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Thomas Harriot's optics, between experiment and imagination: the case of Mr Bulkeley's glass

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Abstract Some time in the late 1590s, the Welsh amateur mathematician John Bulkeley wrote to Thomas Harriot asking his opinion about the properties of a truly gargantuan (but totally imaginary) plano-spherical convex lens, 48 feet in diameter. While Bulkeley's original letter is lost, Harriot devoted several pages to the optical properties of "Mr Bulkeley his Glasse" in his optical papers (now in British Library MS Add. 6789), paying particular attention to the place of its burning point. Harriot's calculational methods in these papers are almost unique in Harriot's optical remains, in that he uses both the sine law of refraction and interpolation from Witelo's refraction tables in order to analyze the passage of light through the glass. For this and other reasons, it is very likely that Harriot wrote his papers on Bulkeley's glass very shortly after his discovery of the law and while still working closely with Witelo's great *Optics*; the papers represent, perhaps, his very first application of the law. His and Bulkeley's interest in this giant glass conform to a long English tradition of curiosity about the optical and burning properties of large glasses, which grew more intense in late sixteenth-century England. In particular, Thomas Digges's bold and widely known assertions about his father's glasses that could see things several miles distant and could burn objects a half-mile or further away may have attracted Harriot and Bulkeley's skeptical attention; for Harriot's analysis of the burning distance and the intensity of Bulkeley's fantastic lens, it shows that Digges's claims could never have been true about any real lens (and this, I propose, was what Bulkeley had asked about in his original letter to Harriot). There was also a deeper, mathematical relevance to the problem that may have caught Harriot's attention. His most recent source on refraction—Giambattista della Porta's

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De refractione of 1593—identified a mathematical flaw in Witelo's cursory suggestion about the optics of a lens (the only place that lenses appear, however fleetingly, in the writings of the thirteenth-century Perspectivist authors). In his early notes on optics, in a copy of Witelo's optics, Harriot highlighted Witelo's remarks on the lens and della Porta's criticism (which he found unsatisfactory). The most significant problem with Witelo's theorem would disappear as the radius of curvature of the lens approached infinity. Bulkeley's gigantic glass, then, may have provided Harriot an opportunity to test out Witelo's claims about a plano-spherical glass, at a time when he was still intensely concerned with the problems and methods of the Perspectivist school.

1 John Bulkeley

In March 1591, Thomas Harriot, then working as mathematician to Walter Raleigh, received a letter from John Bulkeley of Mona (that is, the Welsh island of Anglesey). It accompanied a long mathematical treatise in which Bulkeley made an inevitably unsuccessful attempt on that perennially popular conundrum, the quadrature of the circle. He had been unwilling to share his work with the world (he told Harriot), but Harriot's own encouragement had led him to consider publication and to that end, he presented Harriot with a fair copy of the treatise. Even Harriot's patronage, however, he knew would not save him from all criticism. Archimedes had encouraged the mathematician Conon, who had nevertheless become the subject of envy and dissent; and Socrates was lampooned by Aristophanes, despite enjoying the patronage of Apollo! With these witty and flattering comparisons, Bulkeley urged Harriot to remain a staunch defender of his quadrature; should it ever come under attack, Harriot himself would be to blame, since it was only published with his encouragement.[1]

This is the only letter extant in what must have been a much more extensive correspondence between the two men. Harriot, notoriously, kept very few of his personal papers. And this aspiring Welsh mathematician left very little mark on history beyond this single sheet of paper, barely registering even among those modern researchers who have investigated Harriot's biography and intellectual connections.[2] It is possible, nevertheless, to identify him with some assurance. The Bulkeley family, formerly of Cheshire but by the time of this letter long settled on the Welsh island of Anglesey, had rapidly risen to prominence in the sixteenth century.[3] In 1534, Richard, the head of

[1] The original letter is extant, together with the quadrature, in a manuscript formerly held in Sion College with shelfmark Arc. L.40.2/L.40, now on deposit in Lambeth Palace Library. The letter is at fol. 280. Both this and two other manuscripts from Sion College (Arc. L.40.2/E.10 and Arc. L.40.2/E.6) belonged to Harriot's literary executor Nathanael Torporley, and include Torporley's own commentaries on Harriot's mathematics, as well as some notes (mostly alchemical) by Harriot himself. Bulkeley's letter to Harriot was edited in Halliwell-Phillipps (1841, 34). There are several mistranscriptions in this edition, most notably the valediction "ex aulula Mona" (from Anglesey Hall), which should read "ex *insula* Mona" (from the island of Anglesey).

[2] Bulkeley is not mentioned at all as a friend or associate of Harriot in Shirley (1983). Nor does he appear in the two major collections of studies on Harriot's life and work (Fox 2000, 2012). The only references to him in the literature on Harriot are both brief allusions to this manuscript letter and treatise, in Pepper (1967, 20), and Tanner (1977, 406–407).

[3] *The Dictionary of Welsh biography down to 1940* (1959, 57–58).

the family, was knighted and was succeeded by his son and grandson, each also named Richard and each also knighted (the family was ennobled early in the seventeenth century). The "John Bulkeley" who wrote to Harriot in 1591 was almost certainly a son of the second Sir Richard and his first wife Margaret Savage, styled John Bulkeley "of Cremlyn" (a house in Beaumaris, the seat in Anglesey of the Bulkeley family).[4] He was a younger brother of the most famous member of the family in this period, the third Sir Richard Bulkeley, who through land speculation and commerce became "the wealthiest gentleman in North Wales," to the extent that he was able to cast off the patronage of the Earl of Leicester and challenge directly the Earl's influence in the region.[5] John Bulkeley's oldest brother was born around 1540, so that John himself was most likely born in the first half of the 1540s.[6]

John Bulkeley was thus fifteen years older than Harriot, or more, when he sent him his letter and treatise. It is curious that the older man maintained such a deferential tone toward his addressee, particularly since Harriot, as the hired mathematician to a gentleman and the offspring of an obscure family, was hardly of the same social class. Bulkeley, however, recognized Harriot's learned status, comparing him to the princes of mathematicians and philosophers; he must have had some misgivings about presenting his work to such an educated man, particularly since there is no clear evidence that Bulkeley matriculated at either English university, even in an age in which ample records were maintained.[7]

It is quite likely that Bulkeley hoped to ingratiate himself to Raleigh, through Harriot's good offices. Yet, even taking that into account, there seems to have been a real intellectual connection between the two men; for in the closing years of the sixteenth century, Harriot (by now an employee of Henry Percy, the ninth Earl of Northumberland),[8] devoted considerable attention to an optical problem that Bulkeley had apparently posed to him and recorded his calculations in a small bundle of papers

[4] Griffith (2014, 42).

[5] Powell (2008). John Bulkeley is mentioned as part of this eminent family in Williams (1968, 108), where he and his brother Rowland, both brothers of the third Sir Richard, stand as trustees in the land settlement on the 1578 marriage of their sister Elizabeth to Owen Holland.

[6] One can assume that children in this family were born almost yearly. Besides John, his parents Richard Bulkeley and Margaret had ten children; by his second wife Agnes, Richard fathered a further ten (Burke 1866, 86).

[7] There is no reference to a John Bulkeley in this period, in Foster (1891) nor in Clark and Boase (1885). Likewise, he does not appear to have attended Cambridge (Venn and Venn 1922). These same lists attest, however, to the attendance of many other members of the Bulkeley family throughout the sixteenth century, and into the seventeenth. There is an intriguing possibility, however, that Bulkeley is to be identified with a "John Bowekeley" or Buckley, or Bulkeley of New Inn Hall who, in 1570, was arraigned and sent to the Tower under suspicion of debasing the coinage by alchemical means. A travelling stationer, William Bedo, had contacted him under the apprehension that Bulkeley could recover lost money by supernatural means; in the course of their acquaintance, Bulkeley read to Bedo a metallurgic experiment from della Porta's *Magia naturalis*, which led to a plan to dissolve and recover minute amounts of silver from silver coinage, while leaving the coins apparently unaffected; by this means Bedo accumulated 2 oz of silver. See Hart (1867, 391–394) and Ryrie (2008, 119). I am very grateful to Neil Ap Jones (editor of the website *Greats of Wales*) for sharing this information with me.

[8] For the origin and nature of their relationship (Harriot was classified as a "pensioner," not a "servant"), see Batho (2000).

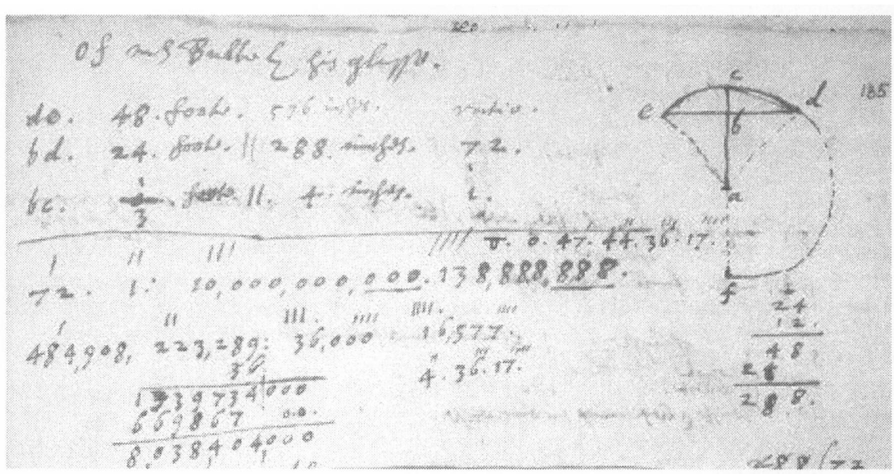

Fig. 1 Detail from BL MS Add. 6789, fol 135: parameters of glass

labeled "Of Mr Bulkeley his Glasse."[9] These papers, the principal focus of this paper, occupy an important place in the development of Harriot's optics, in which he made a significant break from medieval "Perspectivist" optics and employed, it seems, his newly discovered law of refraction for the first time.

We have no letter or record of a conversation in which Bulkeley posed his problem; yet, we can reconstruct it from Harriot's manuscript, beginning with the concise mathematical sketch of the apparatus that he made on a single page, a detail of which is reproduced in Fig. 1. His correspondent's question concerned a circular glass lens,[10] concave on one side, flat on the other. Harriot's description of the lens stated that it was to be 48 feet in diameter (*de*, in Harriot's diagram) and 4 in. thick at its thickest section (*bc*). Harriot's sketch of the lens (on the right side of Fig. 1) greatly exaggerated the bulge of the glass; drawn to scale, the lens would be virtually indistinguishable from a flat piece of glass. From these parameters, he calculated that the radius of curvature of the lens was 10,370 in., or 864 2/12 feet (*ca*; see calculation of *semidiameter sphaerae* in the detail reproduced in Fig. 2), and that the lens subtended $3^\circ 10'58''$ when viewed from its center of curvature.[11]

Bulkeley had suggested to his friend a lens with a diameter of 48 feet and 4 in. thickness—nice round numbers—and Harriot had gone on to calculate the other dimensions of the glass. Why did Bulkeley suggest Harriot explore the properties

[9] British Library, MS Additional 6789, fols. 132–141. The papers are misarranged (see "Appendix 1" for the likely correct arrangement, and for a short description of all the papers in this bundle); the title to the whole bundle, as cited, is on fol. 135.

[10] That the lens was made of glass, and not crystal (which would have had quite different refractive properties) is clear both from Harriot's rough calculation on fol. 141, which explicitly state "a vitro ad aerem" ("from glass to air"), and his use of Witelo's tables for refraction through glass, as detailed below.

[11] The lower third of the page (not reproduced) consists a series of long divisions to calculate "*de*" (by which Harriot means $\angle dae$ in his diagram) as $3^\circ 10'58''$ (which Harriot continues with meaningless precision to 25 thirds (sixtieths of a second of arc) and 9 fourths (sixtieths of thirds), as can be seen in the lower right corner of Fig. 2).

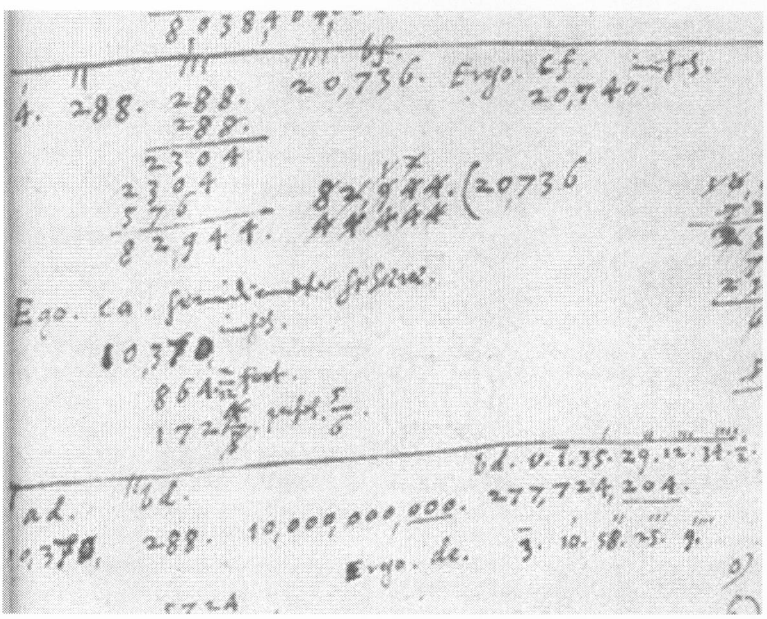

Fig. 2 Detail from BL MS Add. 6789, fol 135: calculation of center of curvature

of this lens, and what did he want to know about it? That question is answered in subsequent sheets in this bundle. Another, more immediate question may have occurred to the reader by this point, however: did Bulkeley possess such a glass? It would be easy to assume that "Mr Bulkeley his Glasse" was a real optical instrument that Harriot was examining, like "Mr W. Copes Cristall," sketched and described elsewhere in Harriot's papers: an imperfect, long triangular prism with which he conducted some experiments on refractive colors.[12] Harriot certainly did borrow, or commission, real glasses from friends and instrument makers, both in London, and in his own retinue in the Earl's house.[13] But this glass, without a shadow of a doubt, existed only in Bulkeley's imagination (and perhaps Harriot's too). Quite apart from the fantastic precision needed to grind a large lens with such a huge radius of curvature, which was far beyond the technology of Harriot's or any age, such a lens could not even have supported its own weight.[14] Lens craft reached that barrier with glasses that were a fraction of the size of Bulkeley's. The objective lens in the great 1895 refractor at Yerkes Observatory

[12] Ibid., fol. 148. These experiments perhaps formed part of the series of investigations of prismatic colors that Harriot conducted (and dated) in 1605 (fols 190–200). There is a discussion of Cope's crystal, and a full-page reproduction of this manuscript folio, in Gage (2000, 131–132). For the possible appearance of [Sir Walter?] Cope's prism, in a newly discovered masque by Ben Jonson, see Reeves (2010, 181).

[13] From around 1605 until his death, Harriot employed the spectacle maker Christopher Tooke to grind lenses and other optical devices for him on site (Shirley 1983, 382–383).

[14] Assuming that the lens was made of a substance similar to crown glass, which has a specific gravity of 2.54, one can calculate that Bulkeley's lens would have weighed 2919 lb; clearly a glass that was only 4 in. thick at its center, and only a fraction of an inch thick at the edges, would have shattered under its own weight.

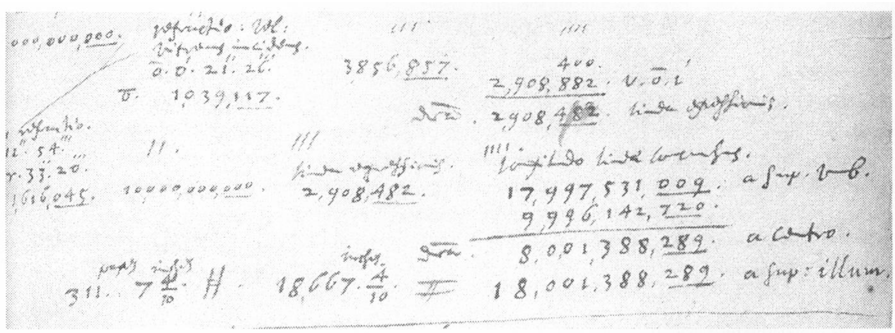

Fig. 3 Detail from 132r: calculation of burning point

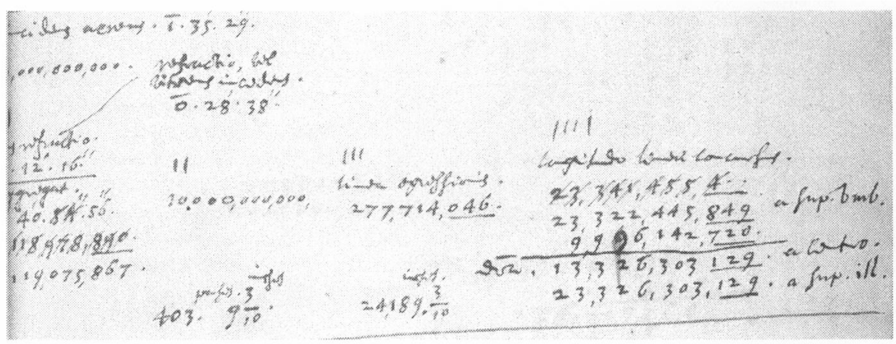

Fig. 4 Detail from fol. 133r: calculation of burning point

(the largest refractor in the world) had a diameter of 40 in. and represents to this day the upper limit of lens size: larger glasses sagged and cracked under gravity, and even the Yerkes objective proved to be maddeningly difficult to manufacture and mount.[15]

In the second part of this paper, I will attempt to answer *why* Bulkeley and Harriot were interested in a vast, imaginary lens. But first, the question of *what* they wanted to know about the lens. This question has a quite simple answer: Bulkeley wanted to know how far such a lens would be able to burn things, and this is the question that Harriot considered on fols. 132 and 133 of this bundle. He determined that the focal point of the lens would be found at a distance of 18,664.8 in. (on fol. 132; see Fig. 3) or 24,189.3 in. (on fol. 133, Fig. 4). (The fact that there were two *different* answers is crucial to understanding the place of these papers in Harriot's development and it will be examined below.) To put it another way, he found that the lens would ignite objects placed between 500 and 670 yards away—the upper limit a little more than a third of a mile distant. Harriot determined the burning point of the lens by considering the passage of a ray through the glass very close to its central axis, and at its very edge, tracing how it bent through the glass, and then following its route back to the

[15] Van Helden (1984, 51).

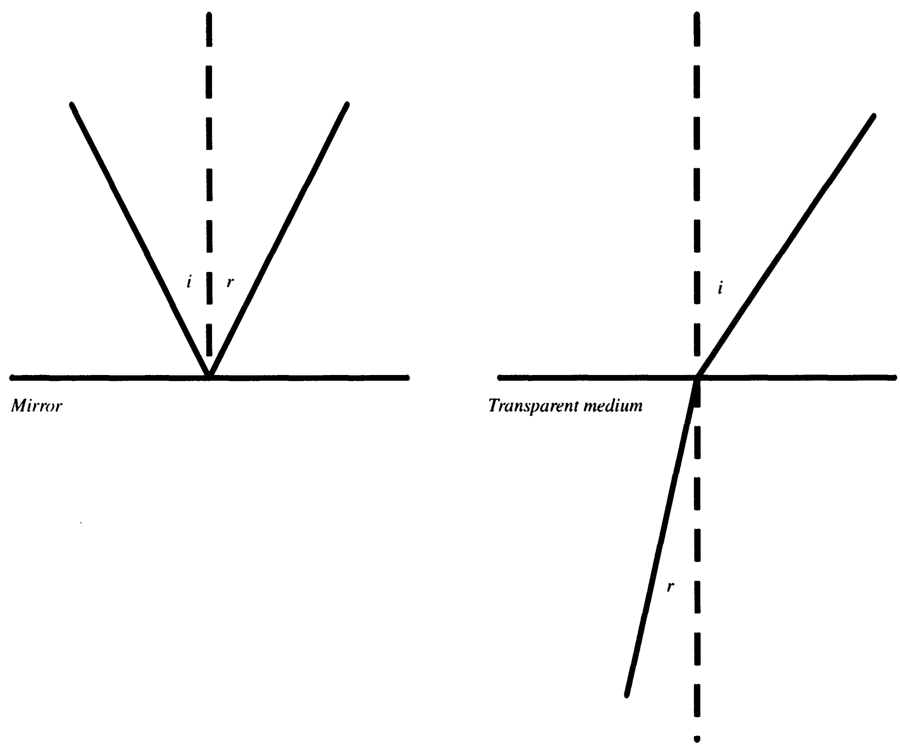

Fig. 5 Angles of incidence and reflection or refraction

axis.[16] He was able to do so because, at some point between 1597 and 1602, Harriot had discovered the long sought-after law that governed the refraction of light.

2 The problem of refraction: a digression

Since antiquity—indeed, before a science of "optics" had even been formulated—it was well known that a ray of light (or rather, for most ancients, a ray of *vision*) striking a mirror at a particular angle of incidence was reflected from the angle at exactly the same angle.[17] Refraction, the bending of light as it passes between different transparent substances, had also been observed since antiquity; it was a truism of the ancient skeptical school, for instance, that the apparent bending of rowers' oars in the water was a sign that the sense of sight could not be trusted.[18] Ancient and medieval theorists of light assumed that there was some regularity in the size of refractive angles, just as simple as that observed in *reflective* angles; yet, they found themselves quite unable to formulate it (Fig. 5). The greatest physical scientist of the Roman Empire,

[16] See "Appendix 2" for a detailed account of Harriot's ray-tracing through the lens.

[17] One of the most sensitive and comprehensive accounts of the early history of the law of reflection can be found in Takahashi (1992).

[18] For ancient optics as an attempted defense of sight against skeptical attacks, see Berryman (1998).

Claudius Ptolemy, carefully measured angles of refraction as light passed between air and water, air and glass, and glass and water, but could not enunciate a relationship between the angles; he included his tables of measurements in his *Optics*, however.[19] Some later theorists attempted to formulate a relationship that would emphasize the kinship between reflection and refraction: Damianos in late antiquity, for instance, asserted that, for any angle of incidence on any transparent medium, the light would be refracted at an angle precisely half the size. This rule was so evidently incorrect that it attracted hardly a single supporter; nevertheless, the later optical tradition remained convinced that some rule of this order of simplicity would eventually be discovered.[20] The most acute optical theorist before Kepler, the Egyptian ibn al-Haytham (Alhacen), described even more elaborate and sophisticated experiments than Ptolemy's to measure refraction, but came no closer to stating a rule, beyond what he took to be safe (but, as it turned out, not universally true) statements about the relative sizes of incident and refracted rays.[21] In Western Europe, among the late thirteenth-century optical theorists known collectively as the "Perspectivists," Roger Bacon and John Pecham contented themselves with a merely *qualitative* description of refraction (that light bends toward the perpendicular when passing from a less dense to more dense substance, for instance), while Witelo, after adding even further refinements to Alhacen's experimental designs, passed off Ptolemy's refraction tables as his own. It was through Witelo's presentation of the tables (into which he introduced some significant errors) that opticians of Renaissance Europe knew of Ptolemy's experimental results, which they continued to rely on well into the seventeenth century, even after Descartes had published the modern law of refraction.[22]

The most important development before Harriot came with Giambattista della Porta's *De refractione*, of 1593 (della Porta 1593). Della Porta was famous throughout Europe for his encyclopedia of "natural magic," which included a very influential chapter on the marvelous properties of lenses, mirrors, and the *camera obscura*.[23] His work devoted to the refraction of light was altogether more obsure. As Kepler wrote a decade after its publication in his great work on optics, *Ad Vitellionem paralipomena*

[19] For Ptolemy's original text, including his tables, see Smith (1996, 229–238). It is clear that Ptolemy did think he had discovered a mathematical relationship, and fudged his observations so that the relationship-constant second-order differences would hold; he did not state the relationship explicitly, because such an arithmetic rule could not be expressed using Greek geometrical language, and only such relationships could be considered "laws." For this argument, see especially (Smith 1982).

[20] Robert Grosseteste, in the early thirteenth century, independently formulated the same rule. See Eastwood (1970), and Eastwood (1967).

[21] Smith (2010a, lxiii–lxvii). The slightly earlier Baghdad theorist, ibn Sahl, formulated a construction for refraction that implied the modern sine law; see Rashed (1990); and, for an edition and translation of all the relevant Arabic texts on refraction (Rashed 1993). Rashed's thesis, that ibn Sahl's work influenced al-Haytham and the subsequent tradition of Arabic optics, has been convincingly challenged in Smith (2010a, lxv–lxvii, lxxxii–lxxxiv); he argues, in effect, that ibn Sahl's treatise seems to have remained unread or unnoticed by all later Arabic writers, and had no effect on the future development of optics.

[22] For Witelo's maladroit versions of Ptolemy's tables (which were already of suspect value), and the unwarranted authority they held for some four hundred years, see Lohne (1968).

[23] This is in book 17 of the longer recension of the work, first published as della Porta (1589) and subsequently reissued in dozens of editions, and translated into every major European language. On della Porta and the "secrets" tradition, see Eamon (1994).

(1604), he had long searched for a copy and had never been able to locate one.[24] It was perhaps to the benefit of della Porta's reputation that the work was known more by repute than by acquaintance; for, although it contains moments of insight, the work as a whole is one of the most bizarre and misguided attempts on the problem of refraction ever devised.[25]

Della Porta opened the main section of his *De refractione* with an analysis of the *reflection* of light in a spherical mirror. His work in this part of the treatise is a model of clarity and accuracy. In fact, he provided the first correct description of the focusing power of the mirror since antiquity, showing that rays close to the axis of the mirror are reflected near to a point halfway between the center of curvature of the mirror and its surface. He was correct both in his location of the focus and in his recognition that the spherical mirror focused only imperfectly, the phenomenon known as "spherical aberration." The ancient optician Diocles and Alhacen had both demonstrated the same fact, but in works that became known to Western Europe only in the modern period. The extant ancient and Arabic works on the spherical mirror that were known to the Perspectivists and Renaissance optics (including even Euclid's *Catoptrica*) provided confused and often self-contradictory information about the focal properties of the mirror.[26]

Della Porta's treatment on the burning mirror was thus of great value, and Harriot recognized as much in his own work on the concave spherical mirror.[27] But the only reason that della Porta was interested in the spherical mirror in a book on *refraction* was that he used the mirror to explain the passage of rays through a spherical glass ball and a concave lens-like glass. Figure 6 shows della Porta's construction for refraction in a glass ball (*De refractione* II.2), which della Porta claimed acted analogously to a spherical mirror. If a ray is incident from P, to the left of the ball, we are to imagine that there is a spherical mirror fitting around the right-hand half of the ball (at XWZ, sharing the center O of the glass ball). If the imaginary mirror would *reflect* the ray to point T on the axis, then (della Porta asserted, without a word of justification) the ball will *refract* the ray to the symmetrical point outside the ball (to point R, so that R and T are equally distant from W, the "back" of the ball). He made a similar statement for the concave shape in Fig. 7. We must imagine that a refracting glass ball ($XYZW$) is fitted into the cavity XYZ. If the glass ball (according to della Porta's

[24] As Zik (2003, 490) and Smith (2010a, ccxxiii) have argued, however, Kepler's summary in the *Paralipomena* of della Porta's theory of the eye was derived from the *De refractione* (of which he must have obtained a *précis*, if not the text itself), and not merely the easily available *Magia naturalis*.

[25] There is still little modern scholarship on della Porta's *De refractione*. Lindberg (1984) surveys della Porta's refractive theory, and comes to very critical conclusions about its value. Smith (2010a, xciii–xcvii) covers much the same ground, but concludes (a little perversely, I think) that della Porta's method was promising, and may have informed Kepler's approach to the analysis of lenses. Dupré (2005, 167–170), while acknowledging the "arbitrary geometry" used by della Porta in his optics, makes a convincing case that his account of the optics of lenses in *De refractione* belongs within the tradition of northern Italian optics associated with Ettore Ausonio, whose influence Dupré's various publications have done so much to illuminate.

[26] See Knorr (1983).

[27] Harriot's calculation throughout his notes on the spherical mirror of the axis intercept of a ray incident at angle θ as $1/2 \sec \theta$ was derived directly from proposition II.1 of the *De refractione* (pp. 36–41). His explicit statement of the rule at MS 6789, fol. 390, uses the same diagram as della Porta in II.1.

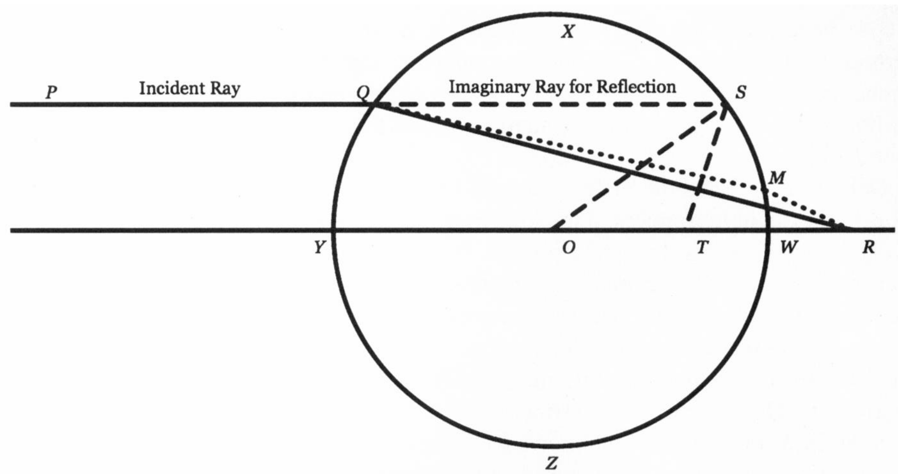

Fig. 6 Della Porta's construction for refraction in a glass sphere (after De refractione II.2, pp. 42–43)

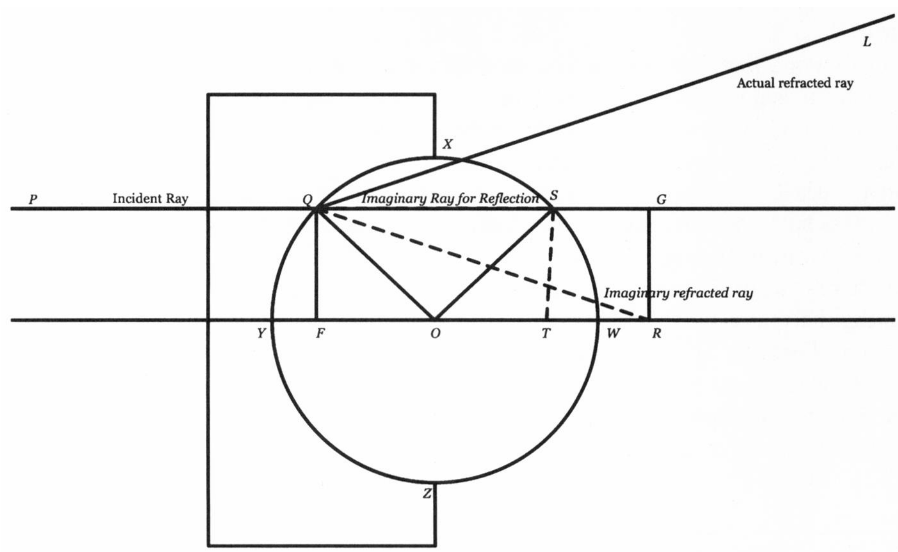

Fig. 7 Della Porta's model for refraction in concave glass (after De refractione II.4, pp. 43–45)

method in the previous proposition) would refract along line *QR*, then (claimed della Porta, again without a word of justification) the concave instrument will refract along *QL*, the mirror image of *QR* in the continuation of the original incident ray, *PQS*.

Della Porta's treatment of refraction has some obvious peculiarities. He gave no demonstration that rays were refracted in the way he claimed—he did not even attempt to justify his models, in particular the importation of the spherical mirror into the refractive constructions. Perhaps the long-held belief that refraction *should* be analogous to reflection seemed sufficient grounds for his assertions. What is more, his model was not particularly accurate in predicting what a real glass ball or concave lens would actually do;[28] and took no account of the fact that refracting balls made of different substances (glass, lead crystal, water) would refract in an entirely different way, a fact known since antiquity.[29]

There is something yet more puzzling about his constructions, however. Della Porta's attention was directed entirely to finding the point at which the emergent ray cuts the axis of the instrument (in the case of the glass sphere). He entirely ignored what happened in between the point that the ray contacted the ball (at point Q in Fig. 6), and its final arrival at the axis (at R): note that the "path" of the ray, QR, passes out of the ball without being bent at the glass–air interface. By determining the final trajectory of the ray through an *a priori* analogy with a mirror, della Porta rejected the fundamental method of ancient and medieval (and even modern) optics: ray analysis, in which the theorist determines the effect of a glass by tracing the ray from interface to interface, whether reflective or refractive. Every optical treatise on mirrors and combinations of mirrors that della Porta could have read worked in this way. And refraction could be treated the same way, although with more difficulty, even without an adequate theory of refraction; Alhacen and Witelo both stated and proved theorems in which they ingeniously managed to trace quite accurately the path of light ray through a glass sphere, refraction by refraction, even though their understanding of refraction was qualitative and imprecise.[30] Della Porta, on the other hand, treated his instruments as a kind of "black box:" light entered one side, *something* happened to it in the middle, and it exited the other to reach the point predicted by his model.

An interesting question could be posed to della Porta, however: what mathematical relationship would have to hold between the angle of incidence (i) and refraction (r), such that a ray passing through these instruments would emerge at just the place predicted by his model? (The dotted line QMR in Fig. 6 shows such a ray). Or, to put it another way, what would happen if we insisted on using traditional ray analysis on della Porta's models? Della Porta never offered an answer—if the question even occurred to him—but it *is* in fact possible to derive a relationship. Light would travel through the glass ball to just the place that della Porta predicted if this relationship held between the angles of incidence and refraction:

$$\sin 2\,(i-r) = \frac{\sin 2i}{4\cos i - 1}$$

And if one analyzes the concave glass using the same method, the following relationship is found to hold:

$$\tan(r-i) = \frac{\sin 2i}{\cos i + \cos 2i}$$

[28] See Smith (2010a, cxxii) for a comparison between della Porta's predictions and actual refractions.

[29] Lindberg (1984, 144). Smith has shown (2010a, cxxi–cxxii) that della Porta locates the focal point of the refracting sphere at just the right point for a sphere made of crown glass; but this is quite accidental, since della Porta actually specifies that the ball should be made of crystal, which has a much higher refractive index than glass!

[30] These important theorems have been examined in detail recently by Smith (2010b).

In other words, della Porta's constructions would have implied two quite *different* laws of refraction.[31]

3 Harriot, della Porta, and refraction

Della Porta never asked what law of refraction emerged from his peculiar models, but Harriot, one of the very few readers of *De refractione*, did. After asking this question, and examining the answers mathematically and experimentally, Harriot was led to discover the modern law of refraction. Yet, it is worth reflecting here on the peculiar nature of a scientific discovery that is never revealed; and how that permanently our approach to and understanding of the discoverer.

Since Harriot's own time, it had been known that he had measured the refractive properties of various substances. In an exchange of letters with Johannes Kepler, Harriot even revealed some of his experimental data and promised (rashly, as we now know) that a full treatment of the problem would soon be ready for publication.[32] But modern interest in Harriot's work on refraction really began in 1951, when John Shirley published a short note in which he showed that Harriot was more than just a careful experimenter. After the death in 1640 of Walter Warner, Harriot's longtime collaborator, the mathematician John Pell made a record of conversations he had had with Warner on mathematical subjects.[33] One recollection was particularly significant:

> Mr Warner says that he had of Mr Hariot this proportion: As the sine of one angle of incidence to the sine of its refracted angle, by experience; so the sine of any angle of incidence upon the superficies to the sine of its refracted angle, to be found by supputation.[34]

That is, once experiment had established that some ray incident at angle i_1 was refracted at angle r_1, it was possible to find the angle of refraction r_2 for any other ray incident at i_2 by the relation

$$\frac{\sin i_1}{\sin r_1} = \frac{\sin i_2}{\sin r_2} = K$$

where K is a constant for a particular pair of media. This was a spectacular discovery—both for Harriot and for Shirley, who would go on to write the standard biography of

[31] Lohne must have performed the same analysis of della Porta's constructions: in (1963, 158–159) he states these formulae (without demonstration) with the implication that they were della Porta's explicitly stated refraction laws. A complete derivation of the formulae will be found in "Appendix 3" to this article.

[32] Goulding (2012).

[33] British Library, MS Birch 4407.

[34] Shirley (1951, 507). The passage went on to describe an experimental demonstration of this relationship which he (Warner) had performed. Shirley assumed this was a description of Harriot's method. However, it seems to be identical to the method described in one of Warner's manuscripts (British Library, MS Additional 4395), where it is stated that this experiment was performed by Warner and Sir Thomas Aylesbury (another one of Harriot's associates) only after Harriot's death.

Harriot.[35] This was a solution to the millenia-long search for a simple mathematical relationship that would govern refraction. Moreover, it was identical to the modern law of refraction, supposedly discovered by Willebrord Snell around 1621, and first published by Descartes in 1637; here was apparent proof that Harriot, who died in 1621, had discovered the law first. The evidence, however, was questionable. The report was at third hand, based on the testimony of one of Harriot's most loyal supporters. In addition, it was written twenty years after Harriot's death, when the Cartesian *Dioptrique*, and hence the law of refraction itself, were already well known. Pell himself even observed (later in the same reported conversation) that this proportion was the same as that used by Descartes. Shirley, who was little inclined even in his biography of Harriot to tackle the "eight great folio volumes" of Harriot's notes, concluded that, if there were in fact any refraction experiments or determinations of a law, the original records had probably perished.

Harriot's priority in the field of refraction was independently rediscovered by Johannes Lohne in 1959.[36] Lohne was researching the history of refraction and found in the Oslo University Library a copiously annotated printed copy of Witelo's *Optics*. In the back of the volume, the reader had constructed comparative table of refractions. The writer summarized the results of Witelo and had also transformed Giambattista della Porta's second model into a mathematical law of refraction, and tabulated the predicted refractions alongside Witelo's.[37] Finally, he included his own observations of refractions from air to glass and water, experiments which he dated August 1597 and February 1598, and noted had been performed at "Sion." Elsewhere in the book, next to Witelo's description of a peculiar optical phenomenon (seeing one's own image approach when walking in the fog), the reader noted in the margin: "T. H. Ego talem vidi, an. 158(?) august. 29 in aedibus regiis otelands" ("I, T. H., saw such a thing on August 29, 158? at the royal estate at Otelands"). From this annotation, and the reference to "Sion," Lohne identified the annotator as Harriot, mathematician to the Earl of Northumberland, whose London residence was Sion House.[38]

Lohne's curiosity about Harriot was piqued, and he turned his attention to his optical papers in the British Library.[39] On the very first sheets, he found tables of refractions for various media, for every degree between 0° and 90°; the sines of angles of incidence and refraction were in the same proportion in each table, which had

[35] Shirley (1983).

[36] Lohne (1959).

[37] That is, he derived and applied the tangent formula on p. 15 above. This is the only known instance of a reader of the *De refractione* extracting the formula implied for ray analysis. Curiously, in none of Lohne's articles on Harriot or refraction does he mention that Harriot applied this formula—despite the fact that Lohne seems to be the only modern scholar who has derived the formula from della Porta's constructions. See further in "Appendix 3" below.

[38] Oslo University Library Lib. rar. 790f (of which there is a photocopy in folder 75 of John Shirley's papers, in the University of Delaware library. A copy is also available online at http://ub-prod01-imgs.uio.no/minuskel/11g136514).

[39] The second half of Additional MS 6789.

obviously been calculated.[40] Elsewhere in the volume, he found tables of observed refractions dated 1602; they were far more accurate than the 1597 observations, and each table now had an extra column for the refraction *per calculum* ("by calculation"). These calculated values were, of course, found by the same sine law of refraction, which (Lohne concluded) Harriot must have discovered in the period 1597–1602. In his first article on Harriot and many subsequent studies of Harriot's optics, Lohne demonstrated beyond any doubt Harriot's priority in discovering the sine law and went a long way to explain *how* he discovered it.[41] He also explicated some of Harriot's most important *applications* of that law, such as his analysis of the nature of the rainbow.[42]

This brief account of modern scholarship's rediscovery of Harriot's engagement with refraction reveals some important characteristics both of Harriot as a problem in the history of science and of Harriot's development as an optical scientist. It is, perhaps, surprising that the matter of fact of something so momentous as the discovery of a fundamental physical law needed to be *established*, by following up on the rumors of Harriot's achievements. We could not imagine wrangling over whether Newton, for instance, had actually formulated a law of universal gravity, or whether that was only a story circulated by sentimental friends after his death. But Newton, of course, was a public figure and public scientist; for all his many quirks of personality, he published his *Principia* and *Opticks*, and (to say the least) was prepared to acknowledge them in the face of criticism. With Harriot, we are dealing with an altogether more private person, all of whose work remained in obscurity, while yet generating (as one scholar memorably put it) a "legacy of hearsay."[43]

Lohne's brilliant detective work, on the trail of Thomas Harriot, also illuminates the path by which Harriot made his way to refraction: through the intensive study of Witelo's ancient results on refraction, and through the deconstruction of della Porta's refraction models in order to discover the universal mathematical regularity between angles of incidence and refraction that lay behind them. Harriot seems to have been the only person to attempt the latter task (he was, in any case, one of the very few readers at all of the *De refractione*). It is very likely significant that Harriot discovered that a *trigonometric* law was implied by della Porta's constructions—but one of almost absurd complexity; his own law would propose a radically more simple, yet still trigonometric relationship. Finally, in the blank pages of the Oslo Witelo, we see him comparing the results of Witelo and della Porta, and also (again, in this, probably one of the very few who had ever done so) making his own precise measurements of refraction—through which he discovered that both Witelo *and* della Porta were quite far off the mark.

40 MS 6789, fol. 88r. Taking a random line, the angle of refraction corresponding to an angle of incidence of $31°$ is $19°19'59''$. The ratio of the sines of these angles (and of *all* the pairs of angles in the table) is $0.642785 = \sin 40°$. The value of $\sin 40°$ (or, rather, its reciprocal) is what we would now call the refractive index of the glass. (Harriot's glass thus had a refractive index of approximately 1.55, well within the range of indices for crown glass).

41 In Lohne (1959); and especially Lohne (1973, 1979).

42 Lohne (1965).

43 Tanner (1967).

One other thing should be noted from this survey: as exemplary as Lohne's scholarship was in almost every way, he was interested almost exclusively in the question of the law of refraction and in other problems of great subtlety that coincided with modern scientific interests (the nature of the rainbow, for instance). An apparently absurd problem like Mr Bulkeley's fantastic glass did not attract his attention, nor that of any other scholar. But by focusing on the high points of the papers—the tabulated law, the careful measurements—and ignoring the hundreds of pages of scribbled problems and calculations, Lohne and other scholars missed both the context in which Harriot proposed his law and, ironically, one of the important turning points in the development of the law.

For, in fact, the papers on Bulkeley's glass are very odd, and not just because of the imaginary subject matter. Harriot, as noted above, worked through the problem twice and obtained two quite different answers. In one set of pages,[44] he calculated the focal point of the glass using his sine law of refraction. In the other set of pages,[45] he performed the same calculations, but, in order to find the refraction at each interface, instead of using the sine law, he derived his refraction values from Witelo's tables of refraction, using simple linear interpolation.[46] His use of this method suggests, for several reasons, that the Bulkeley's glass papers represent an early stage of his optical work. First, there is only one other place in any of his extant papers that Harriot used the same method: in some papers that concern a similarly huge, flattened cone of a glass, a problem which he seems to have picked up as a simple approximation to Bulkeley's spherical glass.[47] As it happens, it is possible to say with certainty that these papers on the refractive cone were written earlier than most of the other optical papers. In one corner of fol. 353, Harriot calculated the value of versin 16′—that is, $(1 - \cos 16')$. The correct value should be .0000108308; we can observe Harriot making a carrying error, however, which leads him to the incorrect answer of .0000118308 (see the upper left of the detail reproduced in Fig. 8). This parameter is fundamental to Harriot's analysis of burning glasses and is copied from this page into dozens of what must be subsequent bundles of papers.

The peculiar versin expression leads us to the final part of Harriot's analysis of Bulkeley's glass. At the bottom of the two pages, he neatly wrote calculations about the "angle of a cone" (*angulus coni*), involving the erroneous value of versin 16′ (details from both reproduced in Fig. 9).[48] It would take us too far afield to explain in detail what is going on here; but very briefly, 16′ is the angular measure of the radius of the sun, and his calculation at the bottom of the page was his attempt to quantify the burning power of the instrument in multiples of the sun. In the equally early flat

44 Fol. 132, and the associated working papers, as listed in "Appendix 1".

45 Fol. 133, and others as listed in "Appendix 1".

46 Thus, on these pages he states that a 1° ray is refracted at an angle of 42′. Witelo's table says that a 10° ray is refracted at 7°; 42′ is one tenth of 7°. Full details of Harriot's calculations are in "Appendix 2" below.

47 MS Add. 6789, fols 348–361. One of the glasses, considered on fol. 352r, has very similar dimensions to Bulkeley's glass: a radius of 25 feet (compared to Bulkeley's 24), and a thickness at the center of 3 in. (compared to Bulkeley's 4).

48 The little "u" without a tail is Harriot's usual symbol for "sine"; so that "u versus" means versin, or 1-cosine.

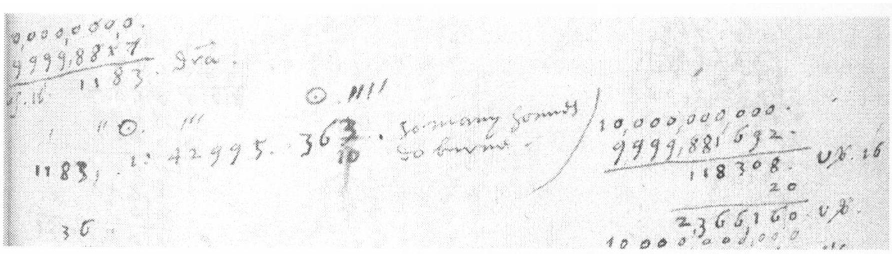

Fig. 8 Detail from fol. 353r: Harriot's error in calculating versin 16′

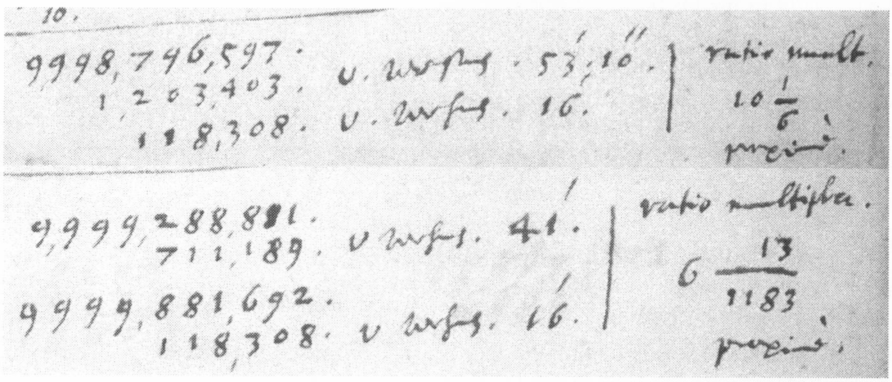

Fig. 9 Details from 132r and 133r: calculation of burning power, using the two methods of refraction

conical glass pages, he gave this parameter the title "so many sonnes doe burne" (see Fig. 8), a concept that he would elsewhere in the manuscripts express with the more concise Latin phrase "tot soles." He concluded here that the glass, by the sine law, burns with the power of 10 suns, but only 6 suns using Witelo's numbers.[49]

The very fact that he did not use the otherwise ubiquitous term "tot soles," and referred to the "angle of the cone" (of radiation from the surface of the glass), as if to explain to himself what he was calculating, again suggests that there are early sheets. Elsewhere in the manuscripts, he scrawled *tot soles* calculations almost by rote, sometimes several times on a single page. But far more significant for the dating is the fact that he labored through all the calculations using both the sine law *and* interpolation from Witelo's time-hallowed tables. His double calculation is a second indicator (after the connection to the early conical glass papers) that the notes on Bulkeley's glass were written early—in fact, very likely soon after his discovery of the sine law. Harriot would go on to write out degree-by-degree tables of refraction calculated according to the sine law, having discovered that the results obtained in experiment after experiment tallied, with stunning accuracy, with his calculations of sines; once he had become confident with his law, it would be hard to imagine him bothering to turn back to Witelo's tables, which squared so poorly with his experimental

[49] The calculation of *tot soles*, ubiquitous in Harriot's optical papers, and its central significance in Harriot's optical researches, is one of the main topics treated in my forthcoming work on Harriot's optics.

data, and repeat a series of onerous calculations. The pages on Bulkeley's glass look like the work of someone with a *new* law, who nevertheless still half-relied on the old. His work on Bulkeley's glass (and the closely related work on the flattened conical glass) very likely represent his first application of the sine law, after its discovery; and his use of both methods in a "real" problem provided an opportunity for him to test out the difference that his new law would make. His efforts, we must imagine, paid off; for, as we have seen, the sine law predicted a significantly different result for the burning position of the lens.

There is a great deal of surmise and triangulation involved in placing these papers in relation to the rest of Harriot's optical work. But several independent lines of argument point to the strong possibility that we are seeing in these pages the first outing of a fundamental physical law: paradoxically enough applied not to a real experimental instrument, but to a vast, imaginary flight of fancy he shared with his circle-squaring correspondent! The question "why," raised at the beginning of this paper, now intrudes itself with even more urgency than before. Why did he choose to apply his new discovery to this imaginary, even ridiculous problem? In the rest of the paper, I will take two approaches to answering this question. In the first, I will sketch out what might be called a cultural context for Bulkeley's glass: what could such a huge glass "mean," to Bulkeley, or to Harriot? In the second, I will consider a *mathematical* context: what outstanding problems in mathematical optics could have inspired Harriot to investigate this glass so thoroughly? Was there some mathematical or scientific reward to working through this problem, beyond satisfying the curiosity of his correspondent?

4 The cultural context

In a letter revealing glimpses of "the secrets of art and nature," the thirteenth-century English Franciscan Roger Bacon, founder of the Perspectivist school of medieval optics, promised great marvels from lenses and mirrors:

> Perspective glasses can be constructed in such a way that things located far away can appear very close, and vice versa; so that we are able to read the tiniest letter from an incredible distance, and we can count things however small, and we can make the stars appear wherever we want. It is believed that this is how Julius Caesar, from the coast of Gaul, was able with the aid of huge mirrors to discern the arrangement and location of the forts and cities of Great Britain.[50]

And in his *Opus maius*, the work he wrote in part to inform the Pope of the technological precautions that would need to be taken for the end times, he wrote that "[the parabolic] mirror would burn fiercely everything on which it could be focused. We are to believe that Antichrist will use these mirrors to burn up cities and camps

[50] Roger Bacon, *Epistola de secretis operibus artis et naturae, et de nullitate magiae* 5 (Bacon 1859, 534): "Possunt enim sic figurari perspicua ut longissime posita appareant propinquissima, et e converso; ita quod ex incredibili distantia legeremus minutissimas literas, et numeraremus res quascunque parvas, et stellas faceremus apparere quo vellemus. Sic enim aestimatur Julius Caesar super littus maris in Galliis, deprehendisse per ingentia specula dispositionem et situm castrorum et civitatum Britanniae maioris."

and armies."[51] We can see that Bacon's fantasies of technological magic were just that: imaginative extrapolations from the technology and natural philosophy of antiquity and his own age, sometimes uncannily prescient, as when (in the same letter on *Secrets*) he imagined flying machines and underwater ships. Earlier readers, however, were less certain whether they were reading fact or fiction, and in England, a myth of Bacon as a superhuman magus quickly developed, which culminated in popular printed literature of the late sixteenth century such as the anonymous *Famous Historie of Fryer Bacon*, and Robert Greene's *Honorable Historie of Frier Bacon, and Frier Bongay*.[52]

Even the learned took some account of Bacon's supposed marvelous feats. While they tended to ignore the wild claims of levitation and enchanted talking heads, they seemed to find the claims about mirrors quite plausible. As early as 1385, the Franciscan chronicler Peter of Trau recorded that Bacon had two glasses, one that could ignite candles by sun or moonlight, and another that would reveal misfortune anywhere in the kingdom:

> He was so complete a master of optics that from love of experiment he neglected teaching and writing and made two mirrors in the University of Oxford: by one of them you could light a candle at any hour, day or night; in the other you could see what people were doing in any part of the world. By experimenting with the first, students spent more time in lighting candles than studying books; and seeing, in the second, their relatives dying or ill or otherwise in trouble, they got into the habit of going down, to the ruin of the University, so by common council of the University both mirrors were broken.[53]

Robert Recorde, the pioneer of mathematical education in Tudor England, had to devote some energy to persuading his superstitious countrymen that there was nothing diabolical in mathematical diagrams or geometrically inspired technology.[54] The myth of Bacon seemed to him an ideal example of purely natural "magic." As he wrote in his *Pathway to Knowledge*, the first English vernacular textbook of geometry and its applications:

> Great talke there is of a glasse that [Bacon] made in Oxforde, in whiche men myght see thynges that were doon in other places, and that was iudged to be done by power of euyll spirites. But I knowe the reason of it to bee good and

[51] *Opus maius* IV.2.2: "Nam hoc speculum potenter combureret omne quod posset objici. Et credendum est quod Antichristus his utetur, ut civitates et castra et exercitus comburat." Latin text from Bacon (1987, 116); translation from Bacon (1928, 135).

[52] On these two texts and their sources, see Molland (1974).

[53] Power (2006, 660). For the original Latin text, see Molland (1974, 446–447). For the English interest in Bacon's account of marvelous mirrors, see Reeves (2008, 15–30 and 58–70) on the legend of Bacon, Dee's great mirror and Digges's telescopic claims (discussed below); *passim* on parallel myths from antiquity to the middle ages of mirrors that warned of danger.

[54] On Recorde's role in combating a popular association of mathematics (including optics) with demonic magic, and his defense of Roger Bacon, see Zetterberg (1980, 86–90). On Recorde's mathematical and medical career in general (Williams 2011).

> naturall, and to be wrought by geometrie (sythe perspectiue is a parte of it) and to stande as well with reason as to see your face in common glasse.[55]

In Oxford itself, the young mathematician Henry Savile bolstered the credibility of the legend by including it in his famous 1570 lectures on the history of mathematics. Bacon, and optics in general, fitted into the patriotic English cast he attempted to give to his history:

> It was first from this country that John Pecham, Archbishop of Canterbury, published his *Perspectiva communis*, preoccupied though he was by the affairs of state. In almost the same century, Roger Bacon imitated his industry. After he had exhibited that marvelous mirror of his—an image, one might say, of his genius and learning—to his fellow Oxonians, he inspired the whole world with enthusiasm for studying optics, so demonstrating that things could be achieved very easily by the optical philosophy that to others seemed very difficult, or even impossible.[56]

A little later, Reginald Scot, in his *Discoverie of Witchcraft* (published in 1584), introduced into the English optico-magical discourse the specular illusions of della Porta's *De magia naturali* (in its shorter, first edition), to which he added a deeply felt moral purpose lacking in its model. Scot wrote his book, he explained, because he was appalled to see unfortunate wretches persecuted as witches. But where, he asked, was the evidence for witchcraft? As far as he could determine, these so-called witches were not in league with the devil, but had been singled out because they were poor, or resented by others, or led odd, solitary lives, or—in some cases—actually had inexplicable powers. But these powers were not obtained through demonic intercession; rather, they were examples of *natural* magic. Scot devoted himself to showing how almost *anything* that these reputed witches were accused of could be explained either through the action of wholly natural causes, or through some deception or sleight of hand—they were dishonest, perhaps, but not diabolical. In the process of compiling his treatise, Scot gathered everything he could learn not only from books like della Porta's, but also from performers he saw in the marketplace, until his work became a veritable encyclopedia of illusion and trickery. It is unlikely that any hustlers were threatened with the stake for performing the three-card monte; but, in case they were, detailed instructions revealing how that scam worked could be found here as well.[57]

[55] Recorde (1551, sig. t3r (Preface)). Cited by Baxandall (1923, 305).

[56] Bodleian Library, MS Savile 29, fol. 15v: "hinc primum tot reip[ublicae] districtus occupationibus Joannes Peccham cantuariensis archiepiscopus perspectivam communem aedidit. cuius industriam eodem ferme seculo imitatus Rogerius deinde Bacho postquam suum illud mirificum speculum, tamquam ingenii et doctrinae specimen suis oxoniensibus proposuisset, totam orbem perspectivae discendae incendit[,] Optica philosophia facillimum illud esse demonstrans, quod aliis arduum negotium imo plane adunaton videretur."

[57] Scot's book was, in fact, the Urtext for the entire English tradition of conjuring books. Samuel Ryd plagiarized parts of the *Discoverie of Witchcraft in his Art of Jugling* of 1612, the first English book devoted entirely to stage magic. This in turn was subsumed into the anonymous *Hocus Pocus Junior* of 1634, which subsequently appeared in dozens of editions through to the eighteenth century. See Hall (1972) 47–62 (on the *Discoverie*), and 120–132 (on the tradition of *Hocus Pocus Junior*). See also Toole-Stott (1976, 125) for the reliance of *Hocus Pocus Junior* on Scot. On Scot, and the reception of the book in larger English society, see Almond (2011).

Scot's greatest interest, and certainly his expertise, was in sleight of hand. But there was also a brief chapter on "divers woonderfull experiments, and of strange conclusions in glasses, of the art perspective &c."[58] The chapter is bizarre in places, reflecting the mish-mash of natural and (as we would see them) magical experiments that comprised most medieval works on illusion, as well as della Porta's Renaissance compilation.[59] For instance, Scot borrowed from della Porta some methods of making an animal's head appear on a man's body—a trick with its roots in early Greek antiquity, if not earlier. One recipe involved burning the animal's semen, and using it as an ointment: "and any bodies face therewithall annointed," Scot assured his reader, "he shall seeme to have the like face as the beast had," an effect that Scot insisted proceeded from natural causes. To make the trick more effective for the audience, one could throw in some of the comical incantations usually associated with a "witch or papist."

But, astonishing as such tricks might be, Scot was far more interested in the wonders *mirrors* and other glasses could perform. His long catalogue of these optical illusions was, in almost every case, taken from della Porta's work on natural magic. "Some glasses are so framed," he began, "as therein one may see what others doo in places far distant; others, whereby you shall see men hanging in the aire; others, whereby you may perceive men flieng in the air," and so forth (Scot 1584, 316). He concluded this chapter by referring the curious reader to Witelo and della Porta, but added that these illusions were not merely bookish fantasies: "These I have for the most part seene, and have the receipt how to make them: which, if desire of brevitie had not forbidden me, I would here have set downe" (Scot 1584, 317).

Scot's claim to have witnessed such illusions and marvels of telescopic vision sounds like a bluff; but a group of contemporary English mathematicians may have provided at least two opportunities for him to see mirrors and lenses in action. Thomas Digges, in a passage that has become notorious in modern scholarship, claimed that his father Leonard had built some sort of optical device for seeing at a distance, which could also be adapted to concentrate the sun's rays and ignite gunpowder at a great distance:

> But to leave these celestiall causes and things doone of antiquitie long ago, my father by his continual painfull practises, assisted with demonstrations *Mathematicall*, was able, and sundrie times hath by proportionall Glasses duely situate in convenient angles, not onely discovered things farre off, read letters, numbred peeces of money with the very coyne and superscription thereof, cast by some of his freends of purpose uppon Downes in open fieldes, but also seven myles off[f] declared wat hath been doon at that instante in private places. He hath also sundrie times by the Sunne beames fired powder, and dischargde Ordinaunce halfe a myle and more distant, whiche things I am boulder to reporte, for that there are yet living diverse (of these his dooings) *Oculati testes* [i.e., eyewitnesses], and

[58] Scot (1584, 315–317) (book 13, ch. 19).

[59] See Goulding (2006) for the text and translation of a medieval compilation of illusion experiments, and the categories of "magical" and "natural."

many other matters farre more straunge and rare which I omitte as impertinente to this place.[60]

Digges makes, we note, two principal claims in this passage: that his father's apparatus was able to see minute details of things at seven miles' distance, and that it could ignite a fire from half a mile. We must assume that the latter claim concerned *one* glass in isolation, because of the difference between the power of the his instrument to see *versus* the much smaller distance of ignition (one fourteenth as far). The "telescope," on the other hand, must have been (imagined to be)[61] compounded of two or more glasses, the most powerful of which individually had a half-mile focal distance.

Such an distinction between the individual glass (for burning) and the compound glass (for telescopy at much greater range) is supported by William Bourne's manuscript on "the properties and qualities of glasses," a research proposal and plea for patronage that he wrote to William Cecil, Lord Burghley in around 1585.[62] Bourne wrote at length of the focal qualities of converging mirrors and lenses and, in the final chapter, suggested how such glasses might be used to accomplish "those things that Mr Thomas Digges hathe written that his father hathe done." The experimenter was to take several concave mirrors and convex lenses, and arrange them so that the first mirror or lens sent its beam into the second, the second into the third, "and so reseaved from one glasse into another, beeynge so placed at suche a distance, that every glasse dothe make his largest beame." In other words, Bourne imagined (as perhaps did Digges, *père et fils*) that the power of the individual concave mirror or convex lens (glasses which actually do magnify small things close at hand)[63] could be doubled and redoubled by combining the glasses in series, each sending its image to the next for further magnification until one could see small things at a very great distance[64]—an arrangement which makes rational and imaginative sense, but is unfortunately optical nonsense. In any case, Bourne had not been able to attempt such experiments himself, because unlike "Mr Dee, and allso Mr Thomas Digges," he lacked "hability of the purse, for to seeke thorowly what may bee done with these two sortes of Glasses."

60 Van Helden (1977, 29–30). This passage is from Digges's completion of his father's *Pantometria*, published (London, 1571): sig. A3v.

61 There is a quite extensive bibliography about the so-called "Elizabethan telescope," with scholars still divided as to its reality. The strongest argument in favor of its existence is found in Ronan (1993), Van Helden (1977) argued that the Elizabethan telescope had no basis in fact, but was nevertheless a crucial part of the legend of telescopic seeing that allowed the actual telescope to be recognized as a valuable instrument, once it was actually invented; in his preface to the second printing of his work (in 2008), he allowed that Elizabethan experimenters might have been aware of telescopic combinations of lenses and mirrors, even if they could not actually obtain the results they claimed with them. Eileen Reeves (in the passage noted in n. 53) surveys this literature comprehensively.

62 The most accessible edition is Van Helden (1977, 30–34). On Bourne's telescopic claims and its relationship to the Elizabethan telescope, see now (Dupré 2010).

63 They also slightly extend the range of hypermetropic sight, a fact that may have contributed to the idea of using such glasses for telescopy.

64 It is perhaps worthy of note that Digges gives precisely examples of things that one might examine with a magnifying glass (writing, and the markings on a coin), extended to a great distance just as Roger Bacon had done in his entirely imaginary account of marvelous glasses.

He closed with a plea for financial support for an invention that would be of obvious service to the state.

Bourne's mention of Dr. John Dee brings us to the second place that Scot and, for that matter, Harriot may have had a chance to experience optical marvels. Dee is best known as the great Elizabethan magus, who summoned angels and spirits at will to his house in Mortlake. He was also a talented mathematician, one of Thomas Digges's teachers, in fact, whose greatest work was a monumental edition in English of Euclid's *Elements*, published in 1570.[65] To this work, he prepended a remarkable "Mathematicall Praeface," intently studied as much by his contemporaries as by modern historians of science, in which he laid out a "Groundplatt" of the whole of the mathematical arts. Optics was of particular interest to him, not least because of the promise it held of understanding astrological influences, carried from the planets to the earth with their light. But it also could provide uncommon entertainment:

> if you, being (alone) nere a certaine glasse, and proffer, with dagger or sword, to foyne at the glasse, you shall suddenly be moued to giue back (in maner) by reason of an Image, appearing in the ayre, betwene you & the glasse, with like hand, sword or dagger, & with like quicknes, foyning at your very eye, likewise as you do at the Glasse. Straunge, this is, to heare of: but more meruailous to behold, then these my wordes can signifie.[66]

For once, we can be certain that Dee was not describing a mere fantasy of an instrument. The "certaine glass" was a large concave mirror of a fairly short focal length, which, at the time that Dee wrote this, belonged to Sir William Pickering, a gentleman whom Dee had taught while at the University of Leuven.[67] Shortly thereafter, Pickering gave it to Dee, who astonished Queen Elizabeth with its illusions, and later he took it with him in his wanderings through Europe in the mid-1580s. Dee's spiritual amanuensis, Edward Kelley, was by that time receiving almost daily visits from angels, who imparted to the Englishmen urgent, apocalyptic messages that they demanded should be relayed to the Emperor in Prague. One would think that that would guarantee an imperial audience, but the two men (and their legions of angels) were refused for months. The fame of Dee's mirror had preceded him, however. Eventually he was granted a meeting with Rudolph II, who wanted to hear nothing of the angels' increasingly unhinged warnings; he would meet Dee only if he could see the curved mirror with its marvelous floating images, of which he heard so much. Clearly a well-made concave mirror was more of a rarity in sixteenth-century Europe than an apocalyptic prophet. The demonstration must have gone well, for, some years later, Dee wrote in his diary: "I gave to Mr Edward Kelley my Glass, so highly and long esteemed of our Quene, and the Emperor Randolph (sic) the second, *de quo in praefatione Euclidis fit mentio* [which is mentioned in the preface to Euclid]." After his death, the mirror passed to Thomas

[65] On Dee's interests, magical and mathematical, see Clulee (1988).

[66] Henry Billingsley and John Dee, The *Elements of Geometrie* (London, 1570), sig. b4r. On this passage, and the optical trope of figures hanging in the air, see Dupré (2007). For Dee's interest in and contribution to the *theory* of mirrors, see Clulee (1977) and Clagett (1984).

[67] (Johnston 2006, 69).

Allen, the mathematical "intelligencer" of Elizabethan Oxford, and eventually, in Allen's will, to Thomas Aylesbury, a friend of Harriot's.[68]

Harriot and Bulkeley have left us not a word on the reason for their interest in a giant lens. But it is not at all unlikely that the persistent English traditions about the marvelous powers of great glasses contributed to its appeal. Even if they were somehow unaware of the Bacon's depiction in popular culture, Harriot would surely have heard of Bacon's Oxford mirrors while he was a student there, perhaps even from Henry Savile, or in Recorde's popular textbook. In any case, the Harriot and his associates were very interested in Bacon's perspectival and natural philosophical works, in which he could have read Bacon's extravagant claims in the original.[69] Dee's mirror was famous in England and Europe, all the more so since Dee himself had described it in his widely read *Mathematicall Praeface.* And the mirror itself eventually ended up in the possession of his friend Thomas Allen, in whose Oxford rooms he may even have seen its amazing effects for himself.[70] Finally, he could hardly have remained unaware of the telescopic glass supposedly used by Digges's father. Thomas Digges's various works were among the most sophisticated published in Elizabethan England, and he had acquired a great reputation as an expert in all things mathematical and natural philosophical.[71] Bourne, writing to a politician, was able to assume that Cecil would already have heard of Leonard Digges' amazing instrument. Harriot could hardly have been less well informed about the English technology of mirrors than the Lord High Treasurer.

All of these marvels may have contributed to Harriot and Bulkeley's interest in a large glass. But the Diggesian glass is of particular significance here, since it is the only one about which specific, quantitative claims are made and, as such, may explain Harriot's own quantification of Bulkeley's glass, in particular the *numerical conclusions* that Harriot drew from his calculations about Bulkeley's glass. The papers that used the sine law to analyze the glass's burning capacity showed that the lens would burn at a distance of about 500 yards; moreover, it would burn with the heat of a little more than 10 suns, according to Harriot's idiosyncratic *tot soles* calculation. Digges, on the other hand, claimed that one of his father's glasses—a component of his telescopic apparatus—could set fire to gunpowder and discharge ordinance a whole half-mile distant. The much shorter focal distance of Harriot's absurdly ideal glass, as well as the relatively modest heat it would generate,[72] was perhaps meant as

68 This is the character mentioned in n. 33 above; Allen, however, died after Harriot. On the history and reputation of Dee's great concave mirror, see Feingold (1984, 157–158).

69 On the interest of Harriot and his collaborators in Roger Bacon, see Clucas (2000, 109) and the references cited in n. 68 there.

70 In his will, Harriot directed his executors to return the twelve or fourteen manuscripts he had borrowed from Allen. The fact that he states he cannot remember which manuscripts precisely belonged to Allen, and that he had no list of them, suggests that a long-lasting friendship, during which he had borrowed manuscripts on several occasions. See Tanner (1967, 246) The fact that Aylesbury was Allen's executor also suggests that Allen would have known Harriot—as, indeed, he knew almost every working mathematician in England.

71 Ash (2004).

72 Elsewhere in the manuscripts, Harriot measures effective tot soles in the hundreds or thousands. One paper in the bundle on the concave spherical mirror, for instance, measures the burning power of one incident ray as 4647 *tot soles* (MS 6789, fol. 116r).

a rebuke to Digges's inflated claims. For all we know, Bulkeley may originally have asked Harriot about the plausibility of Digges's burning instruments and to that end suggested that Harriot considers the burning power of an imaginary glass that *must* have been better than whatever Leonard Digges had at hand.

5 The mathematical context

The tangle of myth and reality that characterized sixteenth-century English thought about optics was surely a part of what inspired Bulkeley to ask about a gigantic lens, and the specific claims made by Thomas Digges enabled Harriot to put the stories to the test. But, in addition to this factor, there is a *mathematical* and optical problem to which Bulkeley's glass offered a solution—a problem that Harriot was indeed thinking about in his early optical work. This problem provided a plausible "mathematical context" for Harriot, an intellectual stimulus to see through to the end his laborious calculations about Bulkeley's imaginary lens.

It has often been noted that Perspectivist optics paid no attention to lenses, even though many of its protagonists must have had a pair of spectacles pinched onto the bridge of their noses.[73] The multiply formed refractive surfaces seemed to defeat their qualitative grasp of refraction. Both John Pecham and Roger Bacon instead attempted to trace the path of light rays through a glass ball—the "burning sphere"—but, due to an error in the diagram tradition of Alhacen (ultimately the source of the Perspectivist tradition), they imagined that all parallel rays incident on the ball, however distant from the axis, would coincide at a single spot beyond the ball. They were unaware, in other words, of the spherical aberration of the refracting sphere: its focal point, like that of the concave mirror, is imprecisely defined. Alhacen himself had been aware of aberration; although he did not state it explicitly in his *Optics*, it was implied in two important theorems—theorems that Pecham and Bacon ignored, no doubt because of their technical complexity.[74]

In contrast to his two Perspectivist predecessors, Witelo set himself the task of subsuming and reworking the entirety of Alhacen, proposition by proposition, and incorporating into the structure the work of the ancient Greek opticians, as well as Pecham and Bacon.[75] By necessity, then, he became aware that Alhacen's two theorems rendered the Perspectivist analysis of the burning sphere false. He thus faced a quandary: for completeness' sake, he needed to produce a theorem that covered the same ground as that of the other Perspectivists, but, while he could see that their results were false, he had no conclusive theorem with which to replace them.

His attempt at the problem, in X.48, fell into two parts. First, he considered the parallel rays incident on the glass ball in concentric circles around the axis of the sphere. Each of these families of rays, he showed correctly, would be refracted to a single point beyond the ball. He said nothing about whether each family would be

[73] See Smith (2010a, lxxxvii–lxxxix) for a survey of some of the reasons put forward for their silence.

[74] This thesis on Alhacen's knowledge of aberration and the origin of the error in the analysis of the ball in the diagram tradition is argued by Smith (2010b).

[75] For the relations and influences among the thirteenth-century Perspectivists, see Lindberg (1971).

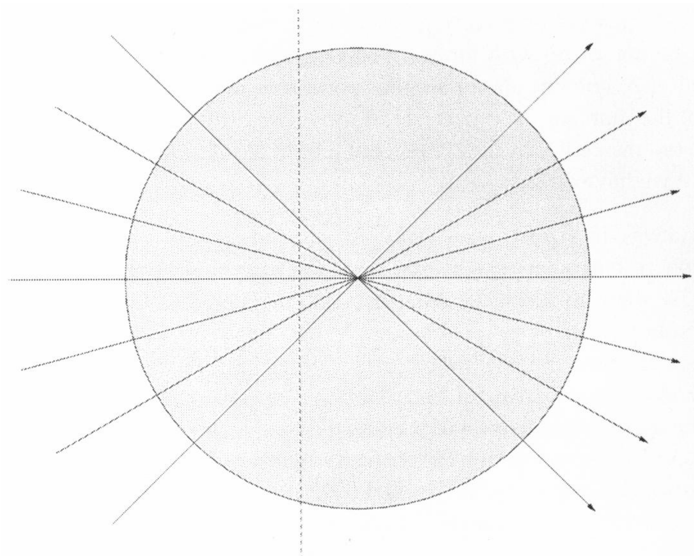

Fig. 10 "Focus" of spherical segment (after Witelo X.48)

directed to the same point (they would not, because of spherical aberration), but, at the same time, he did not assert that all rays *would* be directed to a single point, as the other Perspectivists had done. This statement about families of rays represented the most that could be salvaged from the earlier Perspectivist analysis of the burning sphere.

But was there, in fact, a single focal point of the refracting sphere? In the second part of the proposition, Witelo hit upon an ingenious idea: if the ball were to be bombarded not with parallel rays, but with rays each *perpendicular* to the surface of the ball, then they would all pass unrefracted through the surface of the ball and meet in the center. So, after all, he *was* able to provide a sort of answer to the standard Perspectivist question: where is the burning point of the refracting sphere?

But even Witelo recognized that his answer was unsatisfactory. The purpose of the problem was to find a burning point that could be put to use. The burning point he had discovered, however, was locked away fast inside the ball itself. Thus, Witelo concluded by suggesting that one might slice the ball just behind its center point and thereby reveal a usable burning point (see Fig. 10). This was a momentous suggestion, since Witelo had, for the very first time in the history of optics, suggested a method of analyzing not the glass ball, but the *lens*: the shape that remained after cutting through the ball was, in fact, a very thick, plano-convex lens.

In his *De refractione*, della Porta singled out Witelo's treatment of the focal point of the glass sphere as one of the principal flaws in the perspectivist treatment of refraction. He wrote:

> Witelo is greatly in error, where he thinks that the rays of the sun, incident at right angles to the surface of the ball, are refracted to its center; and, because it seems impossible to make a test there, he wants there to be a portion of the sphere less than a semicircle, so that he can see whether fire is kindled in

its center—which is completely false. In fact, in the whole of Witelo's work, whatever he comes up with himself (beyond what is from Alhacen) is almost always false. A semicircular or smaller portion of the sphere reaches almost to the end of the diameter; and because of the second refraction, [the rays will be found] not in the center of the sphere, but a little closer, the reason being drawn from what we have already said.[76]

Della Porta was, in a sense, quite right. The second refraction at the flat interface—where the sphere has been cut—would bend the rays away from the perpendicular and thus cause them to reach the axis *before* the center. But that was not the most significant problem with Witelo's theorem. In his construction, he required all of the rays to be incident perpendicular to the surface of the ball and that is not how light falls from the sun or from any artificial light. Della Porta was aware that non-parallel rays were not to be used in the analysis of burning glasses; indeed, it was a first principle of his own optics.[77] In the paragraph before his criticism of Witelo, he had rebuked John Pecham for stipulating that rays proceeded from the center of the sun; "fire is kindled by *perpendicular* rays from the sun."[78] In other words, his own insistence on the use of parallel solar rays should have alerted him to a problem in Witelo as much as it did in Pecham. One can only think that della Porta missed this difficulty in Witelo's theorem because of his own tendency to confuse "parallel" with "perpendicular," evident in the phrase quoted above, against Pecham. (In fact, on only the previous page, della Porta had drawn a diagram of light falling in "perpendicular" rays on a ball,[79] in which the rays were clearly drawn (and described) to be *parallel*.)

We can be sure that Harriot considered this proposition with some care, since it is one of the very few he annotated in the Oslo Witelo. In theorem X.48, he underlined (only) the sentence in which Witelo suggested that the sphere be cut a little short of a hemisphere. In the margin, he wrote, "This passage is criticized by della Porta in II.22, but poorly."[80]

One should note that Harriot did not charge della Porta with criticizing Witelo *incorrectly*, only poorly or maladroitly (*male*); it is perhaps worth stepping back for a moment and considering how Harriot might have approached all of this material on the

[76] Della Porta (1593, 64) (II.22): "Maxime errat Vitellio, qui putat Solis radios superficiem pilae invadentes perpendiculariter, refrangi ad centrum, et quia ibi experimentum videri nequit,optat sphaerae portionem semicirculo minorem, ut videret an in centro illius ignis excitetur, quod est falsissimum. In universo enim opere suo quidquid ex se supra illud Alhazeni est, falsum fere est[.] Semicircularis sphaerae portio, vel minor accedit loco prope finem diametri, et ex secunda refractione paulo propinquius, non autem in centro, ratio ex his quae prius diximus, deducitur."

[77] In the proem to book II, on p. 35, he laid down some postulates taken from ancient optics, among them "Solis radios aliquos ab eo emergentes sibi invicem parallelos esse" ("that any rays that emerge from the Sun are parallel to each other"). Harriot, it is worth noting, regularly used a geometrical model of a sunbeam using non-parallel rays as I shall explain in my longer treatment of his optics.

[78] Della Porta (1593, 63): "putat enim radios a Solis centro progredientes supra pilam igne[m] accendere, quod est falsum[.] Nam a Solis perpendicularibus ignis excitatur."

[79] Ibid., 63: "Radii perpendiculares supremam extimam pilae superficiem invadentes ...".

[80] Oslo University Library Lib. rar. 790f, p. 444. Harriot underlined "Forte tamen portio sphaerae crystallinae ...inflammabili; quoniam omnes radii". His annotation reads: "Hic locus reprehenditur a Baptista Porta lib. 2 p. 22 sed male."

refractive properties of "lenses." Della Porta had come up with a treatment of lens-like objects that did in fact explain some of the broad characteristics of lenses (aberration and the shape of the caustic, in particular),[81] but at the expense of treating the entire glass as a black box that *somehow* sends the rays to just the right place. That must have been unsatisfactory to someone like Harriot, an expert at ray analysis, who was also of a naturally analytic, rather than synthetic cast of mind. Witelo, on the other hand, indicated a way that one might get to the focal point of the lens by ray analysis, but at the expense of requiring the apparently absurd condition that all of the rays should be incident perpendicularly to the glass. Della Porta had drawn Harriot's attention to that proposition, but had identified the fundamental flaw in the wrong place: the absence of a second refraction, rather than impossible configuration of the incident rays. This was, perhaps, what Harriot meant in saying that della Porta criticized Witelo "male." The question that may have presented itself to Harriot, then, is this: how can we explore the qualititative features of the lens that della Porta described for the first time (the place of the burning point, aberration, and so forth), while somehow using Witelo's orthodox ray analysis, appropriately corrected?

If one begins with Witelo's ray analysis, the task is somehow to satisfy Witelo's requirement of perpendicularity. Bulkeley's glass may have offered Harriot a way to satisfy this requirement. For, in order for light to be incident at right angles, either one can somehow arrange light sources around the lens so that they all impinge onto it at right angles (which would be a most peculiar situation), *or one could use an extremely large lens.* As we have seen, Bulkeley's vast lens would have been indistinguishable from a sheet of plane glass at close quarters, so that all natural, parallel beams of light would have been almost perpendicular to it. The greatest deviation from perpendicularity was at the very edges of the lens, and even then the incident rays deviate by only $1°35'29''$ from the perpendicular. Thus, the huge lens gave Harriot an opportunity to test out (on paper) Witelo's claim, in the first ever theorem proposed about a lens, that its focal point would be at the center of curvature of the convex surface. In fact, as Harriot discovered, light was refracted to about *twice* that distance.

6 Conclusion

It has taken us quite far afield to make sense of a small bundle of Harriot's papers, a mere ten sheets out of his eight thousand. But the papers themselves posed an intriguing conundrum. Several pieces of evidence within the papers and elsewhere in the Harriot manuscripts suggested that they were an early product of his optical explorations and perhaps even the very first problem to which he turned his newly discovered sine law. Why would he have devoted such effort to what was an apparently absurd problem, of a fantastically large lens? And what drew Bulkeley's interest to the problem in the first place? The rich tradition of English speculation on glasses and lenses, and the marvels that they could perform, was surely at the back of both men's minds and was probably the reason the problem occurred to Bulkeley in the first place. In the course

81 Harriot seems to make no allusion to della Porta's actual treatment of lenses in book VIII of the *De refractione*, perhaps because it was so flawed, even by della Porta's own principles. He often made refracted rays bend in the wrong direction, for instance. See Lindberg (1984, 146).

of working through the problem, Harriot delivered an implicit rebuke to one of the most marvelous claims of all: Leonard Digges's mysterious glasses that could spy out coins and writing many miles distant, and set fires half a mile away. But the problem possessed another facet that Harriot, the careful student of Witelo and his critic della Porta, must have perceived: it allowed one to test out, even only on paper, Witelo's valiant but flawed attempt to analyze the focal properties of a lens. Harriot succeeded where Witelo had failed—and this first success was, I believe, the impetus for all his future optical researches.

Acknowledgments Earlier versions of parts of this article were presented at the *Temper of Evidence* conference at Caltech, and at the Durham *Thomas Harriot Seminar*, and also at the History and Philosophy of Science Seminar at Indiana University, Bloomington, and the Fellows' Seminar at the Newberry Library, Chicago; suggestions and criticisms at all of those venues have improved this article. I am grateful to the National Endowment for the Humanities and the American Council of Learned Societies for funding the research leave during which this article was completed, and to the Newberry Library as the host of my NEH residential Fellowship. The British Library and the Oslo University Library generously allowed the images in this article to be reproduced.

Appendix 1: Papers in the "Bulkeley's glass" bundle

The following folios of MS London, British Library, Additional 6789 contain Harriot's treatment of "Bulkeley's Glass." I have rearranged them in the order that they should probably be read. The phrases in italics are the actual headings on the sheets.

135 *Of Mr Bulkeley his glasse.* Description of Bulkeley'sglass and calculation of its various dimensions.

132 *Bulkeley. De refractionibus per segmentum sphaerae vitreae. Pro concursu radiorum centralium.* ("On refractions through a segment of a glass sphere. To find the meeting point of the central rays"). Uses the sine law of refraction to calculate the axis intercepts of rays incident at $1'$, $1°$ and $1°35'29''$ (the very edge of the glass). Convex surface of lens is directed toward the sun.

134 *Bulkeleys glasse.* On recto and verso, calculations for fol. 132.

140 [No heading] Summary of some of the calculations for fol. 132.

133 *Bulkeley. De refractionibus per segmentum sphaerae vitreae. Ex observationibus Vitellonis. Pro concursu radiorum centralium.* ("On refractions through a segment of a glass sphere. From the observations of Witelo. To find the meeting point of the central rays"). Uses linear interpolation from Witelo's refraction tables to calculate the axis intercepts of rays incident at same angles as on 132.

136 *Bulkeley. De refractionibus per segmentum sphaerae vitreae. Ex observationibus Vitellonis. Pro concursu radiorum centralium.* On recto and verso, calculations for fol. 133. Also on verso, a large long division to calculate the *tot soles*.

137 *Bulkeley. De refractionibus per segmentum sphaerae vitreae. Pro concursu radiorum centralium.* On the recto, calculations for fol. 132. On the verso, calculations related to *tot soles*.

138 *Bulkeley. De refractionibus per segmentum sphaerae vitreae, et plana superficie ad solem. Pro concursu radiorum centralium.* ("On refractions through a segment of a glass sphere, with its plane face turned toward the sun. To find the meeting point of the central rays") Apart from the heading, this page is blank. The verso contains a careful sketch of the refraction of rays through a flat, conical glass.

139 *Bulkeley. De refractionibus per segmentum sphaerae vitreae, et plana superficie ad solem. Pro concursu radiorum centralium.* Calculations, using sine law of refraction, for Bulkeley's glass with its flat side directed toward the sun (i.e., reversed from above).

141 *A vitro ad aerem.* ("From glass to air"). Scribbled refraction calculations, using sine law of refraction. Based on the values that appear, it seems that Harriot is noting that the action of the lens in the two possible orientations with respect to the sun is quite different.

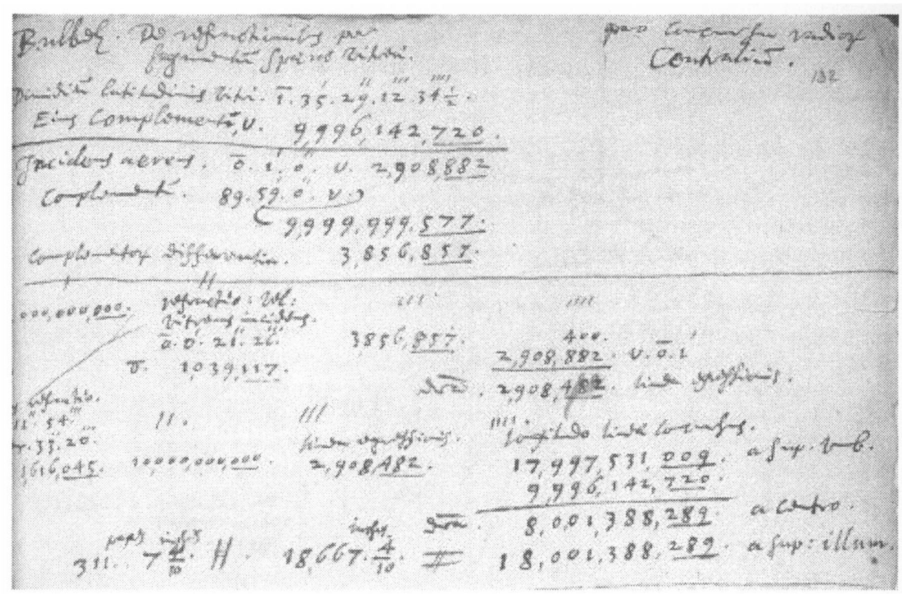

Fig. 11 Upper half of 132r: calculation of 1′ ray by sine law

Appendix 2: Calculation of axis intercept

General geometrical method

Harriot provided no guide nor diagram (beyond the rudimentary sketch at 135r, in Fig. 1) to explain how he calculated the place at which an incident ray cut the axis. All we have are calculations with brief labels; a typical example is from fol. 132r, where he calculates the path of a single ray incident at an angle of 1′ (reproduced in Fig. 11). Thus, it is necessary to reconstruct the geometrical reasoning that led to Harriot's calculations—and that is the purpose of this appendix. In Fig. 12, I have added some construction lines that will allow us to make sense of (and are implicit in) Harriot's calculations. And in Fig. 13, I have excerpted part of Fig. 12, in order to clarify the relationships that hold among the four angles θ_{1-4}, formed between the light and the perpendiculars to the glass in the course of its passage, *regardless of the method used to calculate refraction.*

In general, then, the light is incident on the lens at X with angle θ_1; and the angle at the center of curvature $\angle XAB$ is also equal to θ_1 (it will be useful here to consult Fig. 13). The light is refracted along XY, making an angle of refraction $\theta_2 = \angle YXA$. Now, all of the angles of $\triangle XYT$ are known: $\angle YXT = \theta_2, XTY = 90° - \theta_1$, and so $\angle TYX = 180° - \theta_2 - (90° - \theta_1) = 90° + (\theta_1 - \theta_2)$. But $\angle TYX$ is the angle that the light makes with the plane interface; so the actual angle of incidence XYW, measured from the perpendicular to this interface, will be this angle less the 90-degree angle XYT: or, $\theta_3 = \theta_1 - \theta_2$. Finally, the angle at which the light arrives at its axis intercept Z will be equal to the final angle of refraction: $\theta_4 = \angle YZB$.

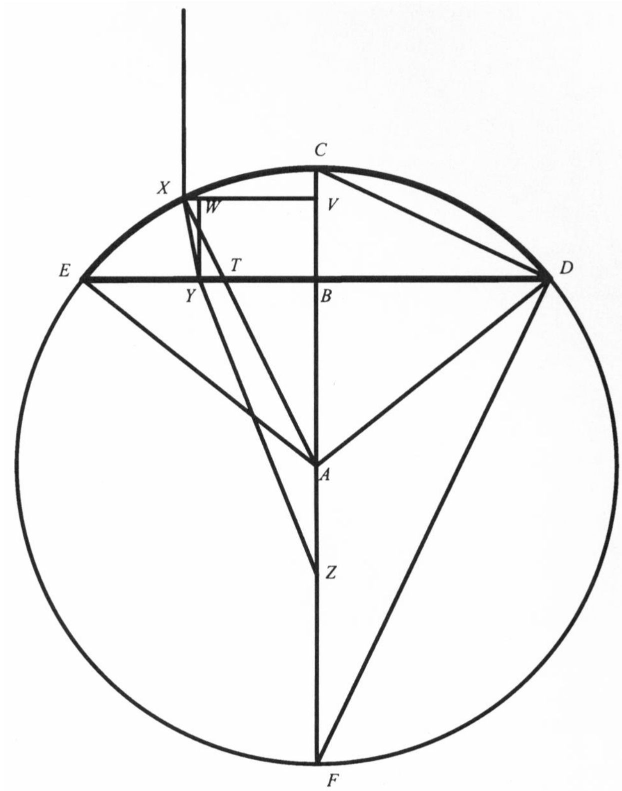

Fig. 12 Reconstructed diagram corresponding to Harriot's ray-tracing method

With these simplifying angular relationships in mind, we turn to reconstructing Harriot's method for calculating the effect of the lens. In what follows, I will first set out the geometrical and trigonometrical steps that (I claim) Harriot followed in order to solve the problem of the lens. Then, I will show that all of those steps are set out in the calculation in Fig. 11, which, moreover, uses the sine law to calculate refractions. I will then compare another calculation for the same ray, using precisely the same method but calculating refraction through interpolation from Witelo's tables.

Harriot has set himself the problem of discovering where on the axis point Z is, which he will express in several ways: as length ZB (*longitudo lineae concursus*), ZC (*distantia a superficie illuminata*) or ZA (*distantia a centro*). Since the distances BC (the thickness of the lens: 4 in.) and AC (the radius of curvature: calculated as 10,370 in.) are known, the problem amounts to discovering just distance ZB,[82] which could be determined if the right-angled triangle BZY were solved (referring now to Fig. 12). And, in order to solve that triangle, one would need to know two independent elements, such as the side YB and the angle at Y, $\angle BYZ$. And that is in fact what Harriot will

[82] Since $ZC = ZB + BC$, and $ZA = ZB - AB = ZB - (AC - BC)$.

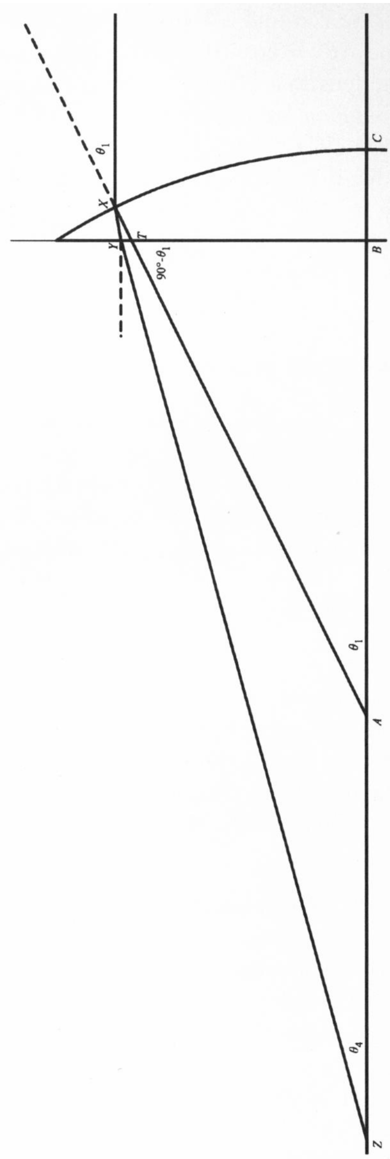

Fig. 13 General angular properties in plano-spherical lens

do, determining YB (which he calls the *linea egressionis*, or "line of emergence") and $\angle YZB$, which, as noted above, is equal to θ_4.

Discovering YB, the *linea egressionis* takes a little work. We note that $YB = WV = XV - XW$. Now, XA is a radius of the circle, which we will take as unity for all trigonometric calculations. Thus, observing $\triangle XAV$, we see that $XV = \sin\theta_1$ (using one of the angular relationships determined above); so that $YB = \sin\theta_1 - XW$. The problem of finding the *linea egressionis* now amounts to determining XW.

We can find XW by solving $\triangle WXY$. Now, $WY = VB = AV - AB$. $AV = \cos\theta_1$, and $AB = \cos\angle BAE$. This last angle $\angle BAE = \angle BAD$ is one of the parameters of the lens that Harriot calculated at the very beginning of his analysis (on p. 135r): *dimidium latitudinis vitri*, "half the width of the glass," which he determined to be $1°35'29''12'''34.5''''$. Thus, $WY = \cos\theta_1 - \cos\angle BAD$. Moreover, $\angle WYX$ is nothing other than the angle of incidence at the glass–air interface, or $\theta_3 = \theta_1 - \theta_2$. So,

$$\tan\angle WYX = \tan(\theta_1 - \theta_2) = \frac{XW}{WY}, \text{ or}$$
$$XW = (\cos\theta_1 - \cos\angle BAD) \,.\, \tan(\theta_1 - \theta_2)\,.$$

Putting this all together, we have thus found that:

$$\textit{linea egressionis} = \sin\theta_1 - (\cos\theta_1 - \cos\angle BAD) \,.\, \tan(\theta_1 - \theta_2)\,.$$

It is very easy now to obtain the required burning distance; since we have the *linea egressionis*, we now possess enough information to find ZB, or the *longitudo lineae concursus*, by solving $\triangle BZY$, the task we set ourselves above:

$$\tan\theta_4 = \frac{BY}{ZB} = \frac{\textit{linea egressionis}}{\textit{longitudo lineae concursus}}$$

To reiterate: this method of determining the burning distance of the lens may be followed regardless of the theory of refraction. Whether Harriot (or anyone else) uses the sine law of refraction or any other rule, the sequence of steps will be the same. But in order to obtain the value of two of the angles required for the method—θ_2 and θ_4—*some rule for determining refraction will have to be applied.*

Analysis of calculation on 132r

The calculation reproduced in Fig. 11 concerns the behavior of a ray with an angle of incidence $\theta_1 = 1'$ (*Incidens aereus* $\bar{0}1'0''$). Harriot begins by calculating the *linea egressionis* which, we recall, was determined as:

$$\textit{linea egressionis} = \sin\theta_1 - (\cos\theta_1 - \cos\angle BAD) \,.\, \tan(\theta_1 - \theta_2)$$

The first calculation on the sheet, above the line drawn across the full width of the page, determines $\cos\theta_1 - \cos\angle BAD$. $\angle BAD$ is "half the width of the glass" (*dimidium latitudinis vitri*); Harriot calculates its cosine (or, as he terms is, *Eius complementi* υ, where υ is Harriot's usual character for sine) as 9996142720.[83] Beneath this, he calculates the cosine of $1'$ and then takes their difference: 3856857.

[83] Harriot, like his contemporaries, expressed the values of trigonometric functions not as decimal fractions, but as whole numbers, taking the radius of a circle of reference as some arbitrarily large number (the *sinus totus*, here equal to 10^{10}). His trigonometric functions are very accurate: a modern electronic calculator returns a value for the cosine of this angle of 0.99961427194. Very likely he was using Georg Rheticus's ten-figure trigonometric tables, *Opus palatinum de triangulis* (1596).

In the next step, Harriot solves the following proportion for x:

$$\textit{sinus totus} : \tan(\theta_1 - \theta_2) :: (\cos\angle BAD - \cos\theta_1) : x$$

a rearrangement of the formula above, where $x = \sin\theta_1 - \textit{linea egressionis}$. The first and third elements of the proportion need no explanation and can be clearly seen in Harriot's proportion beneath the full-width line: 10000000000 and 3856857. The second element of the proportion demands further examination, however.

We note that he labels the angle $0°0'21''26'''$ as *refractio, vel vitreus incidens*. This is an accurate description of the angle $\theta_1 - \theta_2$: it is either the *refractio* which, we recall, for Harriot and his contemporaries meant the *deviation* in a refracted ray, or *angle of incidence—angle of refraction*; or it is the angle of "incidence onto the glass[–air interface]," that is, θ_3. Harriot finds the tangent of this angle, using his customary symbol $\bar{0}$ for tangent.

But how has Harriot discovered $\theta_1 - \theta_2 = 0°0'21''26'''$; or, more precisely, how has he found θ_2, which, given that $\theta_1 = 1'$, must be equal to $1' - 0°0'21''26''' = 38''34'''$? In his air-to-glass refraction table on fol. 88r, as noted above (in n. 40), Harriot determined the angle of refraction r such that, for any given angle of incidence i, $\sin r / \sin i = \sin 40°$, so that, if Harriot was using the same method, we should find that $\sin 38''34''' = \sin 40°.\sin 1'$. And, in fact, $\sin 38''34''' = 0.000186976$, while $\sin 40°.\sin 1' = 0.000186979$, which shows very convincingly that Harriot did indeed use the sine law of refraction on this page of his working.

Harriot solves the proportion above, obtaining $x = 400$. He then subtracts this value from $\sin 0°1'$, to find the *linea egressionis* of 2908482. Now, in order to find the *longitudo lineae concursus*, he must solve the proportion

$$\tan\theta_4 : \textit{sinus totus} :: \textit{linea egressionis} : \textit{longitudo lineae concursus}$$

which he sets out very clearly next on the page. To use his numbers,

$$\tan 33''20''' : 10000000000 :: 2908482 : y$$

with the result that the *longitudo lineae concursus* $y = 17997531009$, ZB on our diagram.

Once again, Harriot has had to obtain the angle of a refracted ray, θ_4. And, again, he has used the same sine law. For, recalling that the angle of incidence on the glass $\theta_3 = 21''26'''$, we find that $\sin 33''20''' = \sin 21''26''' / \sin 40° = 0.0001616$ (correct to 4 significant figures). These numbers are recorded also in the left margin of the paper: *Eius refractio* $11''54'''$, and *Agg[regatio]* $33''20'''$, with the sine 1616045 written below. The *refractio* is, again, the amount of *deviation*, or $\theta_4 - \theta_3$, while the *aggregatio* is the sum of this number and θ_3 (to which we note that Harriot draws a connecting line) or, in other words, simply θ_4.

In the remaining calculations, Harriot calculates the distance from the center of curvature (ZA) and from the "illuminated [i.e., convex] surface of the glass" (ZC). The latter is 18001338289 parts of a 10000000000 part radius. Since Harriot has already

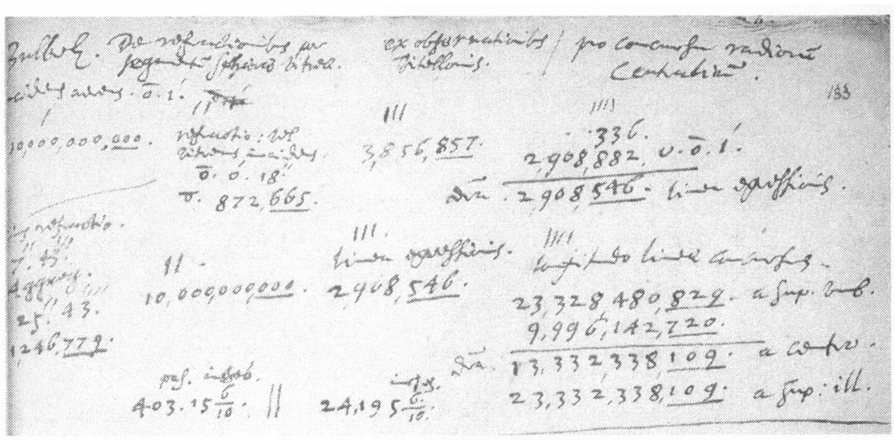

Fig. 14 Upper half of 133r: calculation of 1′ ray by interpolation from Witelo

calculated the radius of curvature as 10370 in, he can convert the burning distance into standard units: 18667.4 in, or 311 paces, 7.4 in., where a pace is equal to 5 feet.

That concludes Harriot's calculations for the 1′ ray; his calculations on this page for other angles of incidence proceed identically.

Analysis of calculation on fol. 133r

Even a cursory examination of fol. 133r (Fig. 14) should be enough to show that the geometrical method for finding the burning point is precisely the same as that used on 132r. The question, however, is the method that he used for calculating the angles of refraction, θ_2 and θ_4. He records the *refractio* (i.e., deviation after refraction) of the 1′ ray as 18″, so that $\theta_2 = 1' - 18'' = 42''$—a different value from that obtained by the sine law above.

According to the heading on this page, these calculations were made *ex observationibus Vitellonis*, "following the observations of Witelo." Harriot could only be referring to the tables of refractions Witelo took from Ptolemy and included (with some errors) in his optical treatise.[84] In that table, Witelo recorded that a 10° ray is refracted at an angle of 7°. Harriot was considering a ray incident at an angle 1/600 of 10°—and his value for its refraction was $42'' = 7°/600$. This one example suggests that he used very simple interpolation from Witelo's tables to obtain the refractions on this page.

To confirm this hypothesis, let us consider his value for $\theta_4 = 25''43''$ (labeled, as before, as *aggregatio*), corresponding to an incident angle to the glass of $\theta_3 = 18''$. Our hypothesis would be that he again used Witelo's table, but now in the opposite direction, because the ray is moving from glass to air: an incident ray of 7° refracts to 10°. And, indeed, $\theta_3/\theta_4 = 7°/10°$, or very nearly (a calculator puts the value of the ratio at 0.699935, very close indeed to 0.7). And in all the other rays Harriot analyzed

[84] See discussion at n. 22 above.

on this page (not depicted in Fig. 14), he determined the refractive angles so that they are in a 7/10 ratio with their angles of incidence.

Appendix 3: The refraction expressions implied by della Porta's models

It is relatively simple to extract an expression for the laws of refraction implied by della Porta's models, because of certain symmetries that obtain in these models *whatever* law of refraction is employed. In this appendix, I will take the two refractive apparatuses (the glass sphere and the plano-concave glass) separately. In each case, I first consider what relationships must hold among the various angles of incidence and refraction as one traces a ray through the glass, regardless of the law used to calculate the amount of refraction at each interface. Then, I introduce della Porta's prediction of where the ray must finally cross the axis of the instrument; combining this black box result with the observations made on the ray-traced model, it is possible in each case to extract a trigonometric expression for refraction in just a few steps. These two formulae are identical to those presented by Lohne as the relationships implied by della Porta's constructions "im modernen Sinne." Lohne gives no indication of how, or where, he found these formulae, and one can only presume he followed precisely the steps I lay out below.[85] Finally, I will show that, in a table of refractions in the Oslo Witelo, Harriot used the trigonometric formula derived from della Porta's plano-concave glass.

The glass sphere (De refractione, II.2)

Suppose a ray of light AB is incident on the glass ball at B, making an angle of $YBA = \theta_1$. We draw the axis OX through the center of the sphere, parallel to this incident ray. The ray will refract and will form some angle θ_2 with the perpendicular BO. The ray will then traverse the interior of the sphere to C, where the ray will form angle θ_3 with the perpendicular CO. Now, since the radii of the sphere $BO = CO$, $\triangle BOC$ is isosceles, so that $\theta_2 = \theta_3$. Finally, the ray is refracted out of the sphere, making an angle θ_4 with the perpendicular CZ, and meets the axis at point D.

Now, refraction is reversible,[86] so that a ray incident at θ_2 within the glass would refract into the air at angle θ_1. But $\theta_2 = \theta_3$, so that a ray incident on the glass–air interface at angle θ_3 will refract into the air at angle θ_1 or, in other words, $\theta_4 = \theta_1$.

To review, then, given *any* law of refraction, in the glass sphere, we will always find that $\theta_4 = \theta_1$ and $\theta_2 = \theta_3$. These simple angular relationships are what permit us

[85] Lohne (1963, 158–159) Lohne claims (on p. 159) that Harriot, Schickard and Huygens knew della Porta's *De refractione*, but Harriot and Schickard came up with a different expression (from the first formula) for the refraction relationship. He says that yet another formula could be derived, if, following della Porta's lead in some diagrams, one did not refract the ray at the spherical interface. In that case, one would derive the second formula I give below. But this is all very confused; as I show, the second formula was derived from della Porta's plano-concave glass, not from the spherical model, and, as I also show, this second formula was the one that Harriot himself derived. It seems that Lohne may have been relying imprecisely on a secondary source that I have not as yet identified.

[86] That is, if a ray at angle a refracts to angle b when passing from medium A to medium B, a ray going in the opposite direction, from medium B to medium A, incident at angle b will refract to angle a. This is an ancient principle of refraction; see Smith (1999, 133) for an example from Ptolemy's *Optics*.

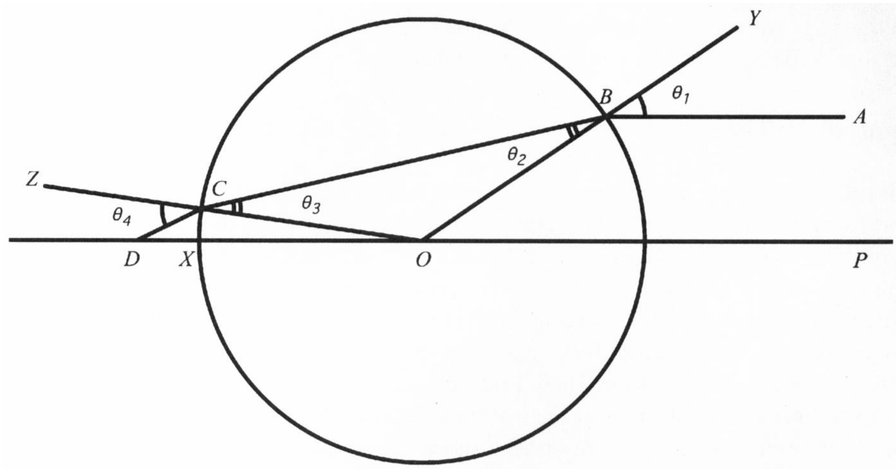

Fig. 15 Angle relationships in the general *refractive sphere*

to transform della Porta's prediction of an intercept point into a "law of refraction." For, if we wish to know *in general* what is the relationship between angle i of a ray incident on glass, and the corresponding refraction angle r, we have only to determine the relationship between $i = \theta_4 = \theta_1$ and $r = \theta_2 = \theta_3$. Since only two angles are in fact involved, henceforth I shall refer to them simply as i and r.

So, where does della Porta predict the ray will be refracted to? We recall (see Fig. 6) that he wishes us to imagine that the ray passes unrefracted to the concave surface of the sphere and is there reflected to a point on the axis within the sphere; the real *refracted* ray will meet the axis at a point symmetrical about point W (or point X in Fig. 15) with this notional point. Now, della Porta had shown that a ray incident on a spherical mirror at some angle θ was reflected to a point on the axis $1/2 \sec\theta$ distant from the *center* of the mirror, or $1 - 1/2\sec\theta$ from its vertex, so that, in della Porta's model of the refracting sphere, $XD = 1 - 1/2\sec i$ (and, to give another length we shall need in a moment, $DO = 1 + XD = 2 - 1/2\sec i$).

Now, consider ΔDCO. All of its angles are known. Since $\angle BOP = i$, and $\angle BOC = 180° - 2r$, $\angle COD = 180° - (\angle BOP + \angle BOC) = 2r - i$. $\angle OCD = 180° - \angle ZCD = 180° - i$, and finally $\angle CDO = 180° - (\angle COD + \angle OCD) = 2(i - r)$. We now solve $\angle DCO$ using the sine rule:

$$\frac{\sin OCD}{DO} = \frac{\sin CDO}{CO}, \text{ or } \frac{\sin(180° - i)}{2 - 1/2\sec i} = \frac{\sin 2(i - r)}{1}$$

so that

$$\begin{aligned}\sin 2(i - r) &= \frac{\sin i}{2 - 1/2\sec i}\\ &= \frac{\sin i}{\frac{1}{2\cos i}(4\cos i - 1)}\end{aligned}$$

$$= \frac{2\sin i \cos i}{4\cos i - 1}$$
$$= \frac{\sin 2i}{4\cos i - 1}$$

This equation thus expresses $i - r$: the amount of deviation of the light from its incident path, a value used very often by medieval Renaissance opticians[87] (including Harriot himself, as noted in the previous Appendix) instead of r—as a trigonometric ratio involving only i. Lohne gives precisely this expression on p. 158 of "Zur Geschichte der Brechungsgesetzes."

The plano-concave glass (De refractione, II.4)

As can be seen in Fig. 7, della Porta's plano-concave glass is made simpler by the fact that there is only one refraction involved: the ray from P passes perpendicularly and unrefracted through the plane side at P', and undergoes a single refraction at Q. Thus, we need to find a relationship between the angle of incidence $i = \angle Q'QP = \angle QOY$, and the angle of refraction $r = \angle LQO$. (We note that, in this case, the refraction takes place from glass to air, rather than from air to glass

By analogy to the previous case, we shall in fact find an expression not for r directly, but for the *deviation* of light, $r - i = \angle LQS = \angle SQR$:[88]

$$\begin{aligned}
\tan \angle SQR &= \frac{GR}{QG} \\
&= \frac{QF}{QS + SG} \\
&= \frac{QF}{FT + SG} \\
&= \frac{\sin i}{2\cos i + (QG - QS)} \\
&= \frac{\sin i}{2\cos i + (FR - QS)} \\
&= \frac{\sin i}{2\cos i + ((FO + OR) - QS)} \\
&= \frac{\sin i}{2\cos i + \cos i + (2 - 1/2\sec i) - 2\cos i} \\
&= \frac{\sin i}{2 + \cos i - 1/2\sec i}
\end{aligned}$$

[87] Witelo (in his tables in proposition X.8, for instance) uses the term *angulus refractus* for r, and *angulus refractionis* for $(i - r)$—a usage that Harriot also employs frequently in his manuscripts.

[88] $r - i$, rather than $i - r$, because of the direction of the refraction, according to which $r > i$.

The step-by-step derivation above should be quite self-explanatory; note, in the second to last line, the introduction of the expression for reflection in a spherical mirror. *OR* in this model is analogous to *DO* in the previous model.

It is possible to use trigonometric identities to simplify this expression a little further:

$$\begin{aligned}\tan(r-i) &= \frac{\sin i}{2+\cos i - 1/2\sec i}\\ &= \frac{2\sin i}{4+2\cos i - \sec i}\\ &= \frac{2\sin i\cos i}{4\cos i + 2\cos^2 i - 1}\\ &= \frac{\sin 2i}{4\cos i + \cos 2i}\end{aligned}$$

and it is this final version that Lohne provides on p. 159 of his article.

Harriot's use of a formula derived from della Porta

On p. 475 of his copy of Witelo, Harriot compiled two sets of tables of refractions, based both on observation and upon his two principal texts on refraction, della Porta's *De refractione* and Witelo's *Optica*. The first detail observations made on August 11–12, 1597, of refraction from air to water, measured using observation of a staff.[89] The set of tables written beneath this first table reports measurements of refraction between air and crystal, conducted on February 23, 1598 (reproduced in Fig. 16).[90] Despite the heading "From air to crystal," the tables actually record refraction *from* crystal *to* air: the table headed "Vitellio" is the column of *anguli refractionis* (i.e., $r-i$) from glass to air, from X.8 of his optical treatise. The observations on the left thus record observed refractions from crystal to air and the right-hand column, headed "Baptista Porta" must tabulate refractions according to della Porta's model. Of course, della Porta himself had no such table in his text, and Harriot affirms as much when he explains, next to the numbers, "Ex theoria Baptistae haec colligimus," that is, "I gathered these from the theory of Baptista."[91]

It can easily be confirmed that these numbers were calculated using the second expression above, based on the plano-concave glass, which (we recall) provided an

[89] The original observational record on which this table is based can be found at MS 6789, fol. 406r (for August 11), and 407v (for August 12). See Lohne (1973, 192–193), where the tables are transcribed.

[90] I have not found the original records of this experiment among Harriot's papers. Harriot describes the glass here: "Cristallum oblongum fuit, unum latus 3½ unc., alterum 2½ unc." ("The crystal was rectangular, with one side 3½ inches long, the other 2½ inches")

[91] Not *fere colligimus*, as Lohne transcribed these words in Lohne (1973, 192). In his transcription of the table, Lohne footnotes the numbers in this column as taken from della Porta's *De refractione*, pp. 42–43, that is, proposition II.2 on the glass sphere. On p. 194, Lohne explains this column quite vaguely: "Porta gab für Glas ein Faustregel an, aus der Harriot wohl die ...angeführten Winkel berechnet haben mag."

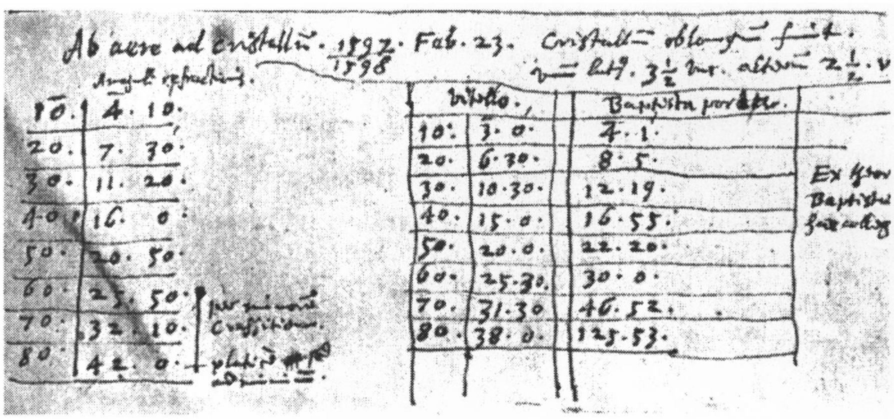

Fig. 16 Detail from Oslo University Library, 1572 edition of Alhacen and Witelo belonging to Harriot (shelfmark Lib.rar. 790f), p. 475

expression for refraction from glass to air, precisely the situation here. Below, I tabulate the results of calculating the *refractio* $r - i$ using this expression, against the numbers Harriot attributes to della Porta.

i	$\tan^{-1} \frac{\sin 2i}{4\cos i + \cos 2i}$	Harriot
10°	4°0′36″	4°1′
20°	8°5′7″	8°5′
30°	12°19′25″	12°19′
40°	16°55′3″	16°55′
50°	22°19′51″	22°20′
60°	30°	30°
70°	46°52′30″	46°52′
80°	125°37′34″	125°53′

The numbers calculated from the della Porta formula match Harriot's tabulated values perfectly (with the exception of the very last line, which must contain a calculational error). There can be no doubt that Harriot derived a trigonometric expression similar to the one that we extracted from the plano-concave glass and used it to calculate refraction values according to the *theoria* of della Porta. (Values calculated from the first formula, derived from the glass sphere, bear no resemblance to Harriot's results).

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