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GLASS WORKS: NEWTON'S PRISMS AND THE USES OF EXPERIMENT

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'Instruments are in truth reified theorems.'
(Gaston Bachelard, 1933)

'Perhaps, said the Marchioness, Nature has reserved the Merit of demonstrating Truth to the English prisms; that is, to those by whose means she at first discovered herself.'
(Francesco Algarotti, 1737)

Experimental controversy involves contest about authority. The acceptance of a matter of fact on the basis of an experimental report involves conceding authority to the reporter and to the instruments used in the experiment. In the seventeenth century, experimental philosophers used a wide range of means to make authority for their work. Conviction was thought to result from a long series of trials or from a single decisive experiment. It might result from being present as a witness at such a trial, achieving a replication of such a trial, or by reading a report given in so much circumstantial details that such direct witnessing was obviated. Authority might be held to lie in the credit of a single experimenter or in the communal assent of the experimental community. In their controversies, experimental philosophers often strenuously debated these differing ways of making conviction. Such fights show some of the uses of experiment in reaching agreement among disputants. Furthermore, they show how experimental instruments play a central role in these usages, and are resources which experimenters deploy in their struggles to achieve authority.[1]

1. For examples of experimenters' ambiguities about the significance of many trials or of uniquely decisive ones, and about the relative importance of direct or 'virtual' witnessing, see Boyle (1664), sigs. A2-A4; Shapin (1984).

The controversy discussed in this paper centred on Newton's work on light and colours between the 1660s and the 1720s. His trials with prisms, notably the celebrated *experimentum crucis*, were emblematic of experimental philosophy. They became so in at least two ways. After 1704, Newton claimed his trials had been replicated by competent experimenters, making facts which natural philosophers must acknowledge and use in their own work. Second, Newton also claimed that amongst these trials it was possible to pick out those which were 'crucial', and which would decisively settle dispute. The authority of these emblems was retrospectively located in the events surrounding the first public announcements Newton made in the early 1670s. But these emblematic uses were developed over several decades – they were not swiftly achieved, nor were they ever uncontested. Newton's programme was a site at which natural philosophers debated the boundary between experiments, 'concluding directly and without any suspicion of doubt', and hypotheses, 'conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phaenomena'. To some of his critics, Newton seemed to violate the rules of the experimental life. He was attacked as dogmatical, overestimating the authority due to his reports, providing too few trials to license his conclusions, and reporting experiments which could not be replicated. Thus, in contrast to Robert Boyle's celebrated emphases on 'histories' of many trials, Newton interrupted his first published account of optical trials to state that 'the historicall narration of these experiments would make a discourse too tedious & confused, & therefore I shall rather lay down the *Doctrine* first and then, for its examination, give you an instance or two of the *Experiments*, as a specimen of the rest'. Comments such as these highlighted differences over proper conduct in experimental work and reportage.[2]

Both the problem of the 'cruciality' of experiments and that of their 'replicability' are typical of the experimental sciences. It is misleading to treat the authority of such experiments as self-evident, for this obscures the detailed character of experimental controversy. The ground of such authority was often the matter in dispute. The resolution of such disputes masks the process by which agreement is accomplished. Agreement includes consensus about the conduct and meaning of a particular trial. Where experiments are interpreted as conveying unarguable lessons about the contents of Nature, this indicates that a controversy has already reached a stage of provisional closure. Only then will experiments be defined through an exemplary method, standardised tools and an agreed matter of fact. This paper examines the

2. Newton (1959–77), 1, pp. 96–7, 209. For comments see Bechler (1974); Dear (1985).

career of one of Newton's experiments, a trial with two prisms which he first recorded in a notebook in 1666 as the 'forty-fourth' of a long list of experiments on light and colour. In significantly changed format, this trial was made into an '*experimentum crucis*'. Newton's *experimentum crucis* was the object of considerable debate during the 1670s and has remained a central topic of philosophical and historical attention. The term was not used in his notebooks, drafts or lectures before 1672, nor did it appear in the *Opticks* in 1704. Nevertheless, the label remained current among Newton's readers and disciples. However, the *reference* of the term changed markedly. There was no consensus among the experimenters on the lesson which its author intended should be taught by this trial, nor on the proper method for conducting it. Some tried their version of this experiment, obtained results different from those which they held Newton had reported and rejected Newton's account of light and colour. These experimenters treated the trial as crucial but used that cruciality to undermine Newton's theory. Others, notably Robert Hooke, replicated Newton's trial, but then argued that the trial was not crucial, and denied that this replication licensed Newton's account. 'Cruciality' was an accomplishment which varied with outcomes of attempted replication.

The character of this accomplishment was intimately connected with issues of instrumentation, specifically, with the evaluations experimenters gave of the quality and arrangement of their *prisms*. There was no uncontroversial way of making these evaluations authoritative. For one community of experimenters during one period of time, Newton's *experimentum crucis* could be associated with an obvious procedure, involving complex arrangements of specially crafted prisms and lenses and a self-evident matter of fact, involving the chromatic homogeneity and the fixed refrangibility of primitive colour-making light rays. In the crucial experiment, a prism was used to make 'primitive' rays, and then one of these rays subjected to a second refraction in a second prism. Newton sometimes claimed that if white light were transmitted through a prism it could be separated into a set of 'primitive' colour-making rays. A properly separated 'primitive' ray could not then be further divided by transmission through another prism. It was a standing difficulty that many of Newton's critics reported that they could split putatively 'primitive' rays into further colours. But for Newton and his allies, a 'primitive' ray could be simply defined as a ray which could not be split by a second refraction. Then experimenters who managed to split such a ray could be criticised by Newton for their failure to produce 'primitive' rays. This argument established a troubling circle, akin to what H.M. Collins calls the 'experimenter's regress'. The criterion of a good experiment was that it produced the matter of fact which Newton sought to establish. Experimenters had to be convinced of this matter of fact before they could share this criterion. Once conviction had been achieved, then this criterion seemed unchallengeable. After closure, the procedures for making 'primitive' rays became self-evident. This paper documents the process by which this self-evidence was accomplished.[3]

The unarguable meaning which individuates an experiment is not achieved without struggle. How, then, do experiments acquire their identity? Scientific instruments play a decisive role in this process. Newton's arguments suggested that good prisms were those which made 'primitive' rays. His critics were told they were using bad prisms. Instruments help make experiments compelling, because the self-evidence which is attached to instrumental procedures after closure links complex experiments to agreed matters of fact. This closure makes instruments into what are seen as uncontestable transmitters of messages from nature, that is, it makes them 'transparent'. This process is comparable to what Trevor Pinch calls the 'black boxing' of instruments: he argues that after closure 'the social struggle over a piece of knowledge has become embedded in a piece of apparatus'. Such pieces can then be treated as if they 'regularly produce reliable and uncontentious information about the natural world'. Prisms have become so uncontentious that it is now hard to recapture the sense of their contingent and controversial use. Yet it is that contingent and controversial use which must be recovered in order to understand how 'transparency' is accomplished.[4]

It must also be stressed that 'transparency' is not necessarily achieved permanently. The 'transparency' of instruments may vary during controversy. Protagonists in disputes may engage in the 'deconstruction' of provisionally achieved 'transparency'. On occasion it may be useful to emphasise the specific complexities of an instrument in order to defend observation reports against criticism. Newton sometimes argued that the failings of prisms explained troubled experimental results. At other times, Newton and his critics minimised the role of their instruments in order to highlight what they claimed were basic conceptual disagreements. Furthermore, the accomplishment of 'transparency' sustains a realist history of experimental argument. Outcomes of earlier debates are then attributed to the 'obvious virtues' of instruments, rather than to the complex of practices and presuppositions which govern organised experimentation. When Newton's supporters claimed that his critics had chosen the wrong instruments, this allowed the further claim that Nature clearly spoke of the truth of Newton's theory of light and colour. But a more considered history of the prisms used in these arguments shows how this claim was accomplished as part of the provisional, local closure of the optical controversy.[5]

3. For 'experimenter's regress' and replication, see Collins (1985), pp. 79–100, 129–30. For discussion of the process by which 'open' settings reach closure in experimental dispute, see Latour (1987).
4. Pinch (1986), pp. 212–14.

THE PRISM BECOMES 'THE USEFULLEST INSTRUMENT' (1660–66)

Newton's reports of trials with lenses, mirrors and prisms, first delivered to the public after 1670, were connected with the active interest of his contemporaries in telescope and microscope design. This instrumental context will be considered first, before analysing the means by which the 'cruciality' of Newton's trials was connected with the troubles of such optical devices. This connection with glass working gave Newton's reports much of their immediate impact in London. When he started lecturing on light and colour at Cambridge in January 1670, Newton began by suggesting that attempts to grind conical glasses to avoid spherical aberration were as futile as efforts 'to plough the seashore'. He then indicated that even if such conics could be produced, there was a 'property inherent in the nature of light' which prevented the 'perfection of dioptrics'. This property was the specific refrangibility of primitive colour-making rays.[6] In 1672, Newton repeated these views in his letter to the Secretary of the Royal Society, Henry Oldenburg. In a carefully crafted reminiscence, Newton claimed that in early 1666 he had been working on the grinding of non-spherical glasses, when the understanding that 'Light it self is a *Heterogeneous mixture of differently refrangible rays*' prompted his abandonment of 'my aforesaid Glass-works'.[7] The tangible products of Newton's glass-works were versions of a reflecting telescope which would avoid some of the difficulties of aberration. The letter of February 1672 followed hard upon the demonstration of one version of this telescope at the Royal Society in January and Newton's election to the Society's fellowship.[8]

5. For the 'deconstruction' of experimental set-ups in controversy see Pickering (1981); Pinch (1981).
6. Newton (1967–81), 3, pp. 438–9; Newton (1984–), 1, p. 49.
7. Newton (1959–77), 1, p. 95. For Wren's contemporary work on nonspherical lenses see Bennett (1982), pp. 34–8.
8. Birch (1756–57), 3, p.4; Newton (1959–77), 1, pp. 3–4, 73–76; Mills and Turvey (1979); Newton (1984–), 1, pp. 427–428.

The range of optical instruments which Newton discussed posed different problems for his colleagues. The status of prisms in experimental optics was rather different from that of telescopes and microscopes. There was little technical work on the design and improvement of prisms, but optical instruments using lenses and mirrors were recognised as troublesome and in need of improvement. Newton could rely on his colleagues' interests in the latter, but he had to assume they had little interest in the former. Following the argument of Descartes in his *Dioptrique*, natural philosophers were aware of the problem of spherical aberration and they debated methods by which non-spherical lenses could be formed.[9] Yet it is unlikely that any parabolic lens was produced in the seventeenth century.[10] There was also much debate between Robert Hooke and others such as the French astronomer Adrien Auzout on the relative quality of various kinds of glass, since optical blanks were supposed to be as free as possible of veins and bubbles.[11] In the 1660s and 1670s, Venetian glass remained the standard against which other glass was to be compared. The role of glass-workers in Italy and elsewhere was also important. Newton's relations with the London glass-maker Christopher Cock were a significant part of his efforts to improve telescopes.[12] In general, however, the secretive practices of glass-makers were not easily subjected to enquiry. During the 1670s, glass workers were beginning to produce 'flint' glass by the addition of lead oxide, which had a high refractive index and was suitable for optical display, such as chandeliers, but which also tended to crack too easily. Most lenses of the period were full of air bubbles and flaws.[13]

The immediate response to Newton's views was partly governed by this work on the glass and metal technology of optical instruments. He debated technical issues with such as Hooke, Auzout and the Scottish mathematician James Gregory.[14] The controversy with Hooke explicitly raised the problem of whether further improvements could be expected

9. For Hooke and Newton on the making of corrected lenses, see Hooke (1665), sig. e2r; Hooke (1666); Newton (1959–77), 1, pp. 4, 53–4, 95; Bechler (1975), pp. 104–6.
10. Van Helden (1974).
11. Hooke (1665), sig. e1v; Oldenburg (1965–86), 2, pp. 383–9, 420, 468–9; for early eighteenth-century work on variations in glass quality see van der Star (1983), pp. 145–59.
12. For improvements of Venetian glass see Charleston (1957), pp. 218–21; Pedersen (1968), pp. 148–9; for Newton and instrument makers see Birch (1756–57), 3, pp. 4, 8, 19, 43; Newton (1959–77), 1, pp. 82–3, 85–7, 123–5, 185; Newton (1962), pp. 402–4.
13. Peddle (1921–22).
14. Newton (1959–77), 1, pp. 126–9. In March 1672, answering Auzout's worries about the performance of his mirrors, Newton proposed the inclusion of a crystal prism along the axis of the reflector.

from glass works. Hooke claimed priority for his own techniques for the improvement of telescopes by compound lenses. He told the Royal Society that 'I am a little troubled that this supposition should make Mr. Newton wholy lay aside the thoughts of improving microscopes and telescopes by Refractions'.[15] Newton composed a series of drafts in answer to Hooke which denied that he had given up the hope of improving optical instruments by lens designs.[16] He claimed he had a method for correcting aberration without conic sections. He now told Oldenburg that 'I examined what may be done not onely by *Glasses alone, but more especially by a complication of divers successive Mediums*, as by two or more *Glasses* or *Chrystalls* with *water* or some other fluid between them, all wch together may performe the office of one *Glasse*'. Such instruments were obvious and visible matters of dispute between Newton and his fellow experimenters and instrument makers.[17]

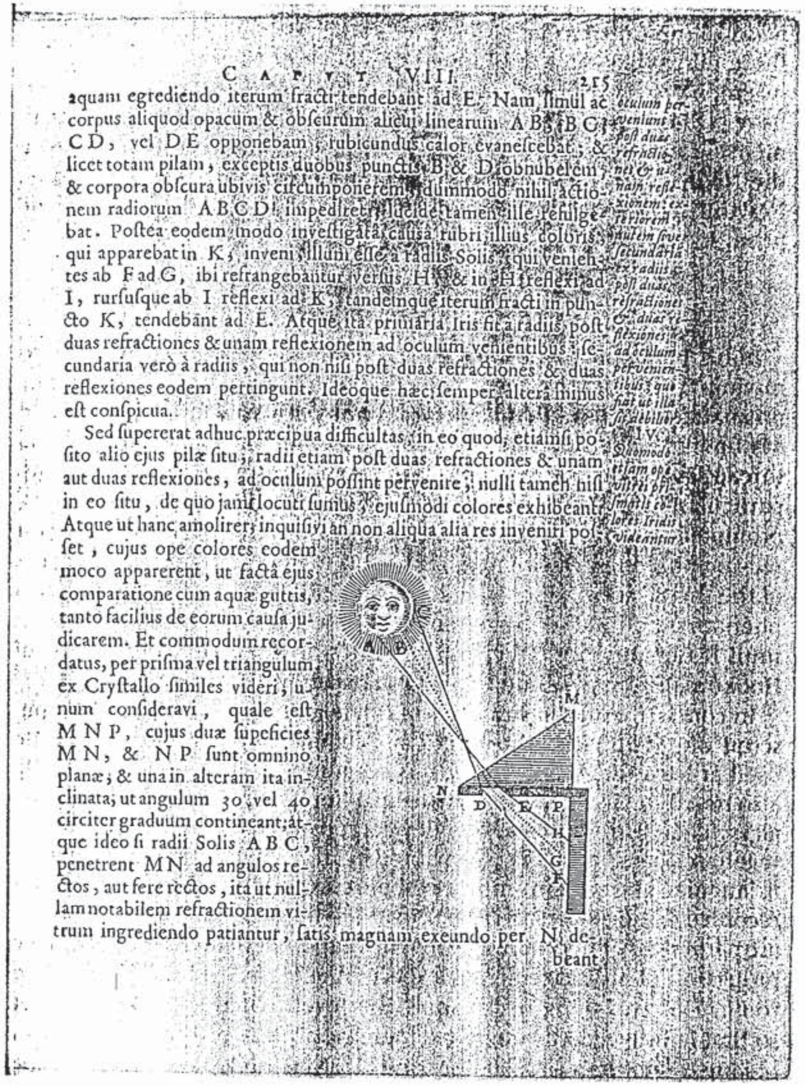
Unlike lenses and mirrors, prisms did not figure significantly in the construction of the instruments of astronomy and microscopy. Indeed, it is not clear why prisms were commercially available to Restoration natural philosophers. It has been suggested that they were used in chandeliers, or as toys. There is some evidence that well into the seventeenth-century prisms were seen as playfully deceitful and that the production of prismatic colours was indeed a common entertainment. The prismatic phenomena appeared in texts of natural magic, some of which Newton read in the 1650s. There were several anecdotes of 'a most pleasant and delightfull experiment' using 'a three square cristall prisme' to cast coloured images. For the Catholic natural philosopher Thomas White, writing in exile in Paris in the 1650s, it was still a commonplace that in 'Prismaticall glasses . . . we are pleas'd to know our selves delightfully cosen'd'. His colleague Kenelm Digby reported that triangular prisms were commonly known as 'Fools Paradises'. The transformation of prisms into instruments of experimental philosophy would have been a marked displacement of their use and significance.[18] On the other hand, there were several references to the use of crystalline prisms in medieval and Renaissance texts, particularly in association with the production of the rainbow. The 1660s saw a widespread deploy-

15. Birch (1756-57), 3, pp.4, 8, 10–15; Newton (1959–77), 1, p. 111.
16. Hall (1955); Newton (1967–81), 3, pp. 442–3, 512–13 n. 61; Bechler (1975), pp. 109–13; Shapiro (1979); Newton (1984–), 1, p. 429.
17. Newton (1959–77), 1, pp. 172, 191-2.
18. Dollond (1758); King (1955), pp. 144–50; Bechler (1975), pp. 125–6; Cantor (1983), pp. 64–9 (for optical instruments and lenses). Birch (1756–57), 3, p.41; Lohne (1961), pp. 393–4; Mills (1981), p. 14, for Newton's work on prisms. Peacham (1634), p. 140; Bate (1654), pp. 150–1; della Porta (1658), pp. 355–70; White (1654), p. 181; Digby (1669), p. 323; Huxley (1959), for comments on prism trials in other texts.

ment of prisms in the detailed investigation of the production of colours. As Alan Shapiro has suggested, they did so partly because of important challenges to scholastic theories of light and colour mounted by Cartesians. The schoolmen often distinguished two kinds of colour. *Emphatic* or *apparent* colours were those displayed through prisms or in the rainbow, where light was changed into colours through adjacent darkness, and *real* colours, disclosed in bodies by light but not produced by that light. In his *Dioptrique* and *Météores*, collected in an Amsterdam edition of 1656 and carefully studied by Newton during 1664, Descartes effaced this distinction. All colours were apparent, and thus all colours were produced the way prismatic colours were.[19]

This gave prisms a key new role in the analysis of colour. Prismatic colours could be seen as representations of the production of many other kinds of colour. In his analysis of the rainbow, Descartes reported a set of experiments using a prism to show how colours were produced in one refraction at the boundary between light and darkness. Such Cartesian texts provided a warrant for analysing colours with prisms, and also provided a new and influential target for criticism. It was reported that Newton bought his first prisms in the mid-1660s in order to attack Descartes's theories. Experimenters such as Robert Boyle and Robert Hooke began to write about prisms. But they did not yet make prisms into privileged instruments. In his *Micrographia*, Hooke coined the term *experimentum crucis* to describe an experiment he claimed decisively refuted Cartesian doctrine, but this was not a prism trial. Newton read *Micrographia* very carefully during 1665. Boyle's *Experiments and considerations touching colours* mainly reported trials with lenses, mirrors, and chemical tinctures and dyes. He used prisms sparingly. Newton's notes of the autumn of 1664 contain extensive comments drawn from Boyle, principally upon the varying appearance of colours in different situations and lights. Boyle reported just four experiments using prisms. Two of these involved the production of as many as four sets of emphatical colours, or 'irises', as Boyle called them, from rays of sunlight falling upon an equilateral crystal prism in a darkened room. In another pair of trials, Boyle sought to sustain the view that there was no difference between real and emphatic colours by casting the prismatic iris upon a 'really' coloured object. He sought to show that emphatic colours combined with real ones just as real colours did with each other. Prismatic blue shone on red cloth made it seem purple. Attempts to use prisms tinted with real colours were very troubled,

19. For the Cartesian and scholastic accounts of colour, see Westfall (1962), p. 343, 347; Lohne (1967); Lohne (1968), pp. 174–9; Sabra (1981), pp. 60–8; Newton (1983), pp. 246–9, 432, 434; Nakajima (1984); Newton (1984–), 1, p.4.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aquam egrediendo iterum fracti tendebant ad E. Nam, simul ac corpus aliquod opacum & obscurum alicui linearum A B, B C, C D, vel D E opponebam; rubicundus calor evanescebat; & licet totam pilam, exceptis duobus punctis B & D obnubelem, & corpora obscura ubivis circumponerem, dummodo nihil actionem radiorum A B C D impediret, lucide tamen ille refulgebat. Postea eodem modo investigata causa rubri illius coloris, qui apparebat in K, inveni illum esse a radiis Solis, qui venientes ab F ad G, ibi refrangebantur versus H, & in H reflexi ad I, rursusque ab I reflexi ad K, tandemque iterum fracti in puncto K, tendebant ad E. Atque ita primaria Iris fit a radiis post duas refractiones & unam reflexionem ad oculum venientibus; secundaria verò à radiis, qui non nisi post duas refractiones & duas reflexiones eodem pertingunt. Ideoque hæc semper altera minus est conspicua.

Sed supererat adhuc præcipua difficultas, in eo quod, etiamsi posito alio ejus pilæ situ, radii etiam post duas refractiones & unam aut duas reflexiones, ad oculum possint pervenire; nulli tamen nisi in eo situ, de quo jam locuti sumus, ejusmodi colores exhibeant. Atque ut hanc amolirer, inquisivi an non aliqua alia res inveniri posset, cujus ope colores eodem moco apparerent, ut facta ejus comparatione cum aquæ guttis, tanto facilius de eorum causa judicarem. Et commodum recordatus, per prisma vel triangulum ex Crystallo similes videri, unum consideravi, quale est M N P, cujus duæ supeficies M N, & N P sunt omnino planæ; & una in alteram ita inclinata; ut angulum 30 vel 40 circiter graduum contineant; atque ideo si radii Solis A B C, penetrent M N ad angulos rectos, aut fere rectos, ita ut nullam notabilem refractionem vitrum ingrediendo patiantur, satis magnam exeundo per N P debeant

Figure 2.1. Descartes's prism trial: 'a prism or triangle of crystal' inflects rays such that they 'paint all the colours of the rainbow' on a sheet of cloth or white paper. Descartes argues that there must be at least one refraction and a shadow to produce these colours. (Descartes, 'Meteors' in *Opera philosophica*, 1656, 3rd edn, p. 215. Amsterdam. This is the version Newton annotated in 1664–65. By permission of the Syndics of Cambridge University Library)

because the tints rendered the glass rather opaque. However, despite these difficulties, and the limited use Boyle made of his instrument, he emphasised the prism's status: it was 'the usefullest Instrument Men have yet imploy'd about the Contemplation of Colours' and 'the Instrument upon whose Effects we may the most Commodiously speculate the Nature of Emphatical Colours, (and perhaps that of Others too)'. Newton soon took up this suggestion, so changing the place of the prism in experimental optics.[20]

NEWTON TRANSFORMS THE USES OF THE 'GLASS-PRISM' (1666–72)

In his 1664 notes, Newton sought to emulate some of Boyle's colour-mixing trials with prisms but his experiments did not involve any prismatic projection, that is, they did not yet involve the casting of an iris upon a screen or wall. Instead, he examined coloured bands and threads by looking at these objects through a prism. These experiments, particularly that with a bicoloured thread examined through a prism, prompted the thought that blue-making rays were refracted more than red-making ones. Such an examination made the blue and red parts of the thread seem to separate from each other. Newton attributed this phenomenon to differing refrangibility. He did not record a projection until his manuscript 'Of colours', written in 1666 after his reading of Hooke's *Micrographia* and the beginning of his 'glass works'.[21]

In contrast with the strategies of Boyle and Hooke, Newton's manuscript of 1666 marked an important change in prism techniques. He used at least three different prisms separately and in combination. He also noted the use of a prism made of a 'four square vessell' of polished glass filled with water and a device constructed of two prisms tied together 'basis to basis'. Newton began changing the commercial 'triangular glass prism' into a complex experimental instrument. He recorded a long series of 'Experiments wth ye Prisme', two of which are particularly important. The seventh experiment involved a prismatic projection of an image across a space of at least 21 feet in a darkened room. This was designed to show that even when the prism was set so that light passed through it symmetrically, the prismatic image was oblong rather than circular, for from this Newton would argue that the shape of the image was due to the different refrangibility of different

20. For Newton's reading in Descartes, Boyle and Hooke, see Descartes (1656), p. 215; Boyle (1664), pp. 191–3, 224–9; Hooke (1665), p. 54; Lohne (1968), p. 179. Newton's notes are discussed in Hall (1955), pp. 27–8, 36–7; Westfall (1962), pp. 345–7; Mamiani (1976), pp. 81–94; Newton (1983), pp. 440–2, 452–62, 481.

21. Newton (1983), pp. 430, 440, 434, 467–8.

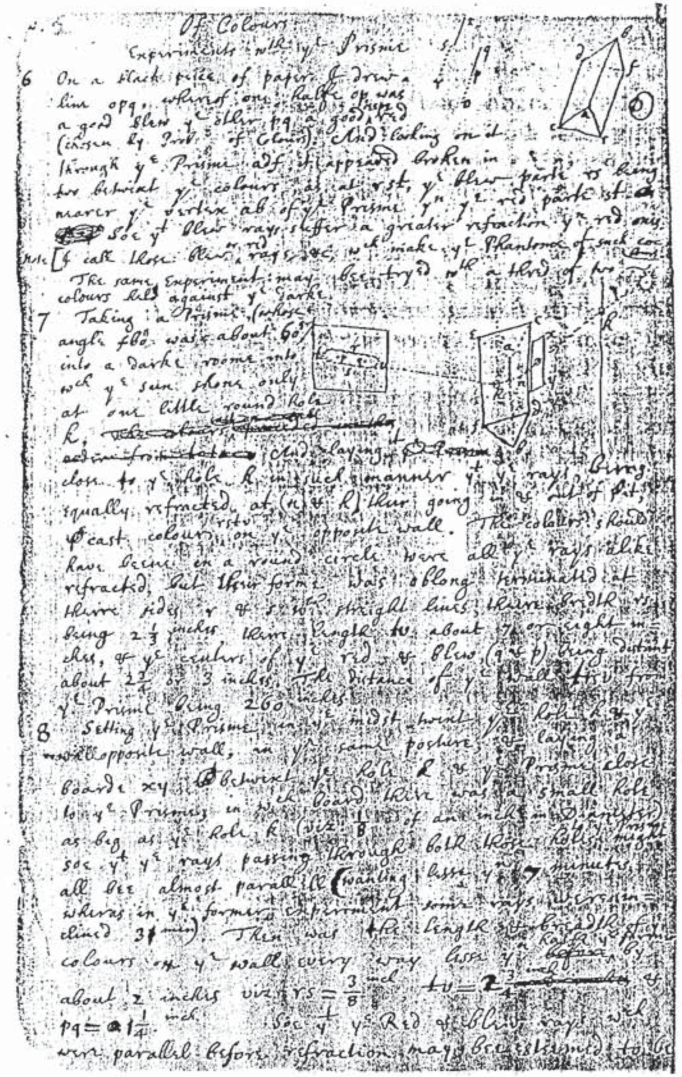

Figure 2.2. Newton's notes on the prismatic projection of light and the formation of a broadened spectrum, recorded in his notebook 'Of Colours' in 1666. 'The colours should have beene in a round circle . . . . but their forme was oblong'. (Cambridge University Library MSS Add 3975 f. 2. By permission of the Syndics of Cambridge University Library)

rays. This experiment appeared, carefully rewritten, as the first trial presented both in 1670 at Cambridge and in 1672 in his letter to Oldenburg, the 'celebrated phaenomena of colours'. The forty-fourth trial involved the use of a second prism to refract light rays again after their emergence from the first one. This experiment was designed to show that each ray had a specific refrangibility and made a specific colour. It was later to be substantially reworked as the celebrated *experimentum crucis*.

Both experiments fulfilled their role because of a set of claims about the way in which prisms worked. In order to understand these transformations in presentation and meaning and the persuasive role which Newton designed them to serve, it is necessary to consider the role and use of the prisms deployed in these trials.[22]

The claims about prisms which Newton made in these early notes remained tacit in the initial publication of his experiments. The dramatic innovation in the tactics of prism trials and the challenges to the utility of common prisms were not made visible. But controversy prompts protagonists to expose such tacit knowledge. During his trials of the 1660s and the controversies of the 1670s, Newton specified more details of how prisms should properly be prepared and used. He gave experimenters instructions about the *differences* between prisms which were commonly available and those which could best display the phenomena he reported. This implies that the provenance of Newton's own instruments is an important factor. However, none of the extant prisms associated with Newton seems to correspond to any of those whose use he describes. It is reported that Newton bought a prism at Stourbridge Fair in Cambridge in August 1665. Since 'he could not demonstrate' his hypothesis of colours against Descartes without a second one, he bought another there in 1666. But Newton left Cambridge before the 1665 Stourbridge fair and none was held in 1666. It has been suggested that Newton was recalling the fairs held at midsummer in those two years. A notebook also records the purchase of three prisms and collections of 'glasses' in London and Cambridge during 1668. As we have seen, the essay 'Of colours' suggests that Newton already had three prisms available to him during 1666. Further manuscripts of the period from 1668 record the purchase of optical machinery and work on lens grinding.[23]

22. Newton (1959–77), 1, pp. 92, 94; Newton (1983), pp. 478, 470, 472, 468.

23. For the provenance of Newton's prisms see Westfall (1980), pp. 156–8; Mills (1981), pp. 14–16, 27–32.

Two implications of these stories are of interest. First, prisms were evidently the sort of objects purchased at commercial fairs and in the City. They were correspondingly priced; prisms were relatively cheap tools for Newton's expanding programme of practical natural philosophy.[24] Second, while it would rapidly become clear to Newton that it was necessary to prepare prisms carefully for his optical trials, to concentrate attention on glass quality and prism design, nevertheless he gave no details of these protocols in his first communications with the London experimenters. As he put it bluntly in February 1672, 'I shall without further ceremony acquaint you that in the beginning of the year 1666 . . . I procured me a Triangular glass-Prisme'. Later in the letter he gave the dimensions of the prism and the refractive power of its glass (a value which indicates that this prism was unusual in containing some lead). Rather few indications were added in the body of the paper, which itself contained but four trials, save the instruction that the prism should be 'clear and colourless'. A second prism was invoked without any specification of its quality or geometry.

Newton's instructions proved insufficiently detailed for his audience. In experiments designed to show the important and controversial fact that 'uncompounded' rays could not be changed, Newton did not provide a recipe for making these 'primitive' or 'uncompounded' rays. Instead, he said that 'there should be perfecter separation of the Colours, than, after the manner above described, can be made by the Refraction of one single Prisme'. Evidently the perfect separation of an 'uncompounded' ray relied on special techniques in handling prisms. The separation of such rays was a novel feature of experimental optics. The existence of such rays was a novel feature of optical theory. Yet Newton relied on the familiarity of the common prism, and merely added that 'how to make such further separations will scarce be difficult to them, that consider the discovered laws of Refractions'. But these laws were precisely the matter of dispute. The subsequent career of Newton's *experimentum crucis* and the detailed interpretations of its author and critics show how vulnerable was the 'obviousness' of Newton's account and how important were the 'difficulties' of his instruments.[25]

24. For the prices of prisms see Newton (1936), pp. 52–3; Newton (1967–81), 1, pp. xii–xiii. Newton recorded that his three prisms of 1668 cost three shillings for the lot. This compares with purchases of glasses costing 14 shillings in Cambridge and another 16 shillings in London, eight shillings for a chemical furnace and £2 for chemicals during the same period.

25. Newton (1959–77), 1, pp. 92, 93, 100, 102.

NEWTON'S PRISMS AND HIS AUDIENCES (1670–72)

When he chose to give his first published account of his new doctrine of light and colours in early 1672, Newton helped himself to the rich resource of prism experiments which he had described in his Lucasian lectures at Cambridge. The contrast between these lectures and the version Newton released to his audience helps reveal how he sought to persuade that audience. In the lectures Newton described experiments using several prisms to show that light rays were differently refrangible and that differently refrangible rays displayed different colours. Each refracted colour-making ray was sent successively through a second prism onto a screen. In his sixth lecture, he drew the same conclusions from set-ups where the refracted rays were made to undergo total internal reflection in the second prism. Very few of these trials were then summarised for the Royal Society during the 1670s. In the lectures, no one trial appeared to be especially significant and most experiments seemed to need special equipment and technique. But in his communications with his fellow experimenters Newton made one trial 'crucial' and also suppressed most of the details of the procedures he had used.[26]

Nowhere in the earlier version of his lectures did Newton provide a clear demonstration of his doctrine of the immutability of the colour displayed by 'primitive' rays. There was no 'crucial' experiment. Instead, he rehearsed variations in the placing of screens, the illumination of the chamber in which the experiments were to be performed and the movement, position and quality of the prisms themselves. He proposed moving the first prism from its original place behind the first screen to a position between the sun and the screen. This was designed to remove the suspicion that the different angles of refraction of different rays might be due to different angles of incidence of sunlight at the first prism. He tried covering the leading side of the second prism with black paper pierced with a single hole, in order to admit only a few rays to the second refraction. In his sixth lecture he also changed the orientation of the two prisms so that sometimes they were crossed, and at others parallel.[27]

While enriching the possible tactics of prismatic trials, Newton also addressed the problem of prism quality and design. He was making prisms into experimental instruments. These instruments were supposed to demonstrate a novel and complex doctrine of the origin of

26. Newton (1984–), I, pp. 95–9, 133–9.
27. Newton (1984–), I, pp. 96–7, 134–5.

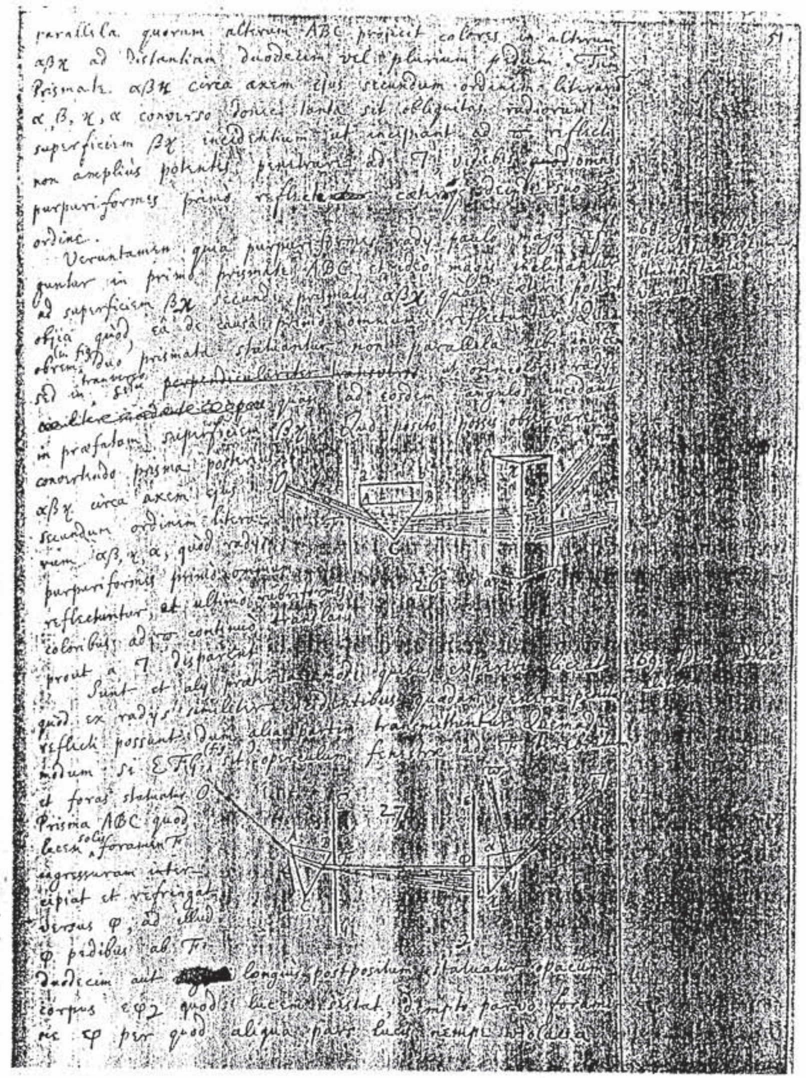

Figure 2.3. Newton's notes in the Lucasian lectures on variants of trials with two prisms, including experiments using total internal reflection (Fig. 27) to demonstrate the specific refrangibility of different 'primitive' rays. (Sixth optical lecture, Cambridge University Library MSS Add 4002 p. 51. By permission of the Syndics of Cambridge University Library)

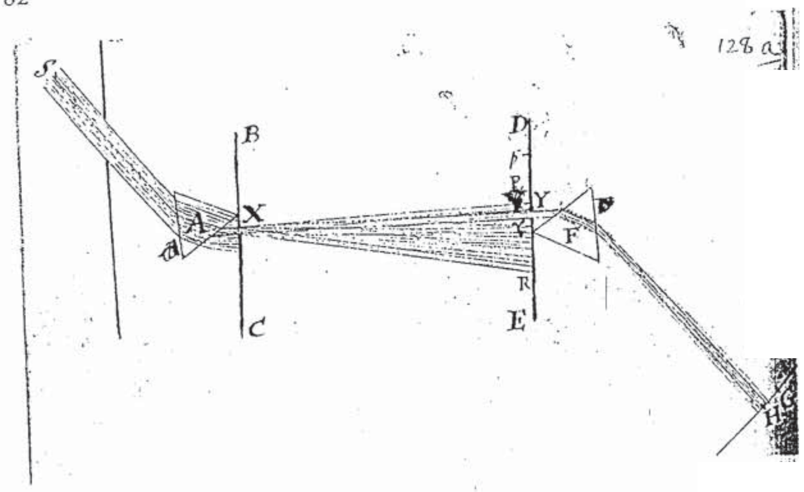

Figure 2.4. A loose sheet recording a preliminary version of the two prism experiment from the manuscript of the Lucasian optics lectures. (Cambridge University Library MSS Add 4002 f. 128a. By permission of the Syndics of Cambridge University Library)

colour. Colours were not generated by modifications of light inside prisms. Refractions analysed light into its constituent rays. This doctrine would not stand if it could be shown that irregularities in Newton's prisms were the cause of dispersion.[28] Furthermore, his prisms had to be capable of separating 'primitive' colour-making rays. He identified three troubles with common prisms: their small angles, their refractive powers and, most importantly, the fact that common prisms were often tinged with colour and vitiated with bubbles and veins. He lectured that

> instead of the glass prisms commonly sold (which are too slender) you must use broader ones, such as those you can make from glass plates highly polished on both sides and joined together in the form of a small prism-shaped vessel; the vessel should be filled with very clear water and sealed all around with cement . . . Those prisms, moreover, that are made wholly of glass are often tinged with some colour, such as green or yellow.

He repeated this advice in the sixth lecture when discussing total internal reflection and later recommended that the best glass was that 'used

28. From 1672, Newton reported attention to 'unevenness in the glass, or other contingent irregularity' in his prisms. He summed up these irregularities, including 'veins, an uneven polish, or fortuitous position of the pores of glass'. Newton (1958), p. 224; Newton (1959–77), 1, p. 93.

to make mirrors'.[29] Therefore, much of Newton's work centred on ways of telling whether prisms were working 'properly'. Only properly working prisms could show that his doctrine was right. To persuade his audience of this doctrine, he would have to persuade them to change the way they used prisms and to change the prisms they used.

This task proved troublesome during the 1670s. There was an ambiguity about the lessons Newton claimed his experiments taught. In the forty-fourth experiment of his notebook of 1666, Newton had derived two important consequences: first, blue-making rays were refracted more than red; second, these rays could not be split into further colours. But during the 1670s these two consequences were often separated from each other. The second lesson raised more trouble, because it was not easy to make 'uncompounded colours' with common prisms. Special instruments and protocols were needed. All colours 'proper to bodys' were mixed, as Newton noted in 1666. Furthermore, even in his lectures Newton had trouble demonstrating immutability. In his sixth lecture he briefly commented that he would 'show afterward' that 'no light of any simple colour can be changed in its colour . . . in refractions'.[30] But he did not honour this promise in the original lectures. He did so only in the revised version he completed in October 1674. Writing in the midst of his disputes with critics, he conceded that the two prism trial 'is not yet perfect in all respects'. In this trial, a red-making ray from the first prism did not display further colours when refracted through a second prism placed transverse to the first. However, if the second prism was placed parallel to the first, then the red-making ray also displayed yellow colours after the second refraction. This seemed to challenge the fundamental doctrine of immutability. To ward off this possible challenge, made all too obvious in the responses he was receiving from his correspondents in London and elsewhere, Newton changed the experimental protocol. He now stipulated that the holes through which the light was transmitted should be made as small as possible, while he also claimed that the best way to perform the experiment was to subject each ray to *many successive refractions*, not just two. He held that immutability would be proven if 'the apparent changes [in colour] would become smaller by repeated refractions, because simpler colours would arise at every step'. As Alan Shapiro has pointed out, this was just the strategy Newton used much later in his published *Opticks*. But he did not make these details clear to any of his colleagues during the seventeenth century.[31]

29. Newton (1984–), 1, pp. 105, 131, 153.
30. Newton (1984–), 1, p. 143.
31. Shapiro (1980), pp. 215–16; Newton (1984–), 1, pp. 453–5, 145 n. 33.

The differences in presentation fomented dispute between Newton and his audiences. In the letter to Oldenburg of February 1672, Newton selected some of his earlier trials, rewrote his autobiography, omitted many important details, notably those on prism quality and design, and revised some of the lessons these experiments were supposed to teach. The *experimentum crucis* was a simplified and revised form of large numbers of experiments given in the third and sixth Cambridge lectures. The first prism was placed between the Sun and the first screen and then turned slowly by hand. The lesson Newton derived here was the existence of differing refrangibility, not any consequence about the specificity or immutability of colour. But elsewhere in the letter Newton did discuss his commitment to immutability of colour. He claimed boldly that 'when any one sort of Rays hath been well parted from those of other kinds, it hath afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change it'. He mentioned efforts involving prisms, coloured reflectors and thin films, but gave no 'history' of these attempts.[32]

The technique for making a 'well parted ray' was not spelt out. The criterion for a 'well parted ray' seemed tautological to some of Newton's audience. 'Well parted rays' were only recognisable as just those which did not display further colours after a refraction, yet the doctrine in question was whether the colours displayed by such rays could be changed. In May 1673, for example, Newton told the Dutch natural philosopher Christiaan Huygens that he had given sufficient details for 'them who know how to examin whether a colour be simple or compounded', while in June he wrote again that the proposition of immutability 'might be further proved apart by experiments, too long to be here described'. This reticence was important, for several reasons. Ronald Laymon has suggested that Newton's *experimentum crucis* only works if 'idealized descriptions of the experiments are used'.[33] Newton's 'idealization' of his group of trials demanded that his prisms be seen as commonplace and the lay-out of his trials be treated in a highly abstracted form. This was part of his effort to win assent to his new doctrine. Furthermore, many of his readers assumed that the experiments reported by Newton to Oldenburg from 1672, particularly the *experimentum crucis*, were designed to demonstrate immutability *of colour*. Thus if the experimenters could show immutability was false, they held that Newton's doctrine would fall. Since Newton held that the

32. Newton (1959–77), 1, p. 97.
33. Newton (1959–77), 1, pp. 265, 294. For an analysis which notes the role of idealisation in Newton's reports see Laymon (1978), pp. 51–3.

demonstration of immutability, or of specific refrangibility, demanded prisms handled in special ways, the details of experimental tactics and of instruments were fundamental items in this dispute.

EXPERIMENTERS DEBATE 'MR. NEWTON'S DIRECTIONS' (1672-8)

The response to Newton's first paper showed that the techniques for handling prisms were an important part of the dispute about his claims. This response varied a great deal. Many found Newton's work dramatic and compelling. For them, Newton became 'our happy wonder of ingenuity and best broacher of *new light*'. But not all were persuaded. These differences evince contrasting experimental technologies and philosophies.[34] In London, Hooke replicated trials with two prisms but denied their decisive role. Newton was also countered by a group of English Jesuits at Liège, including the mathematics professor Francis Line, his student John Gascoines and the theology professor Anthony Lucas. The Jesuits initially proffered challenges to the presuppositions of Newton's trials and series of experiments of their own. When told by Newton that the *experimentum crucis* was the only trial to be examined, they reported a failure to replicate his alleged result. There was here no agreement on proper use and design of prisms. Nor was there agreement on the meaning and authority of the *experimentum crucis*.

Critics often contested Newton's implied claim that the *experimentum crucis* rendered the performance of long series of trials unnecessary. For example, Newton criticised Lucas for his effort to perform large numbers of optical trials. 'Instead of a multitude of things', Lucas should 'try only the *Experimentum Crucis*. For it is not number of Experiments, but weight to be regarded; and where one will do, what need many?' Yet this was not always the view which Newton and his interlocutors expressed. Newton himself denied in 1677 that 'I brought ye *Experimentum Crucis* to prove all'.[35] Hooke denied that this so-called 'crucial experiment' proved anything decisive: 'it is not that, which he soe calls, will doe the turne, for the same phaenomenon will be salved by my hypothesis as well as by his'. Indeed, Hooke implied that one of Newton's principal failings was the small number of trials the Cambridge professor reported, in contrast with the 'many hundreds of tryalls' which Hooke himself had performed and described. Hooke said he had

34. Fairfax (1674), p. 51.
35. Newton (1959–77), 2, pp. 79-80, 258.

not erected an hypothesis 'without first trying some hundreds of expts'.[36]

Hooke was writing a week after reading Newton's first paper on colours. Hooke, Boyle and the mathematician and divine Seth Ward were appointed by the Society to report on Newton's letter when it was read in London on February 8 1672. Hooke also recalled his 'crucial experiment' of 1665, one which did not use a prism. While he attributed great weight to the simple 'Experiment or Observation of Crystal', which, he held, decisively proved his own hypothesis of colour, he did not privilege the prism in the way Newton sought to achieve. His favoured experiments, used tellingly against Descartes in 1665, involved mica, thin plates and glasses filled with variously coloured liquids. He did not pay attention to the detailed flaws and corrections of commercial prisms which Newton charted in his Cambridge lectures. Newton sought to minimise what he saw as the defects of these objects: but bubbles, veins and tints provided Hooke with opportunities for further mechanical ingenuity. Hooke and Newton had different experimental technologies in their treatment of these devices and they drew very different conclusions from their trials.[37]

In April and May 1672, Hooke showed the Society a series of trials with two prisms, including one which demonstrated that 'rays of light being separated by one prism into distinct colours, the reflection [sic] made by another prism does not alter those colours'. By June, he had also replicated the *experimentum crucis*: yet he still insisted that 'I think it not an *Experimentum crucis*, as I may possibly shew hereafter'. He told the Royal Society's President that this trial might prove that 'colourd Radiations' maintain fixed refrangibilities: it did *not* prove what Hooke claimed Newton wanted to prove, that there was a 'colourd ray in the light before refraction'. Indeed, Newton did not seem consistent in his account of what this trial showed. In February, he said that it demonstrated that there were differently refrangible rays in light without reference to colour; in June, when publicly answering Hooke, he said that it demonstrated that 'rays of divers colours considered apart do at equall incidences suffer unequall refractions', so raising the issue of specific colour.[38]

36. Newton (1959-77), 1, pp. 110–11. Contrast Leibniz's views on Newton's experimen in Leibniz (1965), pp. 488–9.
37. For the exchanges between Hooke and Newton see Birch (1756–57), 3, pp. 10–15; Newton (1959–77), 1, pp. 110–14. For Hooke's views see Hooke (1665), pp. 47–54; Hooke (1705), p. 54.
38. Birch (1756–57), 3, pp. 47, 50; Newton, (1959–77), 1, pp. 195, 202–3.

Throughout their subsequent exchanges on prismatic colours, which continued to 1678, Hooke accepted what he took to be matters of fact in Newton's trials, and freely acknowledged that his replications had worked. But Hooke read Newton as arguing for specific refrangibility and for immutability in the 'crucial' experiment, and he denied that this trial was persuasive. Thus when Newton was at a meeting of the Society in March 1675, he apparently heard Hooke confirm that the trials reported three years earlier had been replicated. However, at the same meetings, Hooke developed his own vibration theory of light and colour, citing new experiments on diffraction to show that 'colours may be made without refraction' and that his own doctrine could successfully save all the phenomena of colour. By the end of 1675, Newton was prepared to claim that Hooke had 'accommodated his Hypothesis to this my suggestion' of the origin of colours. Hooke, in his turn, was reported as believing Newton had plagiarised his 'suggestion' from the *Micrographia*. Despite attempts at reconciliation between the protagonists, it appeared that Newton's 'crucial' experiment had not acquired authority, nor a fixed meaning. As late as 1690, Hooke told the Royal Society that he was aware of no 'Better' theory of colour than his own, thus writing Newton out of the history of optics.[39]

Newton and his Jesuit critics also discussed rival prism techniques and the meaning of the *experimentum crucis*. In spring 1672, Newton already found himself compelled to give fuller details of his trials than those presented in his initial paper. Newton sent Oldenburg a diagram of the experiment and conceded that 'I am apt to beleive [sic] that some of the experiments may seem obscure by reason of the brevity wherewith I writ them wch should have been described more largely & explained with schemes if they had been then intended for the publick'.[40] The reaction of Line and the colleagues who continued his work from autumn 1674 showed how hard it was for Newton to achieve authority over his 'publick'. It demonstrated the problems of achieving agreed replication.[41] During 1675, Newton appealed to the Royal Society in order to authorise his claim that the 'crucial' experiment had been replicated in London and was, allegedly, easy to perform anywhere: 'it

39. For Newton's relation with Hooke's criticisms, see Hall (1951), p. 221; Westfall (1963); Hall and Westfall (1967). The exchange and Hooke's historiography of optics are documented in Hooke (1705), pp. 186–90; Birch (1756–57), 3, pp. 194–5; Hooke (1935), pp. 148–9, 153; Newton (1959–77), 1, pp. 357, 360, 362–3, 408, 412.
40. Newton (1959–77), 1, pp. 166–7, 205–6, 212; Oldenburg (1965–86), 9, pp. 132–3.
41. For initial exchanges with Line, see Newton (1959–77), 1, pp. 318, 336, 329; Westfall (1966), pp. 303–4. Line claimed that he had read the optics of Newton's predecessor, Isaac Barrow and had performed optical trials in the 1640s with Kenelm Digby. For these earlier prism experiments see Digby (1669), p. 341.

may be tryed (though not so perfectly) even wthout darkning a room, or ye expence of any more time then a qter of an hower'.[42]

As the argument with the Liège experimenters became angrier, during 1676, Newton was told that they had often done their own optical experiments before witnesses at Line's house: 'we think it probable he hath tried his experiment thrice for Mr. Newton's once'. Gascoines suggested that only some unreported difference in the arrangement or type of prism could explain this conflicting result. In January and February 1676, Newton responded to this challenge with many more details of his own trials. He gave the dimension of the holes used to admit light. He re-emphasised the placing of the prism at minimum deviation and said the trial worked best when the sky was clear. He advised on the best way of darkening the room. Importantly, he said Gascoines should 'get a prism with an angle about 60 or 65 degrees . . . If his prism be pretty nearly equilateral, such as I suppose are usually sold in other places as well as in *England*, he may make use of the biggest angle'.[43]

These specifications were designed to elicit a replication of the experiment in Liège. 'Ye business being about matter of fact was not proper to be decided by writing but by trying it before competent witnesses'. Newton implied that if the Jesuits could not make these experiments work, then it must be due to their wilful incompetence rather than to subtle differences in technique. Newton wanted pictures, because 'a scheme or two . . . will make the business plainer'. The trial was demonstrated at the Society in April 1676 and Oldenburg told Gascoines and his colleagues of this allegedly decisive success.[44] However, the immediate response of the Liège natural philosophers showed that this result was not compelling. On the contrary: Lucas immediately answered Oldenburg's letter with the comment that 'I was much rejoyced to see the tryalls of that Illustrious Company, agree soe exactly with ours here, tho in somewhat ours disagree from Mr. Newton'. Lucas and his colleagues interpreted the Royal Society's experiment as reconcilable with their own. They continued to produce evidence which they held refuted Newton's doctrine. Newton was astonished: the Royal Society 'found them succeed as I affirmed'. Exchanges with Lucas continued until 1678 when Newton violently withdrew from all such dispute.[45]

42. Newton (1959–77), 1, pp. 335, 357–8, 410–11.
43. Newton (1959–77), 1, pp. 394, 409–10.
44. Birch (1756–57), 3, pp. 313–14; Newton (1959–77), 1, pp. 423–4 and 2, p. 6.
45. Newton (1959–77), 2, pp. 12, 76–81. For the dispute with Lucas see Westfall (1966); Gruner (1973); Guerlac (1981), pp. 89–98.

In this correspondence the status of the *experimentum crucis* was challenged at once. Lucas reported a set of new experiments designed to show that different colour-making rays did not differ in refrangibility. Newton told Lucas to try only the *experimentum crucis*. Lucas did so in October 1676, but without conceding its privileged role or the lesson which Newton claimed it taught. Lucas appealed to the precedent of Boyle's pneumatics, which had been supported by 'a vast number of new experiments'. He asked why he should accept Newton's stricture that the controversy must hinge on the outcome of one trial. Lucas also argued that the experiment, if successful, would not show intrinsically different refrangibilities in different colour-making rays. Lucas read Newton as seeking to prove a doctrine about colour with his 'crucial' trial, not merely a simpler result about unequal refraction; and he read Newton as illegitimately basing his authority on a single trial, rather than a mass of evidence. Newton, once again, was furious with what he saw as a failure to grasp the sense, or the authority, of his key experiment. He contemplated a publication of a major treatise on his optical work. The alternative was silence.[46]

Newton was drawn into a final exchange with Lucas. For when Lucas did try the *experimentum crucis* in autumn 1676, he reported a result different from that attributed to Newton. He reported that even though he had worked 'exactly according to Mr. Newtons directions' he found as 'a result of many trialls' that violet rays displayed a 'considerable quantity of red ones' after the second refraction. Newton's delayed reply re-emphasised the meaning he wished to give this trial: 'you think I brought it to prove that rays of different colours are differently refrangible'. Newton held that this thought was mistaken. Yet there were grounds for Lucas's reading. Recall Newton's public statement of June 1672 that the 'crucial' experiment proved that 'rays of divers colours considered apart do at equall incidences suffer unequall refractions'. But Newton told Lucas that 'I bring it to prove (without respect to colours) yt light consists in rays differently refrangible'.[47] Here Newton insisted that the crucial experiment taught nothing about colours. He did so because he had to discredit Lucas's version of this experiment. Lucas's report suggested that Newton was wrong about the constant colour displayed by truly uncompounded rays. So Newton replied that the experiment was not designed to prove the homogeneity of 'uncom-

46. Newton (1959–77), 2, pp. 8–11, 79, 182–3, 183–5; Oldenburg (1965–86), 13, pp. 99–100; Westfall (1966), p. 311; Gruner (1973): pp. 318–21.
47. Newton (1959–77), 2, p. 257; Oldenburg (1965–86), 13, p. 101; Lohne (1968), pp. 185–6. Newton's original formulation is in Newton (1959–77), 1, p. 187.

pounded' rays. This answer was directly linked with the problem of the quality and design of prisms. Newton's other tactic in his attack on Lucas was to challenge the Jesuit's instruments. Newton alleged that they were incapable of producing 'uncompounded' colours.

The controversy demonstrates a central trouble of replication and instrumentation. Further work was necessary to establish whether the two men were discussing the 'same' experiment. Newton said that since Lucas's experiment was different from the 'crucial' experiment, its different result did not discredit that experiment. Lucas insisted that he had 'but follow'd the way which [Newton] himselfe had track'd out for me'. At several points in this exchange, Newton drew attention to the need for proper prisms. In August 1676, he reported trials which compared changing prism angles with changes in the length of the spectrum produced. He then advised that the *experimentum crucis* must be made with 'Prisms which refract so much as to make the length of the Image five times its breadth, and rather more than less; for, otherwise Experiments will not succeed so plainly with others as they have done with me'. He also pointed out the need for prisms with plane or convex sides when making spectra: Newton suspected that Lucas was using a concave instrument. Lucas confirmed that this was so in October; later, he also considered 'the difference of glasse the prismes are made of'.[48]

These details affected the debate on changes of colour displayed by refracted rays, since Newton claimed that the ability to make a wide spectrum affected the ability to separate genuinely 'uncompounded' rays. He instructed Lucas on the character of 'compound' rays. Lucas was wrong 'to take your ordinary colours of ye Prism to be my [un]compounded ones'. So Newton emphasised that Lucas had not obtained properly 'uncompounded' rays because he had the wrong prisms and used them badly. Lucas's believed that none of these differences in *prisms* could explain why he had managed to change the colour of light rays by refraction. He told Newton that 'if all rayes differently coloured had an unequall refrangibility', as Newton apparently believed, then 'the variety of prismes could no more refract different colours *equally*, that it can change the nature of rayes'. To reach closure here, Newton would have had to persuade Lucas to change his prisms and then, as Newton himself did, to interpret these changes as the correction of

48. Newton (1959–77), 2, pp. 80–1, 189–91. As R.S. Westfall has shown, there is no evidence that the Jesuits were using prisms made of different glass and considerable evidence that they had trouble measuring their angles and detecting the concavity of the prisms' sides. See Westfall (1966), p. 309.

important defects. In March 1678 Lucas did report difficulty in getting prisms with good glass. But closure was not accomplished: instead, as we have seen, from summer 1678 Newton broke off any further debate on the issue.[49]

Newton's arguments with Hooke and Lucas show that the *status of the experimentum crucis* was hard to fix. There was no agreed criterion for a competent prism experiment or for a good prism. Only when the status of the experiment was fixed did this criterion become available. Replicability and meaning both hinged on the establishment of this emblem. Some of Newton's audience read the experiment as a claim about colour immutability. Newton sometimes provided them with grounds for this reading. By the 1720s, in fact, he seemed to have come to agree with this reading. That is, the experiment which Newton now counted as his decisive one had a new and fixed meaning: 'refracted light does not change its colour'. This slogan appeared on a technically defective but important emblem of Newton's programme, the vignette of the *experimentum crucis* which Newton designed for the 1722 French edition of his magisterial *Opticks*. 'Cherubs and spectators' were excised from this design so as to give pride of place to the prisms, which testified to the truth of this incontrovertible fact of immutability of colour.[50] Once this fact was established and firmly wedded to the instruments, a means then existed for discriminating good prisms and competent experimental arrangements. The process by which this criterion acquired self-evidence will be examined in the final sections of this chapter.

'AN UNHAPPY CHOICE OF PRISMS': THE ACHIEVEMENT OF 'TRANSPARENCY' (1704–22)

Those who eventually accepted the emblematic status of the *experimentum crucis* and Newton's prisms produced a story which explained why the experiment and the instruments had not swayed critics in the 1670s. In popular texts such as Voltaire's *Elements of Sir Isaac Newton's Philosophy* (1738) and Algarotti's *Newtonianism for Ladies* (1737) it was claimed that those who had not succeeded in replicating Newton's trials 'had not been happy enough in the Choice

49. Newton (1959–77), 2, pp. 252, 254–5, 269; Oldenburg (1965–1986), 13, p. 101; Gruner (1973), p. 327.

50. For the vignette to the French *Optícks* see Newton (1952), pp. 73, 122; Newton (1959–77), 7, pp. 155, 179, 201, 213; Lohne (1968), pp., 193–6; Guerlac (1981), pp. 156–63.

of . . . Prisms'. They were recording a rather common view.[51] Experimenters who had reported trials which differed from those of Newton were now dismissed from consideration because their instruments must have been defective. This claim depended on a prior consensus on the status of Newton's trials and his instruments. After assuming the Presidency of the Royal Society in 1703, producing the *Opticks* and working closely with the experimental philosopher J.T. Desaguliers, Newton was in a position to claim that any optical experiment, if performed with the right prisms, would guarantee the truth of his doctrine. In London, the prism had become a 'transparent' instrument. This was an accomplishment of Newton and his allies. It demanded a reconstruction of the record of the optical controversies. This reconstruction involved both the public exposition of new prism techniques and a reinterpretation of previous failures to replicate Newton's claims. As Newton took power over the key resources of experimental philosophy, Newtonian optics acquired a disciplinary history and a standardised technology.

The appearance of Newton's *Opticks* was a key event in this process. Consideration of its opening sections shows how Newton reconstructed his trials to make his authority inside the experimental community.[52] Initial passages described ways of separating 'uncompounded' rays. This had been a key trouble of the 1670s. Then, Newton had faced a dilemma: he could, apparently, only sway his colleagues with prisms they were used to employing. Yet he reckoned that with these, an 'uncompounded' ray could not easily be made. So it was hard to spell out a decisive experiment to prove that such rays could be made and did not display further new colours when passed through a second prism. The work of Lucas or of the French experimenter Edmé Mariotte made this problem only too clear. Like Lucas, Mariotte had performed trials in the 1670s which purported to challenge the *experimentum crucis* and demonstrate that the doctrine of colour immutability was false. He had considerable expertise in experimental optics. Within a year of Newton's first paper on light and colour, he had conducted trials in Paris on the mutability of colours which challenged Newton's claims about the hues of 'uncompounded' rays.[53].

51. Voltaire (1738), p. 101; Algarotti (1742), 2, p. 60.
52. For the impact of the book see Guerlac (1981), pp. 106–11; Hall (1975).
53. Leibniz mentioned these trials to Oldenburg in February 1673. They were repeated at the Paris Academy of Sciences in 1679 and published in Mariotte's *Traité des couleures* in 1681. Newton owned a 1717 edition of this text and marked the passage which challenged his doctrine. For Mariotte's work see Oldenburg (1965–86), 9, p. 485; Shapiro (1980a), pp. 283–4; Guerlac (1981), pp. 98–9. Newton's notes are in Harrison (1978), pp. 21, 25.

In his book, Mariotte developed an anti-Cartesian version of the modification hypothesis, supposing that the colour of a light ray could be changed in refraction. In his version of Newton's experiment with two prisms, he did not place the first prism at minimum deviation, nor did he place a screen immediately after this prism to collimate the rays produced. He used a white card to separate out a single ray after the first refraction and then examined what happened to this ray when it was refracted a second time. Mariotte was confident that this arrangement allowed him to make well separated rays. His card was at least 30 feet from the first prism, displaying a spectrum of a similar width to that Newton reported. He also ensured that 'the room is very dark and no sensible light passes through the slit in the card apart from that which is coloured'. Yet he reported that a purely violet ray displayed red and yellow tinges after the second refraction. Assuming that Newton's whole theory was supposed to stand or fall by this experiment, Mariotte concluded that 'the ingenious hypothesis of Mr. Newton must not at all be accepted'.[54] This single report of a single refutation of a view Newton had not quite expressed in print was an important resource in European responses to Newton's optical doctrine. Mariotte was often cited, notably by Leibniz, as providing an important challenge to Newton's theory. Leibniz, Mariotte's 'old friend', repeatedly reminded correspondents of the challenge to the matter of fact of colour immutability which the trials of the 1670s suggested.[55]

Newton's *Opticks*, painstakingly assembled during the 1690s, provided new resources with which to respond to these worries. Neither Lucas nor Mariotte was mentioned in the new book. Hooke, recently deceased, received only a cursory reference. Nor did Newton use the name *experimentum crucis* to sanctify the sixth experiment of the book, which used two prisms to prove constant refrangibility. Constancy of colour became at least as important a feature of his scheme. Thus Newton shifted the weight of his argument. The whole of the fourth proposition was devoted to a description of the way to make 'uncompounded' rays. At last, Newton gave a relatively full public account of the instruments 'sufficient for trying all the experiments in this book about simple light'.[56] Notable techniques included the positioning of a lens before the first prism to diminish the incident image. He also detailed the kinds of glass to be used: recalling remarks made in the Cambridge lectures, he specified 'Glass free from Bubbles and Veins'.

54. Mariotte (1740), pp. 226–31.
55. Leibniz (1965), pp. 488–9; Guerlac (1981), p. 116.
56. Newton (1704), pp. 30–2, 49.

The prisms must have 'truly plane' sides, not convex as he had su
to Lucas. The polish should be 'elaborate', not 'wrought witl
which produced 'little convex polite Risings like waves'. The e
prisms and lenses should be covered in black paper. Yet he co
that 'it's difficult to get Glass prisms fit for this Purpose', refe
his own practice of using vessels made of 'broken Looking-
filled with rain water and a lead salt to increase the ref
Elsewhere, he discussed ways these water-filled vessels should
and reported the failings of a prism 'made of a dark coloure
inclining to green.' When he replaced this with a prism of 'clea
Glass' he still found 'two or three little Bubbles' and covered th
tive parts of the Prism with black paper. After 1704, Newton an
his own copy of the book with further changes in specified prisn
and in details of liquids with which to fill prismatic vessels. Onc
these remarks changed the way prisms were to be handled ai
assessed. The stage was set for the claim that unsuccessful rep
were using bad prisms. The book provided a range of such re
and Newton set out to get the Royal Society's experimenters
them.[57]

Newton used his power over the Society against his critics,
what he perceived as a conspiracy headed by Leibniz and the
of the Leipzig journal *Acta eruditorum*. This strategy exploi
resources of the Presidency and the expertise of the experir
Hauksbee and Desaguliers, in a campaign in which the a
superior quality of Engish prisms soon became important. As
Shapin has argued, this was a campaign with very important
resonances. One of its most dramatic aspects was the assertio
authority of the experimenters of Augustan London. In late 17
niz heard about French interest in the *Opticks* and urged experi
there to replicate the troubled Mariotte report. At the same n
Newton ordered Hauksbee to begin trying the *Opticks* expe
before the Royal Society. These trials were then reported in
eruditorum. As the war with Newton began to consume his at
Leibniz decided to publish his views on Mariotte in the Leipzig
So from the summer of 1714, Newton directed Desaguliers, Ha
successor, to show that uncompounded colours could be ma
Mariotte's instruments were defective and that Leibniz was w

57. Newton (1704), pp. 49–51, 55, 63–4. Newton argued that rays which displa
and indigo were hard to make 'uncompounded', because of scattered light
Inequalities of the Prism'. This was an obvious resource to use against
whose trials had produced colour changes in the violet.

58. Hall (1980); Guerlac (1981), pp. 110–11, 116–17; Shapin (1981); Heilbron (
90–1.

Desaguliers had to address the problem of *replicability*. To destroy Mariotte's credit, he had to show experiments which resembled those of the 1670s. If he used too many of the new protocols outlined in the *Opticks* it would appear that Mariotte had been reading Newton's initial reports correctly. If he did not use these new protocols, then he would fail to produce 'uncompounded' colours. Desaguliers turned to Newton for aid: the President helped draft the paper which appeared under Desaguliers's name in the *Philosophical Transactions*. The two men emphasised that previously printed reports were sufficient to allow replication. 'Some Gentlemen abroad' had 'complained that they had not found the Experiments answer, for want of sufficient Directions in *Sir Isaac Newton's Opticks*; tho' I had no other Directions than what I found there'. But Desaguliers also accepted that much new information was necessary to allow these trials to be successfully repeated. He allowed that Newton's original papers of the 1670s were inadequately detailed. A technique for separating monochromatic rays 'was not published before Sir Is. Newton's *Opticks* came abroad' in 1704. This explained why Lucas and Mariotte had 'reported that the [crucial] Experiment did not succeed'. Furthermore, Desaguliers added many significant details even to the fuller descriptions of the *Opticks*, including the lenses and prisms to be used in the optical trials. Prisms should be made of the green glass used for the object glasses of telescopes. 'The best white prisms', it emerged, were inadequate for the purpose, being 'commonly full of veins'. Desaguliers' tactic, following Newton's advice, was to marry the techniques for making uncompounded colours described in the fourth proposition of the *Opticks*, including the careful treatment of clear glass prisms and the use of a collimating lens, with the lay-out of experiment six, that which most closely resembled the original *experimentum crucis* of 1672. Desaguliers now did what Newton had not done. He revived the name *experimentum crucis* for this completely reconstructed trial. Part of this reconstruction involved the claim that the 'crucial' experiment demonstrated colour homogeneity rather than specific refrangibility. Desaguliers tailored his experiments for effective witnessing. Spectators were each given a hand-held prism through which to view the spectrum cast on the final screen. Desaguliers made all these important changes and conceded that 'several have confessed to me that they at first used to fail in this experiment'. But it was essential that he stipulate that he had followed Newton's text to the letter, with no other resource at his disposal. Hence his insinuation that he had relied only on Newton's *publicly available* accounts.[59]

59. Desaguliers (1716), pp. 433–5, 443–4, 447, 448; Lohne (1968), pp. 189–90; Guerlac (1981), pp. 118–8; Heilbron (1983), p. 91. Newton's drafts for Desaguliers are in Newton (1714).

These experiments now had to be deployed in public. After a dry run at his house in Westminster, Desaguliers showed them to the Society. In early 1715, they were displayed to visiting natural philosophers from Holland, Italy and France. The repertoire of reformulated experiments soon became the prize exemplar of Newtonian optics. The visit of the French in 1715 was swiftly followed by successful replications in Paris and elsewhere.[60] Two aspects of this work are of importance for the career of the optical instruments: first, Newton and Desaguliers worked hard to make Mariotte's result depend on his bad glassware. Desaguliers announced in the *Philosophical Transactions* that he had proven this in 1714 and 'still shews it to those who desire to see it', presumably during his courses of experimental philosophy in the capital. News of these lectures, together with the view about the insufficiency of Mariotte's experiment, were then reproduced in Desaguliers's publications and by his colleague Pierre Coste in a French version of the *Opticks* in 1720.[61] Second, they also asserted that any experimenter must use the prisms which were available to the London natural philosophers. Personal visits to London were significant means by which natural philosophers could be won to this new practice. Once local agreement had been accomplished at the Society and among its audience in France, Italy and Holland on the doctrine of colour immutability, it was possible to define good prisms as those which displayed this result. The local and tenuous nature of this agreement was demonstrated by subsequent exchanges with European experimenters.[61]

CONTESTING 'TRANSPARENCY': THE VIEW FROM ITALY (1720–40)

In Italy Newton found several important followers and critics. His key trials had been replicated in Bologna, but the situation in Venice was less happy. As we have seen, Venetian glass had provided the standard of virtue through the seventeenth century and the English work directly challenged this status. In 1719, after reading the *Opticks*, the Venetian natural philosopher Giovanni Rizzetti began to perform

60. Desaguliers (1716), p. 435; Newton (1959–77), 6, pp. 144–5 and 7, pp. 113–14, 116–17; Guerlac (1981), pp. 128–43.

61. Desaguliers (1719), pp. 187–91; Schofield (1970), pp. 80–7. For the reaction of a French experimenter to replication of these results see Newton (1959–77), 7, pp. 114–16, 117 n. 6.

trials which denied Newton's reports and his doctrine.[62] Rizzetti's views were communicated to Newton and Desaguliers and simultaneously given publicity in the *Acta eruditorum*. Claiming that the authority of great men caused experimenters to err, Rizzetti anounced that

> I have taken care to repeat all the experiments, and (is it not right that I should speak of these things?) I have found some of them false and all the rest equivocal and by no means conclusive, because of the omission of some circumstances.[63]

Newton made his own copies of Rizzetti's letters and drafted a number of possible replies. In his draft Newton demanded that his Italian critic take the *experimentum crucis* as his premise: other trials 'might indeed inform us of something new concerning light, but they could not overturn what Sr Isaac has already established upon reasonings as free from all paralogisms as the demonstrations of Euclid'. Desaguliers obliged with a new show, first at his own house, then at the Society. Yet again, it emerged that several important techniques needed to be spelt out in detail to supplement the account published by Newton.[64] Rizzetti reacted with enthusiastic hostility. He sent a new report of his work to the Royal Society and began preparing a lengthy book on light and colour, eventually published in 1727. Rizzetti aimed to raise the standing of his trials by naming his witnesses, James Stirling and Nikolaus Bernoulli. The choice was unfortunate: Stirling was a Jacobite mathematician in exile, soon to be nicknamed 'the Venetian', while Bernoulli's uncle was the leading Leibnizian opponent of Newton. Newton soon saw Rizzetti's attack as one further attempt 'of the friends of Mr. Leibniz to embroil me'.[65]

This gave point to the London response to the Venetian experiments. Rizzetti's trials increased in number and significance, for he now turned his attention to the *experimentum crucis*. Rizzetti asked the English why Mariotte had managed to change the colour of violet rays after a second refraction. He added his own results: rays displaying pure yellow before the second refraction showed red, green and indigo after it, while the yellow colour vanished. He gave instructions for this trial: 'care is to

62. Newton (1704), pp. 13–14; Rizzetti (1722); Rizzetti (1741), p. 91; Hall (1982), pp. 18–19, 20–21; Heilbron (1983), p. 92. The Bolognese project was inaugurated by the natural philosopher Francesco Bianchini by 1707. Bianchini came to London in 1713, witnessed Hauksbee's experiments, met Newton and was given five copies of the Latin *Opticks* to distribute in Italy, including one for his colleague at Bologna, the astronomer Eustachio Manfredi.

63. Rizzetti (1722); Desaguliers (1728), pp. 596–7.

64. Desaguliers (1722); Newton (1959–77), 7, p. 255. Newton's drafts are in Newton (1722).

65. Rizzetti (1724); Newton (1959–77), 7, pp. xli, 53–5, Westfall (1980), pp. 799, 811.

be taken that the second prism is not too distant from the first, nor the slit, through which light of one colour is transmitted from refraction at the first prism to the second, is too narrow'. These were, of course, just the opposite of the conditions Desaguliers and Newton had stipulated for a good separation of an 'uncompounded' ray. But Rizzetti denied that his results were due to imperfect separation: he asked why red-making rays in his trials did not then split into adjacent colours and why he could make yellow-making rays disappear. Nor did he allow the important claim that Mariotte's results, or his own, were due to bad prisms. He had not used 'imperfect prisms, but exact care and suitable instruments'.[66]

These new criticisms reached London at the time of Newton's death, but Rizzetti was swiftly answered there by Desaguliers. In summer 1728 the Royal Society learned that replications of the key trials were planned by the loyal Newtonian group under Eustachio Manfredi at Bologna. The English reacted by electing the promoter of these plans into their fellowship. The decisive issue was the Newtonian experimenters' faith in the virtue of English instruments. Manfredi said that

> nearly all the experiments which can be read in the Optics of Newton, as well as in the little work of Desaguliers, have been done here in public displays. And when the prisms have been completely perfect, like some which we have to hand from England, the outcome has always corresponded to the doctrine.

The English claimed that Rizzetti, having made use of Prisms made at Venice, which are not of so pure a Cristall as ours, has been led into the many mistakes he has asserted for convincing proofs'. Rizzetti's bad instruments 'have rendered him ridiculous for ever'.[67] Desaguliers aimed to show the same thing in London. In August he showed a series of trials at his house, spelling out the heresies of Rizzetti and suggesting that even if the Italian had used common prisms and colours he should still have gotten better results. Desaguliers's witnesses included the President, other Fellows, and an invited group of noble Italians, who were specially named in reports published in London and sent to Italy. Using this technique to establish decisive authority, Desaguliers took the chance to add new evidence for the natural philosophy of attractive and repellent forces Newton had introduced in the *Opticks*. The campaign against Rizzetti had now made visible the scale of commitment to the novel ontology which loyalty to Newtonian principles demanded. Terms such as 'reflexion' took on polemical importance, for they were

66. Rizzetti (1724); Rizzetti (1727), pp. 37–8.
67. Dereham (1728); Desaguliers (1728), p. 597; Manfredi (1728); Rizzetti (1741), p. 112.

glossed by Desaguliers as references to an underlying dynamical theory of matter.[68]

Rizzetti bridled at these demands, as many others did. He rejected both the decisiveness of the London trials and the natural philosophy they were supposed to support. Rizzetti kept up his attacks well into the 1740s. As Geoffrey Cantor has suggested, the efforts to establish a consensus were fragile and indecisive. The 1740s saw important challenges to the basis of the dynamical theory and also specific criticisms of some of Newton's apparent claims. Cantor shows that the authority of the model for which Desaguliers and his allies argued was limited, even in Britain, to the spheres in which they most obviously exercised power: the Royal Society and the public lectures on experimental philosophy.[69] Popular presentations of this doctrine, such as that of Algarotti in the late 1730s, reported wide criticism of Newton's views. The work of Mariotte and of Rizzetti was commonly cited. Against this, propagandists used the double weapon of the crucial experiment, demonstrated by Desaguliers and supposedly proving the unchangeability of colour, together with the virtue of English prisms, allegedly evidently better than Venetian ones. Thus Algarotti severely chastised those who 'resolved to try Nature a thousand ways' when this one 'crucial' trial would serve. Newton had the Midas touch: 'everything Sir Isaac Newton handled became Demonstration'. The English optical experiments were demonstrative just because they were easily replicable: 'in looking upon a Paper of two colours with a Prism, it is of no sort of importance whether the Wind blows East or West'. Yet this replicability seemed to require English instruments. Algarotti reported that when he had tried the *experimentum crucis* he had failed to produce unchangeable colours, because 'our Prisms in Italy are of no other use than to amuse Children or hang up as a fine shew in some window in the country'. In contrast, the 'crucial' experiment worked well with prisms sent from England; 'these we esteemed as sacred'.[70]

The claims about the demonstrative authority of these experiments and the instruments needed to perform them aimed to make such instruments 'transparent'. Assent to Newtonian theories of colour was a precondition of seeing these instruments as untroubled objects. They were untroubled only to the extent that the changes in the design necessary to make the 'crucial' experiment work were viewed as non-partisan. Convinced disciples, such as Desaguliers, Algarotti or Manfredi

68. Dereham (1728); Desaguliers (1728).
69. Rizzetti (1741), pp. 112–21; Cantor (1983), pp. 42–9.
70. Algarotti (1742), 2, pp. 27, 51–2, 35–6, 40, 55–6, 62–5.

all reported that they had needed 'improved' prisms to make the trial succeed. These 'improvements' had to be seen as such by all protagonists in order to achieve persuasive power. That power lay in control over the social institutions of experimental philosophy. In the 1670s, Newton had exercised no such power. After 1710 his authority among London experimenters was overwhelming. This authority allowed carefully staged trials before chosen witnesses and the distribution of influential texts and instruments stamped with the imprimatur of collective assent. Enemies were condemned, as was Rizzetti, either as incompetent or evilly disposed. However, just as in the 1670s, this authority was necessarily unstable and contested. It could not force assent. 'Cruciality' was not a universal feature of Newton's experiment because not all subscribed to the disciplinary history which Newton and his allies helped write. Newton's 'law' did not compel experimenters such as Rizzetti: 'it would be a pretty situation', the Italian exclaimed, 'that in places where experiment is in favour of the law, the prisms for doing it work well, yet in places where it is not in favour, the prisms for doing it work badly'. For such critics, Newton's prisms never became 'transparent' devices of experimental philosophy.[71]

71. Rizzetti (1741), p. 112.

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