483.

[11] As is argued in Chalmers (2015), the mechanics of simple machines on which Stevin modelled his hydrostatics could be presented in Euclidean guise only to the extent that common sense and familiar technologies could yield propositions about weight that could be regarded as evident. The move to pneumatics, and to a lesser extent hydrostatics, involved a move beyond that familiar world to one revealed by way

propositions constituting his hydrostatics were true but complained that he had not shown why they were true.[12] He complained that mathematicians such as Stevin were wont to handle hydrostatics 'rather as geometricians, than as philosophers, and without referring them to the explication of the phenomena of nature'.[13] The kinds of explication that Boyle sought were *mechanical* ones in the sense I have here identified. To provide them in his hydrostatics, and also his pneumatics, Boyle needed to trace the way in which the relevant effects were related to their causes by way of contact action. This quest eventually led Boyle to formulate a technical sense of pressure that went beyond the common sense.

Unlike his predecessor Stevin, Boyle was not particularly concerned to express his accounts of pneumatics and hydrostatics mathematically. Rather he aimed to identify the intermediate causes of the phenomena and to show how they functioned mechanically rather than mathematically. By doing so, he aimed to provide mechanical explanations of hydrostatics and pneumatics that could be illustrated and vindicated by a range of experiments.[14] This is the point of view I bring to bear on my analysis of Boyle's treatment of spring, fluidity and pressure.

3 Anticipations of Boyle's 'spring of the air'

Boyle's expressed aim in *New Experiments Physico-mechanical Touching the Spring of the Air and Its Effects* (1661), his first and major work on pneumatics, was 'to manifest that air has a spring, and to relate some of its effects'.[15] The details of Boyle's case required the recognition that the spring of air is isotropic and is the greater the more the air is compressed. They also required the recognition that air has weight. None of these points were novel at the time that Boyle embarked on his experimental programme. In this section, I survey some of the accomplishments of the preceding decade or two on which Boyle was able to build.[16]

Key ideas lying behind Boyle's treatment of air and its weight and spring were anticipated by Torricelli, in 1644, at the time of his experiment involving what has become known as the barometer. These were expressed in an interchange of letters between Torricelli, in Florence, and Michaelangelo Ricci, in Rome. Ricci sent extracts

Footnote 11 continued
of experiment. As a consequence, novel notions other than weight needed to be fashioned and the adequacy of various claims made with their aid needed to be established, not by appeal to their unproblematic character but by appeal to their explanatory power and the extent to which their use was illustrated and supported by experiment. Hence, Boyle's reference in the passage in the main text to 'surprising' discoveries, and 'unobvious truths' revealed by 'diligent examination of particular bodies' and requiring for their comprehension novel concepts that are 'handsome productions of reason'.

12 *Works* II, pp. 207 and 236.

13 *Works* II, p. 195.

14 In the full title of *Spring of the Air* and elsewhere, Boyle referred to his pneumatics as 'physico-mechanical'. I suspect that by doing so he was drawing a contrast between his mechanical exposition and works with a more mathematical emphasis which were being referred to as 'physico-mathematical', as exemplified in the title of Mersenne (1644).

15 *Works* I, p. 66.

16 My account of the background to Boyle's introduction of 'spring' owes a debt to Webster (1965).

of the letters to Mersenne in Paris, and their contents became widely distributed and well known, especially in France. In his first letter, of June 11, 1644, Torricelli invoked the weight of atmospheric air as the support of the column of mercury in the barometer. We 'live submerged at the bottom of an ocean of the element air, which by unquestioned experiments is known to have weight'. The column of mercury does not fall because 'on the surface of the liquid which is in the basin, there gravitates a mass of air fifty miles high'.[17]

In his response, Ricci raised two objections to Torricelli's appeal to the weight of the air. One of them involved the observation that the mercury in the barometer tube does not fall if the mercury in the outer dish is insulated from the weight of the air by a metal cap, giving a reason to doubt that the weight of air is indeed responsible for supporting the mercury column. In the main part of his reply, Torricelli assumed that a layer of air remains between the metal cap and the upper surface of the mercury in the outer dish. He argued that that air, which before the placement of the metal cap is compressed by the weight of the air above it, remains compressed once the cap is in place, and so presses on the mercury as before, thus preventing the descent of the column of mercury. Torricelli drew an analogy between the compressed air and a cylinder of compressed wool pressing on the base of its container. He invited Ricci to imagine that a sheet of iron be inserted part way up the wool so as to insulate the upper part from the lower. Torricelli insisted that the lower portion of the wool would remain compressed and so continue to press on the base as before. 'Try it yourself', wrote Torricelli, 'for I shall not continue to bore you'.[18]

A second objection raised by Ricci led Torricelli close to acknowledging the isotropy of the forces acting in liquids and compressed air. Ricci noted that the force required to support the column of mercury must act upwards, whereas the weight of air acts downwards. Part of Torricelli's response involved the observation that if a pitcher partly filled with water is thrust mouth downwards in water and a hole is made in its base, now uppermost, to allow the air to escape, then the water will rise in the pitcher in spite of the downwards force arising from its weight. This effect, together with the recognition that wine will spurt horizontally from a hole in the side of a wine barrel, illustrates that 'although by nature liquids gravitate downwards, they press and spout in every direction, even upwards, as long as they find places to reach, - that is, places which resist with less force than their own'.[19]

In spite of Torricelli's appeal to the weight of the air and his use of the 'ocean of air' metaphor, the recognition that it is the compression of air that is central to its characteristic behaviour, rather than weight, which it shares with liquids and air, is implicit in his exchange with Ricci. Air presses on a surface by virtue of the degree to which it is compressed. In the case of our atmosphere, it is the weight of air that causes the lower strata of air to be compressed. However, air compressed to the same degree by some other cause, such as the pressing of the plunger of a syringe, will press on a surface

[17] Torricelli's letters are in Torricelli (1919), Vol. 3, pp. 198–201. The English translations, from which I have quoted, are in Pascal (1937), pp. 167–170.

[18] Pascal (1937), p. 169.

[19] *Ibid.* Torricelli had investigated the horizontal, and also vertical, efflux of liquids early in the 1840s. For details, see Maffioli (1994), pp. 71–89.

that bounds it in the same way and to the same extent. These points were brought to the fore by developments that took place in France in the wake of Torricelli's innovations. An experiment, versions of which were conducted independently by Giles Persone de Roberval and Etienne Noel in 1647, involved introducing equal volumes of water and air into the Torricellian space above the mercury in a barometer, with dramatically different results. The introduction of the air caused a much greater lowering of the mercury level than an equal volume of water, in spite of the fact that the water sample weighed around a thousand times more than the sample of air.

In his discussion of that phenomenon, Roberval stressed that it is a natural property of air, that is, a property it possesses by virtue of being air, to expand spontaneously into any space available to it and to press on any surface that prevents it doing so. When atmospheric air is introduced into the Torricellian space, 'it spontaneously and of itself becomes rarefied in the tube'. 'As a matter of fact', Roberval elaborated, 'if besides mercury or water, there be admitted into any part of the tube some of our compressed and condensed air, as we have stated above, this air obtains its freedom and all its parts recoil and become rarefied and drive out the mercury or water, which for that reason will be depressed below the aforesaid height, either more or less, according to the air itself possesses greater or lesser power of rarefaction'.[20]

Air has a 'power of rarefaction' not possessed by solids or liquids and which is the greater the more the air is compressed. The detachment of the force exerted by condensed air from weight considerations was taken a stage further by Roberval's insertion of a carp's bladder, freed of most of its air and tied at the neck, into the Torricellian space. The power of rarefaction of the air was illustrated in a visually compelling way by the expansion of the bladder, while the near spherical shape of it attested to the isotropic character of the expansive force that pressed 'on all sides'.[21]

Experiments conducted or orchestrated by Blaise Pascal in a 2-year period from late 1646 were widely reported and influential, although, as Boyle became well aware, their repetition proved to be far from straightforward. The most significant ones were Torricelli's experiment conducted with water rather than mercury; the 'void in the void' experiment which demonstrated that the mercury level in a barometer falls to near zero when it is situated in the Torricellian space of a second barometer; the Puy de Dôme experiment which demonstrated the variation of the mercury level in a barometer with height; and the observation that a bladder with air trapped within it expands when taken up a mountain.

Although all of the above developments were made known in England via correspondence, especially that between Mersenne in France and Theodore Haak and Samuel Hartlib in England, a more accessible and influential source in England was a section on pneumatics in *Experimenta Nova Anatomica*, by a French physiologist, Jean Pecquet, published in 1651. An English version was published in London in 1653. Pecquet presented clear accounts of the new experiments in pneumatics described above and deployed them to support his identification of the key cause of the effects involved in what he called the air's 'elater', to be distinguished from its weight. Pec-

[20] The quotations are from the English translation of Roberval's letters in Webster (1965), pp. 497 and 499.

[21] Pascal (1937), p. 497.

quet's 'elater' was the equivalent of Roberval's 'power of rarefaction'. Boyle cited Pecquet on a number of occasions, and his notion of the 'spring' of the air shows clear signs of owing a debt to Pecquet's 'elater'.[22]

4 Boyle's innovations in pneumatics

While the deployment of the spring of the air by Boyle in his pneumatics was not novel, it was highly significant nevertheless. The fact that the concept needed to be elucidated, clarified and publicised is evident from the fact that, as we have noted, the treatise on air written by Pascal in 1654 gave undue emphasis to the weight of air and did not involve a concept playing the role of Boyle's 'spring'. When Boyle came to review the book embodying Pascal's treatises, on liquids and on air, following their publication as one volume in 1663, he paid scant attention to the latter, concentrating his review on *Equilibrium of Liquids*. The reason he gave was that Pascal's treatise on air had been rendered outdated because of the extent to which the matters had been 'more clearly made out by experiments, which could not be made by *Monsieur Pascal*, and those other learned men, that wanted the advantage of such engines and instruments, as have in this place [The Royal Society] been frequently made use of'.[23] But Boyle may well have added that it was not only the character of the experimental evidence he invoked that rendered Pascal's theory outmoded, but the latter's failure to theorise the characteristic feature of air, over and above its weight, that was responsible for pneumatic effects and which Boyle had termed its spring. While Boyle's major contributions to pneumatics came via his experiments, especially but not exclusively those utilising the air pump, he also deployed and reflected on them to clarify and extend its theoretical formulation.

In his explication of the spring of the air, Boyle followed his predecessors in drawing analogies between air and elastic solids. However, Boyle's theorisation of the spring of the air made explicit the distinction between elasticity in solids and in air. An elastic solid resists expansion as well as contraction, whereas air has 'a restless power of expansion' only.[24] What is more, this power of expansion is isotropic, differing from elasticity in solids in that respect. These differences between air and solids were stressed by Boyle, for instance, when he described the force experienced on a finger used to seal the upper end of a barometer tube.

> It is to be considered, that the air being a fluid as well as a heavy body, it does not (as grosser weights would) press only on the upper parts of the finger, but pressing as much of the finger as is exposed to it almost everywhere, and almost uniformly as well as strongly, it does by its lateral pressure on every side thrust in the pulp of the finger into the hole where there is not any resistance at all, or

[22] The debt that Boyle, and other English researchers such as Henry Power, owed to Pecquet is stressed by Webster (1965), pp. 451–458.

[23] *Works* V, p. 205.

[24] *Works* I, p. 245.

> at least near so much pressure against the pulp as that of the ambient air against the parts of the finger ambient to it.[25]

I have selected this quotation from the many other references implying the isotropy of the spring of the air because here Boyle makes it clear that it is the fluidity of air as opposed to the solidity of 'grosser weights', that is responsible for the isotropy.[26]

The passage quoted above is taken from Boyle's *Defence against Linus,* published in 1662 as a response to a critique of the *Spring of the Air* by the Aristotelian Franciscus Linus. The latter proposed that forces attributed by Boyle to the spring of the air were in fact due to the stretching of a 'funiculus' (thread) joining the extremities of evacuated regions, so that, for instance, the force on the finger stopping the barometer tube described in the passage in question is indeed due to a sucking, in keeping with what might well be inferred, and indeed felt, when the experiment is conducted by the uninitiated. Boyle was keen to establish that pneumatic forces arise from the pushing due to condensed air rather than a sucking due to expanded air. Air does not have a natural propensity to contract in the way that a stretched solid does. Forces resisting expansion are plausible in solids, which are relatively rigid and have a natural size. However, the fluidity of air and its propensity to expand imply that its parts are not connected in a way that would generate a resistance to expansion. While a watch can be driven by the pushes and pulls arising from the contraction or stretching of a metal spring, Boyle was intent on showing that pneumatic effects arise as a result of pushes only, those due to the 'restless power of expansion' that he called its spring.

Boyle strengthened the case for attributing the causes of pneumatic effects to pushes arising from the spring of air by correlating the strength of experimental effects with variations in the spring that he was able to bring about in the receiver of his air pump. He was able to give mechanical explanations of a wide variety of effects, some of them previously established and some of them novel fruits of his own experimentation. So, for instance, the difficulty of lifting the stopper in the receiver of the air pump once a significant quantity of air is pumped from the latter is explained by the fact that

> the air in the receiver, being very much dilated, its spring must be very much weakened, and consequently it can but faintly press up the lower end of the stopple, whereas the spring of the external air being no way debilitated, he that a little lifts up the stopple must with his hand support a pressure equal to the disproportion between the force of the internal expanded air, and that of the atmosphere incumbent upon the upper part of the same key or stopple.[27]

Likewise, the expansion of a bladder caused by the evacuation of the receiver in which it is housed is due to the 'surmounting of the debilitated spring of the ambient air remaining in the vessel, by the stronger spring of the air remaining in the bladder'.[28] Two polished discs of marble in contact, one beneath the other, provide a third from

[25] *Works* III, pp. 22–23.

[26] About a decade later, Boyle devoted a whole tract to the differences between pressure in solids and in fluids. See *Works* VII, pp. 215–225.

[27] *Works* I, p. 171.

[28] *Works* I, p. 174.

amongst the many examples that can be drawn from Boyle's text. The difficulty of separating the discs is due to the fact that the lower surface of the lowermost stone 'being freely exposed to the air is pressed upon by it, whereas the uppermost surface, being contiguous to the superior stone, is thereby defended from the pressure of the air which consequently, pressing the lower stone against the upper, hinders it from falling'.[29] The transmission of the force due to air pressing on a water surface through the body of the water is demonstrated by immersing a partially inflated bladder beneath the water surface and demonstrating how the balloon expands or contracts as the degree to which the air presses on the water surface is decreased or increased by means of the air pump.[30]

As Webster has stressed, some of the experiments in *Spring of the Air*, Experiments 7 and 17 for instance, were designed to investigate the quantitative relationship between the spring of the air and its degree of rarefaction.[31] Here Boyle did not meet with much success. However, within 2 years, Boyle had performed the experiment, oft repeated in school classrooms since then, involving a volume of air trapped by mercury in a U-tube the results of which supported the hypothesis that the spring of the air is proportional to its degree of rarefaction, that is, to its density.

I have not attempted to give a detailed and comprehensive account of Boyle's experiments in pneumatics in this place.[32] Rather, I have sought, by means of some examples, to illustrate how Boyle developed his concept of spring and deployed it to give mechanical explanations of phenomena exhibited by his experiments. In the following section, I set the scene for further reflection on his achievement with some remarks on the concept of pressure.

5 A digression on the concept of pressure

So far in my discussion of Boyle's pneumatics, I have avoided any significant reference to a term that he employed frequently in his writings on that topic, namely 'pressure'. As we shall see, there is something to be said for the view that it was by way of introducing the concept of pressure into hydrostatics and pneumatics that the likes of Pascal and Boyle set those sciences on their modern course. However, I stress that any such position requires that we become very clear about what concept of pressure is at stake, the available literature being deficient for its lack of specificity on this crucial issue. It will help sharpen our critical faculties if we contrast the modern, technical concept of pressure with the common sense notion of pressure that dates back to ancient times and which was referred to by the Latin *pressio/pressionem*.[33] We will

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29 *Works* I, p. 238.

30 *Works* I, p. 210.

31 Webster (1965), pp. 467–470.

32 Conant (1970) and Webster (1965) are classic studies of these developments and a new and controversial perspective on them is Shapin and Schaffer (1985).

33 Alan Shapiro noted the importance of the distinction between the common and technical senses of the term 'pressure' in his discussion of the hydrostatics implicit in Descartes' cosmological theory of light which, as Shapiro stresses, lacked a technical sense of 'pressure'. See Shapiro (1974), especially p. 251, note 29.

then be in a position to contemplate where Boyle's talk of pressure is to be situated in relation to these two extremes.

The modern concept denotes the stress on an imaginary boundary within a fluid at any given location, measured as a force per unit area. In spite of its specification as a force per unit area, pressure is not a vector, as force is, but a scalar. In view of this, the equation $F = P.\, A$, giving the force due to pressure P on surface of area A, can be puzzling insofar as the left-hand side of the equation is expressed as a vector, whereas the right-hand side appears as the product of two scalars. The puzzle is resolved once it is realised that an area of surface, A, is directed like a vector insofar as it has an orientation. The orientation of any particular surface can be specified by a unit vector, i, normal to its surface. We can then write the expression for the force on a surface as $F = P.\, A.\, i$, an expression that now equates two vectors rather than a vector and a scalar. This is rarely done, however, and there is a reason for that. The force due to pressure on a surface of area A is independent of the orientation of the surface. Pressure within a fluid in equilibrium is isotropic. The gas equation, $PV = nRT$ that became central to thermodynamics in the nineteenth century relates pressure to two other scalars, volume and temperature. Within the body of a fluid in equilibrium, the net force due to pressure, which acts equally in all directions, is zero. It is variations in pressure, such as those that occur at the boundary between a fluid and a solid, that lead to forces. Since Euler it has been recognised that the key property of fluids that distinguishes them from solids is that the former are unable to sustain a tangential stress. It is a consequence of this that the force on any surface within the body of a fluid in equilibrium is normal to that surface and that the force per unit area, that is, pressure, is isotropic. Given this understanding, pressure occurs only in fluids and not in solids.

Let us now follow this reflection on the concept of pressure as it occurs in modern physics by corresponding reflection on a common conception of pressure employed in ancient times and which persists in today's everyday discourse. Many common usages to be found in Latin texts involve the effect of the weight of solids. The flattening of the grass beneath a blanket at a picnic is attributed to the pressure caused by the weights upon it. An example that moves beyond weight involves the expansion of a bladder caused by forcing a fluid into it. It is natural to understand the expansion as the result of pressure, and the bursting of the bladder as a result of that pressure becoming more than the bladder can withstand. These common usages of the term 'pressure' all involve forces on a solid surface. The verb 'to press', *premo* in Latin, carries with it the idea of a directed force. Individual examples of pressing have a direction. What is more, solids can exert pressing forces as well as fluids. Indeed, the weighing of solids against surfaces constitutes a paradigmatic example of pressure in its common sense, as implicit in the use of the example involving the flattening of grass at a picnic. It still makes common sense to note that the floor of a loft must be built so that it is sufficiently strong to bear the *pressure* of the load it will need to carry, the weight per unit area rather than total weight. The facts that, according to common understanding, solids as well as fluids can exert pressure, and that the pressings on surfaces that constitute pressures are vectorial, signal two significant ways in which the common sense notion of pressure differs from the technical sense.

I do not introduce the contrast between the two notions of pressure in order to cast judgement on Boyle's usage according to how far he moved beyond the common towards the modern sense. Nor do I invoke the modern sense to serve an explanatory function as if it were acting as an attractor drawing the progress of pneumatics and hydrostatics towards it. My purpose is to highlight the fact that if we are to understand the notion of pressure as used by Boyle and his contemporaries, then there is work to be done to discern exactly what they intended by the term. The contemporary literature suffers for want of such work. For instance, contemporary translations into English of works by Stevin in Dutch and by Roberval and Pascal in French make use of the term 'pressure' without comment, presumably in the expectation that the reader will interpret it in its modern sense.[34] The problem lurking here is exacerbated by the fact that while the translated texts talk of pressing, they do not use a noun (*druck* in Dutch, *la pression* in French) that is the equivalent of 'pressure'. The more recent analysis of Boyle's pneumatics by Shapin and Schaffer involves confusions that are of their own making due to the inadequate attention the authors pay to the precise meaning of 'pressure' as it evolves in Boyle's writings.[35]

6 The concept of pressure in Boyle's pneumatics

If Boyle is to be read as making significant advances to the understanding of fluids by introducing a concept of pressure, then it is necessary, on the one hand, to identify the way in which his version of the concept moved beyond the common sense that dates from antiquity without, on the other hand, uncritically presuming that it involved all that is implied by the modern one. In my view, Boyle's writings on pneumatics, at least those prior to his reading of Pascal's Treatises in 1664, do not involve a technical sense of 'pressure' but only a common sense. As the title of his pioneering work suggests, the theoretical merit of *New Experiments Physico-Mechanical Touching the Spring of the Air and Its Effects* lay in the detailed way in which the spring, as distinct from the weight, of air was specified and brought to bear on the explanation of a range of pneumatic phenomena. Boyle did build on the work of predecessors like Torricelli, Roberval and Pecquet to develop a technical sense of spring that he used to capture the distinctive characteristic of air that distinguished it from solids. As a result of this spring, air presses on any surface with which it is in contact, such as the surface of the earth or the walls of a bladder. I claim that Boyle used the term 'pressure' in its common sense to refer to such instances of pressing. The only way in which the usage might be said to have moved beyond common sense was the explicit recognition that the force on a surface due to a sample of air under a given degree of compression is independent of the orientation of the surface together with the specification of that pressing as a force per unit area.

No formal definition or explication of 'pressure' is to be found in *Spring of the Air* which contrasts with the pains to which Boyle went in order to clarify the notion of 'spring'. The first occurrence of the term 'pressure' occurs in Boyle's elaboration of

[34] The works in question are the translation of Stevin's *Elements of Hydrostatics* in Dijksterhuis (1955), the translation of Pascal's treatises on hydrostatics and pneumatics in Pascal (1937) and the translation of the relevant letters of Roberval in Webster (1965).

[35] The confusions occur in Shapin and Schaffer (1985), especially Chapter 2. They are discussed in footnote 42.

Experiment 1, which is concerned with the force on the sucker which is involved in the extrusion of air from the receiver of the air pump. When the handle attached to the sucker is released after such an extrusion, the sucker is drawn up towards the receiver. This happens because of 'the protrusion of the external air, which, being much less rarified than that within, the cylinder, must have a more forcible pressure upon the sucker, than the internal is able to resist'. In the next paragraph, Boyle begins his exposition of the spring at work.

> For the more easy understanding of the experiments tryable [*sic.*] by our engine, I thought it not superfluous, nor unreasonable in the recital of this first of them, to insinuate the notion by which it seems likely that most, if not all, of them will prove explicable. Your Lordship will easily suppose, that the notion I speak of is, that there is a spring, or elastical power in the air we live in. By which ἐλατηρ or spring in the air, that which I mean is this: That our common air either consists of or at least abounds with, parts of such a nature, that in case they be bent or compressed by the weight of the incumbent part of the atmosphere, or by any other body, they do endeavour, as much as in them lies, to free themselves from that pressure, by bearing against the contiguous bodies that keep them bent.[36]

Note that it is weight or force of some other body that is the 'pressure' which compresses the air. This need involve nothing beyond the common sense of pressure. It is the spring of the air that is the novel 'notion' explicated by Boyle.[37] It is by virtue of this spring that atmospheric air presses any solid surface with which it comes in contact, that air trapped in a bladder causes it to expand, that supports the mercury in a barometer and which prevents a smooth marble disc from falling from a second disc to which it is adjacent. The forces on the various surfaces involved in such cases are frequently described by Boyle utilising a common concept of pressure that was far from novel.

Boyle moved beyond the common sense of his day by insisting and demonstrating that air has weight as well as a spring, although he was not the first to do so. The cause of pneumatic phenomena considered by rivals to be due to nature's abhorrence of a vacuum come about 'in consequence partly of the weight and fluidity of bodies here below and partly, perhaps principally, of the spring of the air, whose restless endeavour to expand itself every way, makes it either rush in itself, or compel the interposed bodies into all spaces, where it finds no greater resistance than it can surmount'.[38]

Pneumatic effects are a consequence of the spring, weight and fluidity of air. It is not difficult to understand why Boyle singles out spring as the principal cause. Because of its 'restless power to expand', air will fill any container into which it is put, its spring causing it to press against the sides of the container to a degree depending on its degree of compression. However, in the case of atmospheric pressure there is no containing vessel. In this case, there is a downwards force due to the weight of air,

[36] *Works* I, p. 165

[37] Boyle frequently used the term 'notion' to describe the concepts involved in his science and stressed the fact that novel experimental advances make necessary the fashioning of novel notions or the modification of old ones. See, for example, his remarks to this effect in *Proemial Essay*, *Works* II, p. 20.

[38] *Works* I, p. 245.

the lower layers of which are compressed as a result. At ground level, atmospheric air is compressed to a degree that is dependent on the weight of the air above it, with a resulting equilibrium between the force due to the resulting spring, acting upwards on the air, and the total weight acting downwards. The air, being thus compressed, presses in all directions, on the ground beneath it and on the sides of anything that is immersed in it. It is the weight of the air that causes the compression at ground level, and the spring accompanying this compression that is responsible for the pressing. That the spring of the air is the principal cause of the pressing is illustrated by the fact that the same degree of pressing will ensue if the air is compressed to the same degree by some cause other than the weight of atmospheric air. As Boyle pointed out, this is illustrated by the fact that, in the lead up to his version of the 'void in the void' experiment, when a barometer is inserted into the receiver of the air pump prior to its evacuation, the level of the mercury remains unchanged from what it was before, in spite of the fact that in the receiver it is shielded from the atmosphere.[39] The spring of the air is the principal cause of pressing. However, the weight of air needs to be included as a cause because without it the restless power of air to expand would ensure that there is no atmospheric air and no atmospheric pressure.

Spring and weight of air are on a different ontological level than the pressures to which they give rise. This point was made quite explicit by Boyle in *Continuation of New Experiments Physico-mechanical Touching the Spring and Weight of the Air, and their Effects* (1669). The quotation is from Boyle's Experiment 11, in which he demonstrates that when the air is sucked out from the region above the mercury in a vertical tube the latter rises in the tube until it reaches a maximum height if 29 inches or so, an effect Boyle attributed to atmospheric pressure, thereby removing the need to invoke nature's abhorrence of a vacuum. Boyle argued that

> whether there be or maybe a vacuum or not, there is no need to have recourse to a *fuga vacui* to explicate suction; and also whatever other causes have by *Gassendus* and *Cartesius* been ingeniously proposed to explicate the suction, it seems to depend clearly upon the weight of the atmosphere, or in some cases upon the Spring of the Air; though I deny not, that other causes may contribute to the pressure of the air; which I take to be the grand and immediate agent in these *phenomena*.[40]

All pneumatic effects result from the pressing of air against solid surfaces. In that sense, pressure is the 'immediate' agent and it is a 'grand' one in the sense that it is involved in all such effects. The main, non-immediate causes of these pressures are the weight and spring of air, although Boyle does not rule out the possibility that there be others.[41] So pressures, that is, the pressings against a solid surface exerted by air,

[39] As noted above, this kind of point had already been made by Torricelli in his response to Ricci's queries.

[40] *Works* VI, p. 62.

[41] On a number of occasions Boyle pointed to the phenomenon of capillary rise as an additional cause, significant when liquids in narrow tubes are involved.

come about as a result of its weight or spring (to which pair of causes Boyle elsewhere adds fluidity, as we have seen) or some combination of them.[42]

Air, by virtue of the kind of thing that it is, possesses weight, fluidity and spring, the isotropic character of the spring being a consequence of the fluidity. As a consequence of these properties air presses against any surface with which it happens to be in contact. The common sense of 'pressure' can be readily utilised to describe such pressing, and this is precisely how Boyle used the term in his pneumatics, from the *Spring of the Air* and thereafter. This standpoint will need to be qualified in the context of Boyle's treatment of pressure as exhibited by liquids.

7 'Pressure' in Boyle's hydrostatics

Boyle had not focused on the issue of a theoretical characterisation of liquids as distinct from solids and air in *Spring of the Air*. In that work, Boyle distinguished *air* from solids by stressing that spring is distinct from weight, which solids and air alike possess, and that the spring of the air is distinct from elasticity in solids through being isotropic and being expansive only. Forces are transmitted mechanically from point to point in air on account of its spring. That is why Boyle had no need to develop a notion of pressure that went beyond the common sense one and which he could use to describe the forces exerted by air on solid surfaces. That standpoint could not work for liquids because they lack a corresponding spring, a fact that Boyle appreciated and had explored experimentally.[43] He seems to have become aware of the theoretical challenge posed by liquids, as distinct from solids and air, after reading Pascal's *Equilibrium of Liquids*. His response was spelt out in *Hydrostatical Paradoxes made out by New Experiments (for the Most Part Physical and Easy)* (1666).

Apart from some minor qualifications, Boyle accepted the theoretical content of Pascal's hydrostatics as 'worthy of the author'.[44] However, he had problems with the experimental evidence Pascal invoked in its support. He raised doubts about whether Pascal had performed the experiments he described and whether they could possibly be performed following Pascal's prescriptions. His doubts ranged from the probability that stoppers in apparatus as described by Pascal would leak to the observation that some of the experiments required that experimenters make extended observations

[42] In *Leviathan and the Air Pump*, a book that, as noted in Wootton (2015), Chapter 11, has been referred to as 'the most influential book in the history of science since Kuhn's *The Structure of Scientific Revolutions*', Shapin and Schaffer find ambiguities in Boyle's use of 'pressure' that can be seen as of their own making once the relationship between spring and weight on the one hand, and a common sense of 'pressure' on the other, is appreciated. According to our authors, Boyle used the term 'pressure' generically to refer to spring and weight. 'So "pressure" is to be read as an embracing term, and its ambiguities and variation of meaning were themselves a resource that Boyle used in debating the air-pump trials' (Shapin and Schaffer (1985), p. 55). By grouping together weight, spring and pressure as Boyle's 'principal ontological concern', Shapin and Schaffer interpret passages in Boyle as ambiguous and merely rhetorical which, on the account I offer, can be interpreted literally and not merely rhetorical as making clear and explicit claims about the relationship between pressure, in the common sense of the term employed at the time, and its causes, the spring, weight and fluidity of air.

[43] *Works* I, p. 168.

[44] *Works* V, p. 206.

twenty feet under water.[45] He also pointed out the difficulty of readily reproducing Pascal's experiments due to the fact that many of them required large quantities of mercury and glass tubes over twenty feet in length. Boyle presented himself as an expositor of Pascal's hydrostatics seeking to clarify aspects of it by way of easy to perform experiments. He aimed to show how various hydrostatic phenomena, some of which can appear as 'surprising' and even 'abstruse' can be 'made out by new experiments (for the most part physical and easy)', as announced in the title of his tract.[46]

Near the beginning of *Hydrostatical Paradoxes*, Boyle characterised his objective as follows:

> It not being my present task to deliver the elements, or a body of hydrostatics, but only ten or twelve paradoxes, which I conceive to be provable by this new way of making them out, I shall to avoid confusion, deliver them in as many distinct propositions; after each of which, I shall endeavour in a proof, or an explication, to show, both that it is true, and why it ought to be so.[47]

It is clear from his earlier remarks that Boyle aimed to go beyond 'geometricians' such as Stevin who, in his opinion and as noted above, had asserted various propositions to be true without showing how or why they are true. Boyle found the seeds of what he needed in this respect in Pascal's *Equilibrium of Liquids*. Pascal had indicated that hydrostatic forces are transmitted through liquids by virtue of their 'continuity and fluidity', giving rise to forces on bounding surfaces that are independent of their orientation.[48] However, Pascal did not specify the way in which forces are transmitted from point to point within the body of liquids. As I see it, Boyle's main contribution to the theoretical grasp of hydrostatics was to fill this gap, thereby completing a mechanical account of how hydrostatic phenomena come about.

Before embarking on his exposition in *Hydrostatical Paradoxes*, Boyle indicated that he would 'premise a word or two by way of either *postulation* or *lemma*'.[49] I believe that his ensuing remarks reveal some innovative and consequential aspects of his approach, however innocuous they might appear to a modern eye on first reading. We are to consider a glass pipe, open at both ends, held vertically in a container of water with its lower end below the water surface. Boyle invokes 'an imaginary plane or surface, which touching that orifice [the lower end of the pipe] is parallel to the horizon; and consequently parallel as to sense to the upper surface of the water, and this being but a help to the imagination, will readily be granted'. Boyle proceeds to discuss various equilibrium conditions in terms of the forces acting on either side of this imaginary plane. The water will initially be at the same level inside and outside of the tube (provided the effect of capillary rise is negligible) because the water will press equally on all parts of the imaginary plane. If oil is poured on the water outside of the pipe, the water inside the pipe will rise until the effect of the weight of the oil,

45 *Works* V, pp. 206 and 255.

46 *Works* V, p. 194.

47 *Works* V, p. 207.

48 See Pascal (1937), pp. 7–8 for a clear and explicit expression of this point.

49 *Works* V, p. 207.

acting on the plane outside of pipe via the main body of water, is countered by the additional water rising inside of the pipe. This strategy, of considering the forces acting on an imaginary plain within a liquid, which Boyle introduces simply as an aid to the imagination, is of considerable consequence. By extending its use to situations more complicated than a vertical pipe partially immersed in water to include those involving forces on planes that are not horizontal, Boyle in effect traces how hydrostatic effects are transmitted from point to point through a liquid.

Boyle continued to use the term pressure in its common sense to refer to forces exerted by liquids on a solid surface. For instance, Paradox VI is concerned with the specification of the force exerted by liquids on the surfaces of immersed solids and we find Boyle specifying the 'pressure' on such a surface as equal to a column of liquid having a cross section equal to that of the surface in question and a height equal to the depth of that solid surface beneath the water surface.[50] Boyle extended his discussion to include forces acting laterally as well as vertically in the following terms:

> Whence we may learn (what I have not yet found mentioned by any writer,) that even in the midst of the water we may suppose a pillar of *water,* of a basis equal to the side of an immersed body, (and reaching to the lowest part of it;) and that, though this imaginary aqueous pillar — be not included in any solid body or stable superficies; nevertheless its lower parts will have a lateral pressure tending outwards, against the imaginary sides, from the weight of the water that is above these subjacent and lateral parts; and will have the pressure increased proportionably [*sic.*] to the height to which the imaginary pillar reaches above them.[51]

Here Boyle explicitly uses the concept of pressure in the extended sense that I have identified, to refer to the action of water on water. In addition, his reference to the fact that such an extension is not to be found in the work of any other writer suggests that his own conception of his originality on this point coincides with mine. In Boyle's exposition of hydrostatics, the reference of the term 'pressure' has been extended from the forces on the boundaries between solids and liquids to include the equal and opposite forces that act on each other across imaginary boundaries within the body of liquids in equilibrium.[52]

The shift I have identified, from a common sense of 'pressure' to a more technical sense that serves to distinguish solids from fluids, is exemplified particularly clearly

[50] *Works* V, p. 239. This much was entailed by Stevin's *Elements of Hydrostatics*, but, to use Boyle's words, Stevin had asserted that it was true without showing why it was true. The inadequacies of Stevin's proofs are discussed in Chalmers (2015).

[51] *Works* V, p. 248, emphasis in original.

[52] Stevin introduced two technical terms in his *Elements of Hydrostatics* that to some degree resemble Boyle's imaginary planes. According to Stevin's Definitions VII and VIII a 'surface vessel' is 'the complete geometrical surface of a body, conceived as separable therefrom' and 'bottom' [*bodem*] is 'any plane against which rests any water'. Stevin explicitly likened the former to the planes of geometry, describing them as 'vessels without any corporeal magnitude and without any weight' (Dijksteerhuis 195, p. 385). Although they do not have 'corporeal magnitude', Stevin's 'surface vessels' are solid insofar as they can contain water, in accordance with the nature of solids as specified in Definition VI which reads 'Solid body is one whose matter does not flow, and though which penetrates neither water nor air'. The surface vessels need to be solid surfaces in order for him to put to work his Postulate III, the only postulate that introduces significant hydrostatic content into his theory. 'The weight causing a vessel to sink less deep to be lighter,

in *An Hydrostatical Discourse*. In that tract, published in 1672, Boyle responded to criticism of his position by More, the general drift of which was that the former's pneumatics and hydrostatics needed to be bolstered by reference to non-mechanical causes, such as the tendency of the elements to move to their natural places, in order to adequately explain the relevant phenomena. In his reply, Boyle made it clear that he was more than ready to 'assert an incorporeal being that made and governs the world' but insisted that appeal to incorporeal agents was not necessary to accommodate pneumatics and hydrostatics since they could be explained 'mechanically'.[53] In his subsequent defence of this position, we find Boyle articulating and clarifying a *mechanical* conception of 'pressure' as a cause distinct from weight.

In one specific interchange, More invoked an experiment which he saw as raising doubts about the extent to which hydrostatics can be explained mechanically. It involved the recognition that if a disc of wood, less dense than water, is forcibly held at the bottom of a container of water and then released, it rises to the surface. In his example, More supposed that the surface area of the wooden disc is only a little smaller than that of the cross-sectional area of the vessel of water in which it is immersed. He pointed out that the weight of the wooden disc plus the weight of the column of water vertically above and pressing down on it is greater than the weight of water surrounding the disc. More concluded that considerations of weight as understood by Boyle are incapable of explaining why the disc rises to the surface.[54]

In his reply, Boyle in effect agreed with this latter claim, but argued that the problem is solved, not by assuming, as More did, that water does not weigh in its 'proper place', nor by invoking any other non-mechanical cause, but by invoking pressure, a mechanical cause distinct from weight. I will not rehearse the details of the interchange in this place because the issue at stake is brought out more clearly by an ingenious experiment of his own that Boyle invoked in an extension of the discussion.[55] A focus on that experiment also enables me to draw an illuminating comparison with Galileo's deployment of a very similar experiment.

In the experiment in question, Boyle poured some molten wax into a flask whose base was covered with a shallow layer of water. When the wax cooled and solidified, it contracted into a solid body of wax leaving a small space between it and the walls of the containing vessel. When water was introduced into this small space, the wax, being

Footnote 52 continued
but the weight causing it to sink deeper to be heavier, and that causing it to sink to the same depth, equally heavy'. This postulate is one that can be 'granted' insofar as it is an abstraction from common experience of the effect of adding or subtracting weights to floating vessels, which need to be solid to hold or support them. It is also clear from the context in which Stevin uses the term 'bottom' that these, although lacking thickness and weight, are *solid* surfaces against which water can press. The solid nature of 'bottoms' is made quite explicit when Stevin describes each of those bordering a rectangular prism of water as 'a corporeal rectangle' (Dijksterhuis (1955), p. 415). So, in spite of the degree to which Stevin's surface vessels and bottoms abstract from weight and corporeal magnitude they do not abstract from solidity as possessed by solids and so cannot perform the function played by Boyle's imaginary planes against which liquids press in a way not involving pressure against a solid surface.

53 *Works* VII, p. 159.

54 An English translation of the relevant passages from More's *Enchiridion Metphysicum* of 1671 is given by Boyle, *Works* VII, p. 160.

55 The details of the interchange can be followed in *Works* VII, pp. 158–164.

slightly less dense than water, was raised so that its upper surface remained a little above the water surface. The rising of the wax by the water took place in spite of the fact that 'this collateral water was so far from being heavier than the wax its pressure impelled up, that both the collateral and the stagnant water all together, being weighed in good scales, amounted to little above a quarter of the wax'.[56] Boyle insists that here, as in More's experiment and in the case of immersed bodies in general, the phenomenon is explained by invoking 'the pressure of the ambient fluids on immersed solids as uniform or every way equal'.[57] The wax floats because the force arising from the pressure on its lower surface exceeds that arising from the pressure on its upper surface.

The notion that floating bodies experience an upthrust equal to the weight of water they displace encourages an understanding of floating involving balancing weights, an interpretation to which Archimedes' original text on floating bodies lends itself.[58] A solid displaces an amount of water which weighs down as a consequence of its propensity to return to its former place and floating occurs when that weight balances the weight of the immersed solid. Boyle's experiment undermines this interpretation as did More's, since, in the circumstances that they each consider, the displaced water weighs much less than the floating body. Galileo had undermined this understanding of floating in terms of balancing weights in this way over half a century earlier, and it is instructive to compare his treatment with that of Boyle.

In the hydrostatics that Galileo developed in *Bodies That Stay Atop of Water, or Move in it*, published in 1612, we find him elaborating on the fact that a ship can float in a dock containing water that weighs less than itself. He illustrated this by reference to a block of wood floating in water in a vessel into which it barely fitted and so leaving room for very little water. Consequently, 'a very small quantity of water may raise up and sustain with its small weight a solid body that is a hundred or a thousand times heavier'.[59] The situation is remarkably similar to Boyle's experiment with wax.[60] However, Galileo's explanation of the phenomenon differs markedly from that of Boyle. Galileo dealt with it by invoking a principle which was an extension of one that he found in *Questions of Mechanics* that he attributed to Aristotle.[61] It is the principle that when balancing weights are displaced slightly from their equilibrium positions the product of weight and velocity of displacement on either side of the fulcrum are equal. If the floating body described by Galileo is slightly displaced downwards, the small amount of water surrounding it rises to a considerable degree and in such a way

56 *Works* VII, pp. 162–163.

57 *Works* VII, p. 161.

58 Proposition 5 of Book 1 of Archimedes' *On Floating Bodies* reads, 'Any solid lighter than a fluid, if placed in the fluid, be so far immersed that the weight of the solid will be equal to the fluid displaced' (Heath 1950, p. 257). This is not the case in Boyle's experiment with the wax.

59 I have used the translation of *Bodies that Stay Atop Water* in Drake (1981). The quotation is on p. 26. A detailed discussion of these views of Galileo and the path that led to them can be found in Palmieri (2005).

60 There is no direct evidence that Boyle was drawing on Galileo's work here. He did, in *Works* V, p. 194, cite Galileo amongst those of his predecessors who made considerable contributions to hydrostatics. However, Boyle included him amongst those who handled hydrostatics 'rather as geometricians than as philosophers', a judgement that makes sense in the light of the contrast between the treatment of floating by Boyle and Galileo that I am highlighting here.

61 See Drake (1981), p. 31.

that the velocity of displacement of the body times its weight is equal to the much greater velocity of the displaced water times its much smaller weight. It is in this way that Galileo considered himself to have identified the 'true cause' of floating.[62]

I have sympathy with the view, already voiced by Stevin, that hypothetical displacements that do not in fact take place when systems are in equilibrium cannot function as the true cause of that equilibrium.[63] Once equilibrium conditions are known, they can be shown to be the correct ones by applying Galileo's principle. But, certainly if one is concerned with mechanical explanations, this falls short of identifying the (mechanical) causes of floating. Those were identified by Boyle, and he did so by invoking pressure. The phenomena figuring in the above discussion 'depend upon a mechanical aequipollence of pressure'.

> By this you may see that for the regulation of hydrostatical things, Nature has her balance too as well as Art, and that in the balance of nature the statical laws are nicely enough observed.[64]

8 The relationship of Boyle's hydrostatics to that of Pascal

As I pointed out early in the previous section, Boyle was motivated to give serious attention to the formulation of hydrostatics by Pascal's *Equilibrium of Liquids*. The style of reasoning at work in Pascal's treatise was 'experimental' in contrast to the Euclidean style of Stevin's hydrostatics. Whereas the content of Stevin's hydrostatics was to be derived from postulates sufficiently unproblematic to be granted at the outset, Pascal's version was to be rendered intelligible and grantable by demonstrating the range of phenomena, including phenomena elicited by experiment, which could be readily and straightforwardly explained by appeal to it.[65] Boyle followed Pascal in this respect and improved on the latter's efforts insofar as he applied the new hydrostatics to a range of straightforward experiments capable of informing and being performed by 'persons no more than moderately versed in the vulgar principles of hydrostatics' and, moreover, ones that Boyle did in fact perform.[66] Boyle solidified the case for Pascal's hydrostatics by improving the quality and quantity of its experimental basis. This important point notwithstanding, my focus in this section is on the relationship between the theoretical contributions to hydrostatics made by Pascal and Boyle. There is an important sense in which the two contributions were complementary, with Boyle supplying an account of the mechanisms responsible for communicating hydrostatic forces through the body of liquids and Pascal stressing the extent to which hydrostatics could be accommodated under the umbrella of general mechanical principles governing all machines.

62 Drake (1981), p. 41.

63 Stevin's critique of the appeal to imaginary displacements as causes can be found in Dijksterhuis (1955), p. 509. However, he did not respond to the problem by supplying mechanical causes as Boyle was able to do.

64 *Works* VII, p. 164.

65 This comparison is developed in some detail in Chalmers (2015).

66 *Works* V, p. 207.

The approaches of Pascal and of Boyle have been differentiated by describing the former as mathematical and the latter as experimental.[67] Such a contrast is not borne out by the texts with which we have been concerned. Apart from the fact that Pascal provided and stressed the experimental basis of his theory, neither Pascal's *Equilibrium of Liquids* nor Boyle's *Hydrostatical Paradoxes* abound with geometrical demonstrations as is the case with Stevin's *Elements of Hydrostatics.* The former theories are mathematical to the extent that hydrostatic forces on surfaces need to be expressed and treated as forces per unit area, but they are alike in that respect. Notwithstanding this similarity between the treatments of hydrostatics by Pascal and Boyle, there is a theoretical emphasis in Pascal's treatment that is absent from Boyle's.

Pascal's theoretical grasp of hydrostatics was expressed in his appreciation of the fact that devices such as the hydraulic press can be understood as 'machines for multiplying force' analogous to the simple machines figuring in the science of weight. As such, the former could be seen as conforming to the same principles as the latter. With respect to the hydraulic press, Pascal wrote:

> It is remarkable that this new machine exhibits the same constant relation that is characteristic of all the old machines, such as the lever, the wheel and axle, the endless screw, and others, which is that the distance traversed increases in the same [*sic.*] proportion as the force.[68]

The fact that Pascal should have written 'inverse proportion' rather than 'the same proportion' here is clear from his discussion. Pascal extended the analogy with simple machines by noting that 'it is evident that it amounts to the same thing whether we make one hundred pounds of water move through an inch, or make one pound of water move through one hundred inches'. He also noted that the press also conforms to the principal that 'a body never moves by its own weight without lowering its centre of gravity'.[69] Further, Pascal invoked the hydraulic press to bring out in a forceful way the distinction between the behaviour of solids and liquids. Since the force is transmitted from one aperture of the hydraulic press to the other by virtue of the 'continuity and fluidity' of the water, the functioning of the press is destroyed if the communicating water is frozen. Since the freezing does not affect the continuity, it is clearly the fluidity possessed by the water and not by the ice that is crucial for explaining the hydraulic press and other hydrostatic phenomena.

None of the points figuring in the previous paragraph appear in Boyle's *Hydrostatic Paradoxes.* The hydrostatic press does not get a mention. Boyle was concerned, not to embrace hydrostatics within the scope of some general mechanical principles, but rather to identify how forces are transmitted from one location to another within liquids, thereby bringing about hydrostatic effects. The utility of Boyle's stance can be brought out by the following reflection. In the science of weight, the application

[67] See the recent discussion of this issue in Malet (2013) and the references cited there.

[68] Pascal (1937), p. 6.

[69] Pascal (1937), p. 8. Pascal introduced this point by announcing it as a 'proof which will be understood only by geometers and may be disregarded by others', thereby reinforcing my rejection of the characterisation of Pascal's approach as mathematical as opposed to experimental. I have modified the translation by Spiers and Spiers in Pascal (1937) to make it conform better to the French original.

of the principles appealed to by Pascal to particular instances requires a specification of the constraints at work. In the case of simple machines, such as the balance, the constraints are obvious and taken for granted. It is the rigidity of the balance arm that relates the distances moved by a weight to that moved by the one it balances. When it comes to hydrostatic machines, the precise nature of the constraints are not so obvious. Boyle's identification of the way in which forces are transmitted through liquids supplies what is needed here in a more explicit way than is accomplished in Pascal's *Equilibrium of Liquids*.

Pascal had identified fluidity as essential to the behaviour of liquids. Because of this, together with the continuity of liquids, a force applied to an area at one point in a liquid will be transmitted throughout the liquid, exerting the same force per unit area on any other boundary of the liquid, however remote and whatever its orientation.[70] This falls short of tracing the way in which forces are transmitted across boundaries in the body of the liquid. It falls short of using 'pressure' to identify the force per unit area acting between neighbouring portions of liquid through the body of that liquid, as Boyle came to do. Pascal talked freely of water pressing against apertures using the verb '*presser*' but did not talk of pressure within the body of liquids. As I have already noted, Pascal did not use the noun '*la pression*' in *Equilibrium of Liquids* or *Weight of the Air.* It was Boyle who completed the mechanical account of hydrostatic phenomena by providing the links between cause and effect by way of pressure acting throughout the body of liquids.

9 Concluding remarks

Boyle was a leading proponent of the movement that by the 1660s was being referred to by him and others as the experimental philosophy. Pneumatics and hydrostatics exemplified the new kind of science insofar as it was to be rendered intelligible by reference to, and supported by, experiments rather than via Euclidean-style proofs from given premises. As far as pneumatics was concerned, experiment was also essential insofar as it was only by way of it that many of the key phenomena became apparent. Torricelli's and the Puy de Dôme experiment provide ready examples, and Boyle was able to expand the range of novel experimental effects by way of his air pump. By the time Pascal and Boyle turned their attention to a theoretical formulation of hydrostatics, a need for novel experimenting was not pressing since a wide range of relevant hydrostatic effects was already familiar, as is evident, for instance, from Mersenne's survey and extension of relevant phenomena in 1644.[71] A strong case for the theory in Pascal's *Equilibrium of Liquids* could be made by appealing to the natural way in which it could account for a range of well-known effects, which is why he could be confident of the results of further experiments that he had not in fact performed.[72]

[70] See Pascal (1937), pp. 7–8 for a clear and explicit expression of this point.

[71] See Mersenne (1644), pp. 215–233. As noted in Duhem (1905), Mersenne's somewhat disorganised survey of the hydrostatics available to him contains some original observations. They include an anticipation of the hydraulic press (p. 228) and the recognition that hydrostatic effects are destroyed if the water is frozen (pp. 228 and 229). The latter page is wrongly numbered as 239 in the copy of Mersenne (1644) available on http://books.google.com.

[72] This position is argued in Chalmers (2015).

Boyle shared with many of his contemporaries the desire to rid philosophy of what had come to be seen as the obscure appeal to Aristotelian forms and aimed to replace them with contact action between particles of matter. As far as the account of the ultimate ontology of the material world figuring in Boyle's mechanical philosophy is concerned, it was reduced to the shapes, sizes and motions of particles of universal matter and nothing else. When it came to the less ambitious, but for Boyle very urgent, task of establishing explanations of particular kinds of observable phenomena that were accessible to experiment, he took his cue from the ancient science of weight. He sought to move beyond that science by identifying causes in addition to weight capable of affording the kinds of mechanical explanations that it involved in fresh domains. Like weight, the new causes were to be experimentally accessible and yet intermediate insofar as they lacked an explanation in terms of the ultimate ontology of the mechanical philosophy. As far as pneumatics and hydrostatics were concerned, the novel intermediate causes elaborated on by Boyle were 'spring' and 'fluidity', the latter being elucidated by appeal to a concept of pressure.

In pneumatics, the relevant phenomena were linked to their causes by way of the spring of the air. The relevant pushes were transmitted from point to point through a body of air in a way analogous to that in which a force is transmitted from point to point through a stretched elastic string. This conception will not do in hydrostatics since liquids lack a significant 'spring'. Following his reading of Pascal's hydrostatics and building on the emphasis to be found there on the fluidity of liquids as crucially responsible for the transmission of hydrostatic forces, Boyle came to understand fluidity in terms of pressure acting isotropically throughout the body of liquids. It was in that way that Boyle was able to link hydrostatic phenomena to their causes by contact action communicated *mechanically* through the intervening medium, thereby rendering them intelligible according to his understanding of the term.

The notion of pressure acting throughout a fluid was to become central to Newton's hydrostatic. Proposition 19 of the *Principia* reads:

> All the parts of an homogeneous and unmoved fluid included in any unmoved vessel, and compressed on every side (setting aside the consideration of condensation, gravity and all centripetal force), will be equally pressed on every side, and remain in their place without any motion arising from that pressure.[73]

Alan Shapiro has noted that Newton was the first to explicitly acknowledge that the net force due to pressure at any location in a fluid in equilibrium is zero because each part of it is pressed equally 'on every side'. In his pioneering, and rare, analysis of Newton's hydrostatics, Shapiro traces its evolution for its beginnings in *On the Gravitation and Equilibrium of Fluids* composed by Newton in the late 1660s. He analyses in detail how Newton's ideas on hydrostatics grew out of his critique of Descartes' natural philosophy but acknowledges that 'Newton also drew on the mathematical tradition of Archimedes and Stevin, and the contemporary experimental tradition, which he most probably learned from the works of Boyle'.[74] In the light of the argument of

[73] Newton (1954), p. 290.

[74] Shapiro (1974), p. 274.

this paper, that remark does not do justice to Boyle's theoretical reflections, amongst which we find the first treatment of pressure as transmitted from point to point through the body of a fluid.

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