

Chapter 6 The 'Optical' Controversy between Hooke and Newton

In the final Chapter of this paper, the author aims to demonstrate the merits of considering Hooke as an able astronomer. The example used here is the 'optical' controversy between Hooke and Newton.

It is well known among historians of science that, when Newton made his debut at the Royal Society in 1672 with the invention of the Newtonian reflector, he was criticised vehemently by Hooke. Their controversy has since been analysed almost exclusively as an 'optical' controversy. Newton proposed a new theory of white light as a mixture of differently refrangible rays of light, which exhibit different colours. As is shown below, we have often been told that Hooke did not understand the importance of the revolutionary theory, and fought against the great Newton who later composed his theory into his major work *Opticks*.

However, examining the controversy in the context of the choice of telescopes for astronomical observation - reflector or refractor - reveals a new aspect. The controversy actually began over the future potential of refractors and reflectors. The theory of colour was a side issue. It will be shown that major natural philosophers well-versed in astronomical observation were sceptical of the Newtonian reflector, even though they agreed with Newton's new colour theory. This conservative attitude came from experimental philosophers who had to perform observations in practice. This historiography shows Hooke in a new light, which leads us to re-evaluate the role of experimental science in the latter half of the seventeenth century.

Part A of this Chapter first reviews the traditional historiography of the 1672 Hooke-Newton controversy, which regards the dispute as about optics. Even in that tradition, there were two contradictory interpretations of the controversy: Westfall's and Sabra's. The author proposes a third interpretation, a modified version of Sabra's, which unifies the two preceding views.

Part B reconsiders the Hooke-Newton controversy in the context of observational astronomy. From this perspective, Hooke was not an inferior protagonist of Newton, but a leading astronomer of his time whose viewpoint differed from that of Newton. Their different attitudes to telescopes could be the key to the real character of Hooke's natural philosophy. Hooke's attitude was much more rational than Newton's in the context of astronomy. Its implications will be discussed in detail in the Conclusion.

Part A The Hooke-Newton Controversy as an Optical Controversy

Newton first became known to the academic world, not by his contribution to mechanics, but through his invention of the 'Newtonian Telescope'. The Royal Society elected him a Fellow on 11 January 1672 in recognition of his invention¹.

Newton first mentioned his reflecting telescope in a private letter to a friend on 23 February 1669, as follows:

'The Instrument that I made is but Six Inches in Length, it beares something more then Inch aperture, and a Plano=convex eye glasse whose depth is 1/6 or 1/7th part of an Inch, soe that it Magnifies about 40 times in Diameter...I have seen with it Jupiter distinctly round and his Satellites, and Venus horned'².

John Collins (1625-1683), who was at the centre of correspondence on mathematical topics at that time, wrote to James Gregory (1638-1675) in 1670 that he had first met Newton in London at the end of 1669, and had heard from him about 'a pocket tube, but 6 Inches long'³. In a draft of a letter of 14 December 1671 to Francis Vernon (1637?-1677), Collins again recorded a report via Edward Bernard (1638-1696) from Thomas Gale (1636?-1702), a Fellow of Trinity and Regius Professor of Greek at Cambridge, that 'Mr. Newton (Barrow's successor) hath abbreviated a sixteen feet tube to the length of a span, which is a most happy invention'⁴. Collins may have communicated the news to the Royal Society, since he had certainly taken on this role in regard to Newton's eminent contribution to mathematics⁵.

Newton was a Fellow of Trinity, and was appointed Lucasian Professor of Mathematics in October 1669. He succeeded Isaac Barrow (1630-1677), and it was Barrow who reported Newton's achievement in mathematics to Collins⁶. It was also Barrow who took Newton's reflector to the Royal Society at the end of 1671 (Fig. 6-1)⁷.

This telescope was the second and improved version, and Newton submitted it to the Royal Society at their behest. The Society reported that it was equipped with a

¹ *Birch*, 3, p. 1.

² Newton to a Friend, 23 February 1669, *Newton Corresp.*, 1, pp. 3-9 (p. 3).

³ Collins to Gregory, 24 December 1670, *ibid.*, pp. 52-60 (pp. 53-54). For an estimate of the year of their meeting, see *ibid.*, p. 58, note 9.

⁴ Collins to Vernon, 14 December 1671 in Rigaud (1841), 1, pp. 176-179 (p. 176). A fragment of the draft is seen also in *Newton Corresp.*, 1, p. 5. On Bernard, see *ibid.*, p. 235, and on Gale, see Simpson (1981), p. 86.

⁵ Westfall (1980a), p. 234. Cf. Simpson (1981), pp. 86-88.

⁶ Barrow introduced Newton's 'De analysi' to Collins in July 1669. See letters in *Newton Corresp.*, 1, pp. 13ff.

⁷ Flamsteed to Collins, 31 January 1672, *Newton Corresp.*, 1, pp. 88-89 (p. 88).

concave metallic speculum of radius⁸ of about 13 inches, that is, its focal length was 6 1/3 inches⁹. Reflected light from the mirror was reflected again by another flat metallic speculum to a small eyepiece. The focal length of the eyepiece was 1/6 inch. Therefore objects were magnified about 38 times.

Collins himself examined Newton's telescope in London, and on 26 December 1671 wrote to Vernon that the image from the reflector was 'clere without Colours, and as much magnified as it could be by an ordinary Tellescope of 5 or 6 foot long'¹⁰. He also reported that 'it hath been brought up for his Ma[jes]ties perussal'.

At the beginning of 1672, Oldenburg informed Newton that his telescope 'having been considered, and examined here by some of ye most eminent in Opticall Science and practise, and applauded by them, they think it necessary to use some meanes to secure this Invention from ye Usurpation of forreingers'¹¹. The Society decided to send a description and illustration of Newton's reflector to Huygens in Paris, in order to register its originality¹².

Newton's Theory of Light and Colours

Pleased with the good response to the reflector at the Royal Society, Newton promised Oldenburg that he would write to the Society about 'a Philosophicall discovery wch induced mee to the making of the said Telescope'¹³. He soon fulfilled the promise in a letter containing his 'New Theory about Light and Colors [*sic*]'. It was read at the meeting of the Society on 8 February 1672, and published in *Philosophical Transactions*¹⁴.

It begins as follows:

'To perform my late promise to you, I shall without further ceremony acquaint you, that in the beginning of the Year 1666 (at which time I applied my self to the grinding of Optick glasses of other figures than *Spherical*.) I procured me a

⁸ Radius here clearly meant the radius of curvature. See, *ibid.*, p. 76, note 2.

⁹ For details of the telescope, see 'An Account of a New Catadioptrical Telescope Invented by Mr. Newton', *Phil. Trans.*, 7 (1672), no. 81, pp. 4004-4010 (pp. 4004-4005). See also *Newton Corresp.*, 1, pp. 74-76. The latter was a description of the telescope which was sent to Huygens to prove its originality.

¹⁰ Draft letter of Collins to Vernon, 21 December 1671, Royal Society MS/81, item 13; Simpson (1981), p. 89.

¹¹ Oldenburg to Newton, 2 January 1672, *Newton Corresp.*, 1, pp.73-74 (p. 73).

¹² See note 9 above.

¹³ Newton to Oldenburg, 18 January 1672, *Newton Corresp.*, 1, pp. 82-83 (p. 82).

¹⁴ Newton to Oldenburg, 6 February 1672, *ibid.*, pp. 92-107; *Birch*, 3, p. 9; *Phil. Trans.*, 6 (1672), no. 80, pp. 3075-3087.

Triangular glass-Prisme, to try therewith the celebrated *Phænomena of Colours*...It was at first a very pleasing divertisement, to view the vivid and intense colours produced thereby; but after a while applying my self to consider them more circumspectly, I became surprised to see them in an *oblong* form; which, according to the received laws of Refraction, I expected should have been circular’.

From the description, it is clear that Newton was grinding aspheric lenses which were free from spherical aberration. Kepler (1571-1630) and Descartes (1596-1650) had shown geometrically that spherical lenses could not have a pinpoint focus; this could only be achieved by lenses with elliptical or hyperbolic surfaces. The diffuseness of telescopic images had been ascribed to the imperfection of lenses, and it was expected that telescopes would be improved by the use of aspheric lenses.

While Newton was experimenting with a prism simply for his own amusement, he suddenly hit upon the idea that glass lenses had a much more significant defect, *i.e.*, chromatic aberration. He cast sunlight from an aperture in a window-shutter through a prism to the opposite wall. If, in accordance with conventional theory, all the rays of light had been refracted equally, the image of the sun should have been circular. In fact the image was oblong. Newton considered various hypotheses to explain this anomaly: the effects of the terminating shadow, the size of the aperture, the thickness and unevenness of the glass, the sun’s diameter, curvature of the path of the ray after refraction, and so on.

Dismissing each hypothesis, Newton formulated a new theory: that sunlight consists of rays that are differently refrangible. White light is the mixture of rays which exhibit different colours and different refrangibility. Blue rays are the most refrangible, red rays the least refrangible, and yellow rays in between. Therefore when sunlight is refracted by a prism, it is dispersed into a spectrum. Constituent rays are bent with their own refrangibility.

Newton devised an ‘experimentum crucis’ to prove his new theory (Fig. 6-2)¹⁵. Here, sunlight is dispersed to a spectrum by the first prism ABC. Then, the rays from a part of the spectrum are passed through a hole in a screen to the second prism abc. It was observed that rays of the colour bent most at the first prism were refracted the most again by the second prism.

¹⁵ *Ibid.*, pp. 3078-3079. The phrase ‘experimentum crucis’ was coined by Hooke from Francis Bacon’s ‘instantiae crucis’. See *Micrographia*, p. 54; Lohne (1968), pp. 173-4 & 179.

‘And consequently, the object-glass of any Telescope cannot collect all the rays, which come from one point of an object so as to make them convene at its *focus* in less room than in a circular space, whose diameter is the 50th part of the Diameter of its Aperture; which is an irregularity, some hundreds of times greater, than a circularly figured *Lens*, of so small a section as the Object glasses of long Telescopes are, would cause by the unfitness of its figure, were Light *uniform*’¹⁶.

Newton’s point was that chromatic aberration of a glass (that is, a glass lens) is ‘some hundreds times greater’ than its spherical aberration. Due to chromatic aberration, white light from stars, which consists of a heterogeneous mixture of differently refrangible rays, is dispersed by the lenses of telescopes. It produces halo effects or colours on fringes of the image of the object. The effect is much greater than that of spherical aberration, so aspheric lenses could no longer be seen as the complete answer to the improvement of telescopes.

This led Newton to ‘take *Reflections* into consideration, and finding them regular, so that the Angle of Reflection of all sorts of Rays was equal to their Angle of Incidence’¹⁷. Reflection is affected by spherical aberration, but is free from chromatic aberration. If chromatic, not spherical, aberration is the crucial defect of existing telescopes, it can be avoided by the use of reflecting mirrors. Thus his new theory induced him to make the reflector.

In the letter Newton summarised his theory into thirteen propositions. Their essence is as follows¹⁸:

1. Colours are not qualifications of light derived from refractions or reflections, but original and connate properties, which in divers rays are divers.
2. The same colour belongs to the same degree of refrangibility.
3. The species of colour and degree of refrangibility proper to any particular sort of rays, is not mutable by refraction nor by reflection, nor any other cause.
- 4,5,6. Seeming transmutations of colours may be made by the mixture of divers sorts of rays. There are two sorts of colours: simple and compounded. Rays of compounded colours can be separable again.

¹⁶ *Phil. Trans.*, 6 (1672), no. 80, p. 3079.

¹⁷ *Ibid.*

¹⁸ *Ibid.* pp. 3081-3085

7,8. There is no one sort of rays which alone can exhibit whiteness. White is compounded colour and, to compose it, all the primary colours should be mixed in a due proportion.

9,10. A spectrum of prism is produced by the different refrangibility of constituting rays of light.

Newton's theory of colour was dependent on the corpuscular theory of light. He had arrived at the theory by associating each colour with a particular speed or mass of ray. Rays of light are refracted according to their speed or mass¹⁹. Though Newton wanted to conceal his commitment to corpuscular theory, it was too obvious for Hooke to miss.

Newton's First Encounter with Hooke

The day Newton's letter was read at the Royal Society, Oldenburg wrote to Newton assuring him that his theory 'mett both with a singular attention and an uncommon applause'²⁰. Oldenburg asked Newton's permission to print the letter in *Philosophical Transactions*. But he did not mention that at the meeting 'It was ordered also, that this discourse be entered into the register-book: and that the bishop of Salisbury [Seth Ward], Mr. BOYLE, and Mr. HOOKE be desired to peruse and consider it, and bring in a report of it to the Society'²¹.

At the next meeting on 15 February, Hooke's report was read. It was vehemently critical of Newton's theory of light. So it was ordered 'that Mr. HOOKE's paper might be printed afterwards, it not being thought fit to print them together, lest Mr. NEWTON should look upon it as a disrespect, in printing so sudden a refutation of a discourse of his, which had met with so much applause at the Society but a few days before'²².

Hooke's report, which in fact was never published during the lifetimes of Hooke and Newton, begins:

'I have perused the discourse of Mr. NEWTON about colours and refractions, and I was not a little pleased with the niceness and curiosity of his observation. But, tho' I wholly agree with him as to the truth of those he hath alledged, as

¹⁹ For the development of Newton's theory on the basis of the corpuscular mechanism, see Nakajima (1984), Part II, which analyses CUL MS. Add. 3996, ff. 88-135.

²⁰ Oldenburg to Newton, 8 February 1672, *Newton Corresp.*, 1, pp.107-108 (p. 107).

²¹ *Birch*, 3, p. 9.

²² *Ibid.*, p. 10.

having, by many hundreds of trials, found them so; yet as to his hypothesis of solving the phenomæna of colours thereby, I confess, I cannot see yet any undeniable argument to convince me of the certainty thereof'²³.

Hooke's criticism was directed, not at Newton's experimental observations, but at the theory explaining them. Hooke pointed out that Newton's theory was not necessary, 'for the same phenomænon will be solved by my hypothesis, as well as by his'.

Hooke set his own theory against Newton's. His basic supposition was:

'that *white* is nothing but a pulse or motion, propagated through an homogeneous, uniform, and transparent medium: and that colour is nothing but the disturbance of that light, by the communication of that pulse to other transparent mediums, that is, by the refraction thereof'²⁴.

Hooke's theory was the pulse theory of light, and he continued that he could not assent to 'the proposition, that colours are not qualifications of light, derived from refractions, or ref[lections] of natural bodies, but original and connate properties'²⁵.

Newton believed that white light is the most heterogeneous aggregation of diverse rays, while for Hooke it is the simplest and most uniform manifestation of light. Hooke argued that colours are generated by the qualification of uniform light. He disputed Newton's idea arguing 'why there is a necessity, that all those motions, or whatever else it be that makes colours, should be originally in the simple rays of light'²⁶.

In the latter part of the paper, Hooke attacked Newton's corpuscular theory of light directly:

'But grant his first proposition, that light is a body, and that as many colours as degrees thereof as there may be, so many sorts of bodies there may be, all which compounded together would make white; and grant further, that all luminous bodies are compounded of such substances condensed, and that whilst they shine, they do continually send out an indefinite quantity thereof,

²³ *Ibid.*, pp. 10-15 (pp. 10-11). An almost identical paper was sent as a letter from Hooke to Oldenburg. See Hooke to Oldenburg, 15 February 1672, *Newton Corresp.*, 1, pp. 110-116. Hooke wrote 'I had not above three or 4 hours times for the peruall of Mr. Newtons paper and writing of my answer'. See Hooke to Lord Brouncker (?), c. June 1672, *ibid.*, pp. 198-205 (p. 198).

²⁴ *Birch*, 3, p. 11.

²⁵ *Ibid.*, pp. 12-13.

²⁶ *Ibid.*, p. 11.

every way in orbem, which in a moment of time doth disperse itself to the utmost and most indefinite bounds of the universe; granting these, I say, I do suppose there will be no great difficulty to demonstrate all the rest of his curious theory'²⁷.

Another point of Hooke's criticism was why Newton wholly laid aside 'the thoughts of improving telescopes and microscopes by refractions; since it is not improbable, but that he, that hath made so very good an improvement of telescopes by his own trials upon reflection, would, if he had prosecuted it, have done more by refraction'²⁸. He emphasised that 'The truth is, the difficulty of removing that inconvenience of the splitting of the ray, and consequently of the effect of colours, is very great; *but yet not insuperable*'²⁹ (author's italics).

Newton received Hooke's criticism from Oldenburg on 19 February 1672³⁰. It took almost four months for Newton to answer it, and his letter was read at the Royal Society on 12 June³¹.

Newton's letter showed the result of his comparative calculation of the spherical aberration of a reflecting concave mirror and a refracting convex lens, having the same apertures and focal lengths. He estimated 'that the errors of the refracting Convex are so far from being less, that they are more then sixteen times greater then the like errors of the reflecting *Concave*; especially in great apertures & that without respect to the heterogeneous constitution of light'³². If we add the effect of chromatic aberration of the refracting lens, the advantage of the reflecting telescope was clear.

Newton mentioned his model of light in the letter, and emphasised that the corpuscular model was not a basic premise for his theory:

' 'Tis true, that from my Theory I argue the corporeity of light; but I doe it without any absolute positivenessse, as the word *perhaps* intimates, & make it at most but a very plausible consequence of the Doctrine, & not a fundamentall supposition'³³.

²⁷ *Ibid.*, p. 14.

²⁸ *Ibid.*, p. 12.

²⁹ *Ibid.*

³⁰ Newton to Oldenburg, 20 February 1672, *Newton Corresp.*, 1, pp. 116-117 (p. 116).

³¹ Newton to Oldenburg, 11 June 1672, *ibid.*, pp. 171-193; *Birch*, 3, p. 52. The letter was printed in *Phil. Trans.*, 7 (1672), no. 88, pp. 5084-5103.

³² *Newton Corresp.*, 1, pp. 172-173.

³³ *Ibid.*, p. 173.

Newton made a concession to the wave theory of light, which supposed light to be waves of pulses in the ether in space:

'assuming the rays of Light to be small bodies emitted every way from shining substances, those when they impinge on any refraction or reflecting superficies, must as necessarily excite vibrations in the Æther, as stones do in water when thrown into it'³⁴.

Thus corpuscles of light are accompanied with vibrations of ether. This model has the advantage that it can explain the production of heat by the sun, the emission of light from highly-agitated substances, colours of thin transparent plates and bubbles, and other phenomena³⁵.

Newton did not, however, admit that the light itself was a vibration:

'For to me the fundamentall supposition it selfe seemes impossible; namely that the waves or vibrations of any fluid can like the rays of Light be propagated in streight lines, without a continuall & very extravagant spreading & bending every way into the quiescent Medium where they are terminated by it'³⁶.

Newton also criticised Hooke's idea that all colours were mixtures of the two primary colours, blue and red. He discussed the difference between primary or original colours and compounded colours³⁷.

Westfall's Interpretation of the Hooke-Newton Controversy

As has been shown, in the optical controversy of 1672 Hooke raised two points of dispute: the theory of colours, and the respective merits of reflectors and refractors. Previous research has focused only on the analysis of the former point, and has resolved into two distinctly different interpretations by professional historians of science: Westfall's and Sabra's³⁸. The following introduces each interpretation and compares them, and the author then proposes his own interpretation which is intended to unify

³⁴ *Ibid.*, p. 174.

³⁵ *Ibid.*

³⁶ *Ibid.*, p. 175.

³⁷ *Ibid.*, pp. 179-181.

³⁸ Though their interpretations date from the 1960s, almost no analysis been made since of the Hooke-Newton controversy except for Nakajima (1984).

and supersede preceding interpretations.

In the first half of the twentieth century, the controversy between Hooke and Newton was explained as a conflict between the corpuscular and wave theories of light³⁹. Westfall, who belonged to the first generation of professional historians of science, saw the matter differently. He argued that 'Not the nature of light but the nature of qualities, not undulatory vs. corpuscular but mechanical vs. peripatetic — these were the primary concerns of the critics'⁴⁰. He believed that Hooke subscribed to the latter, *i.e.*, the modification (or qualification) theory of light. Westfall appreciated that Newton's new theory about light and colours 'constitutes a reasoned attack on the doctrine of modification'⁴¹.

It is true that Newton claimed that 'Colours are not *Qualifications* of Light, derived from Refractions or Reflections of natural Bodies (as 'tis generally believed,) but *Original* and *connate properties*'⁴². Hooke clearly opposed this view and wrote that 'I cannot assent to...the proposition, that colours are not qualifications of light'⁴³. The idea of 'qualification' was really at the centre of the controversy.

Was the concept of qualification peripatetic? Westfall's answer was 'yes'. He traced back the origin of the qualification theory of light to ancient Greek philosophy, and described Aristotle's theory of colour in his paper:

'The fundamental assumption of this conception of colors, stated from the point of view of the *Meteorologica*, holds colors to be modifications of pure light. A second assumption equates strength with brilliance. Thus red is considered the nearest approach to white — produced when strong light is modified by a dark medium or reflected from a dark surface. As the strength of light declines and the admixture of darkness increases, there appear first green and finally violet, dark, weak, and the last step before blackness'⁴⁴.

Westfall argued that Aristotle's modification theory was accepted during the Middle Ages, as shown by Robert Grosseteste (c. 1168-1253), Witelo (c. 1230/35-1275) and others, and beyond until Descartes. Westfall writes:

³⁹ For example, Rosenfeld (1928); Ronchi (1939), Chap. 4 & 5. In the latter half of the century, Badcock (1962) adopted the same interpretation.

⁴⁰ Westfall (1962b), p. 47.

⁴¹ Westfall (1962a), p. 353.

⁴² *Phil. Trans.*, 6 (1672), no. 80, p. 3081.

⁴³ *Birch*, 3, pp. 12-13.

⁴⁴ Westfall (1962a), p. 342.

'In formulating his theory of colors Descartes self-consciously played the rebel, casting out the peripatetic doctrine of qualities and colors in order to substitute a mechanical explanation. Little did the rebel comprehend how closely the bonds of tradition still confined him. Even in rejecting the peripatetic doctrine, he accepted, unquestioned and apparently unperceived, basic assumptions concerning colors'⁴⁵.

Although Hooke 'presented a brief and devastating critique of the Cartesian theory of colors in his *Micrographia*, he was fully under the domination of the Cartesian vision of nature'⁴⁶. Thus 'It remained for Isaac Newton to challenge the traditional concept of modification'⁴⁷.

Westfall's interpretation faced the difficulty that Hooke had never argued that colours appeared by the mixture of darkness to light. Westfall sought to overcome this by reference to Hooke's explanation of the production of a spectrum by refraction, which was discussed in *Observ. IX of Micrographia*⁴⁸.

To understand Hooke's explanation, we need to remember that he made two basic suppositions. The first was that light comprises vibrations or pulses of the ether produced by a quick, short vibrating motion inside luminous bodies⁴⁹. The second supposition was that there are two primary colours, blue and red, which he defined as follows:

'That Blue is an impression on the Retina of an oblique and confus'd pulse of light, whose weakest part precedes, and whose strongest follows. And, that Red is an impression on the Retina of an oblique and confus'd pulse of light, whose strongest part precedes, and whose weakest follows'⁵⁰.

Colours of light are defined by the order of strong and weak pulses. When white light is qualified by refraction, &c., the two primary colours blue and red appear, and all other colours are generated by their mixture in various proportions.

The production of the spectrum of the sun by refraction was explained

⁴⁵ *Ibid.*

⁴⁶ *Ibid.*, p. 347.

⁴⁷ *Ibid.*, p. 348.

⁴⁸ *Micrographia*, pp. 57-94 (especially pp. 62-64); Westfall (1962a). Westfall's own discussion is very short, and is not easily understood. So the description below is supplemented by the original argument from *Micrographia*.

⁴⁹ *Micrographia*, pp. 54-56.

⁵⁰ *Ibid.*, p.64.

according to this definition of colours (Fig. 6-3: caption: Hooke's production of spectrum by refraction in *Observ. IX of Micrographia*). In *Micrographia* Hooke used the illustration on the left, but it is much too complicated and the author's simplified version is shown on the right. Here a pulse of light BCDE falls obliquely on the surface AB, and is refracted by a dense medium. The wave front of the pulse is made oblique to its direction by refraction, because the speed of light differs in different mediums. Hooke supposed that light travels faster in a dense medium due to the scarcity of undulating ether⁵¹.

Consider the wave front BE. Its end B precedes its other end E, and so should be somewhat more '*obtunded, or impeded* by the resistance of the transparent medium' than E⁵².

'[The ray] have its side HH more deadened by the resistance of the dark or quiet *medium*...Whence there will be a kind of deadness superinduc'd on the side HHH, which will continually increase from B, and strike deeper into the Ray by the line BR'⁵³.

It is apparent that BO, bordering the darkness, is more deadened than BR. If we put our eye at OR, we see the weaker ray BO and stronger ray BR together. Because of the obliqueness of the wave, the wave front arrives at O slightly earlier than R. Thus we see the comparatively weaker pulse first, and then the stronger; and this shows blue according to Hooke's definition of colours⁵⁴.

On the other side of the ray,

'the end A of the pulse AH will be promoted, or made stronger, having its passage already prepar'd as 'twere by the other parts preceding, and so its impression will be stronger; And because of its *obliquity* to the Ray, there will be propagated a kind of a faint motion into QQ'⁵⁵.

Therefore the ray AK is stronger than the ray AM. If we put our eye at KM, the strong pulse comes first at K, and then the weak pulse at M. This is red by Hooke's

⁵¹ *Ibid.*, pp. 57-58.

⁵² *Ibid.*, pp. 63-64.

⁵³ *Ibid.*, p. 63.

⁵⁴ See note 50.

⁵⁵ *Micrographia*. p. 63.

definition⁵⁶. Now we have the two primary colours of blue and red, all other colours can result from mixing them.

Westfall interpreted Hooke's explanation as definitely a version of Aristotle's modification theory. He even emphasised it with the sentence in parentheses:

'According to Hooke's explanation the forward angle of the oblique pulse is "deadened" by the resistance of the dark bordering the beam. (Again the role of darkness in modifying light!) The further the pulse moves from the refracting surface, the further the deadness penetrates the ray'⁵⁷.

For Westfall, the controversy between Hooke and Newton in 1672 was a battle between a defender of Aristotle's modification theory and a natural philosopher who challenged it with a new theory of light and colours.

Sabra's Interpretation of the Hooke-Newton Controversy

A.I. Sabra applied a different interpretation to the same controversy. He argued that a decisive advance in colour theory had already been achieved by Descartes. He wrote that 'Before him writers on the subject were content to explain colours as a result of the mixture of light and darkness'⁵⁸. This implies that the tradition of Aristotle's modification theory, which holds that colours are generated by the admixture of darkness to white light, was brought to an end by Descartes.

Sabra argued that the contest between Newton and his adversaries was over a different topic. He wrote:

'It was this interpretation of the constitution of white light and the role which Newton assigned to the prism in the production of colours that occupied the greatest and most important part of the controversy between Newton and the critics of his theory, Hooke, Pardies and Huygens. They all suspected from Newton's expressions that he was inclined towards an atomistic interpretation of light which they found disagreeable'⁵⁹.

Critics accepted Newton's claim that he had proved experimentally the fact that light of different colours had different and constant refrangibility,

⁵⁶ See note 50.

⁵⁷ Westfall (1962a), p. 347.

⁵⁸ Sabra (1967), p. 67.

⁵⁹ *Ibid.*, p. 242.

'But they correctly refused to grant an equal status to Newton's doctrine of white light as a heterogeneous aggregate. Hooke in particular envisaged a new formulation of his pulse hypothesis which would have fully taken the experimental results into account without conceding the original heterogeneity of white light'⁶⁰.

Thus the heart of the Hooke-Newton controversy was found in their different ideas of white light: homogeneous vs. heterogeneous aggregate. It is clear that the difference was rooted in their different models of light. Newton supposed that white was the aggregate of the rays of different colours, which were differently refrangible. It was based on the corpuscular theory of light. Hooke adopted, on the contrary, the wave or pulse theory of light in which white was the colour exhibited by original uniform light. Colours were generated when this uniform pulse was disturbed and destroyed. Hooke called this disturbance modification or qualification.

It was impossible for the wave theory of light to envisage that any kind of light could be originally heterogeneous, because the respective elements of the undulatory motion of the ether in space, such as direction, amplitude, and frequency, could take only a single value at a given point and time. The motion had to be a definite one. Hooke could not understand why it was necessary that multifarious rays or 'all those motions, or whatever else it be that makes colours, should be originally in the simple rays of light'⁶¹, *i.e.* white light. Colours were to be procured from white by refraction or other processes.

However, Hooke made an interesting reservation:

'It is true, I can in my supposition, conceive the white or uniform motion of light to be compounded of the compound motions of all the other colours, as any one strait and uniform motion may be compounded of thousands of compound motions'⁶².

'for the ray is like the string, strained between the luminous object and the eye, and the stop or fingers is like the refracting surface, on the one side of which the string hath no motion, on the other a vibrating one. Now we may say

⁶⁰ *Ibid.*, p. 233.

⁶¹ *Birch*, **3**, p. 11.

⁶² *Ibid.*, p. 14.

indeed and imagine, that the rest of straightness of the string is caused the cessation of motions, or coalition of all vibrations; and that all the vibrations are dormant in it'⁶³.

As Sabra suggested, it was possible for Hooke to *imagine* that white light was a superposition of heterogeneous waves, though it was not heterogeneous in a real or physical sense. When white uniform light passes through a prism, it is resolved into component pulse motions of various colours, of which it was imagined to be composed. Once split, a component of this motion could never be split or modified again by another refraction. Sabra introduced a mathematical concept of Fourier analysis here to assist our understanding⁶⁴. Though this sounds somewhat unhistorical, it has considerable value in illuminating Hooke's explanation.

Two Kinds of Modification Theories of Light: Modification Theory Reconsidered

Westfall maintained that the modification theory, which Hooke supported, was Aristotle's. The theory supposed that colours of light were produced by the admixture of darkness to white light. Sabra had a different interpretation of the idea of modification. For him, modification theory was almost identical to the wave theory of light, which regarded white light as an original and uniform wave or pulse.

The author believes that neither interpretation encapsulates the real meaning of the word 'modification' or 'qualification' in the mid-seventeenth century, though Sabra's interpretation has the advantage of understanding Hooke's theory of light⁶⁵. When we consider the development of Newton's theory of colour, he, not Hooke, was much more affected by Aristotle's conception of colours.

Let us first examine Westfall's interpretation. The author finds a serious difficulty in this. Westfall claimed that Hooke defended Aristotle's modification theory, even though he had never argued that colours appeared by the mixture of darkness to light. Westfall had to invoke Hooke's explanation of the spectrum which seemed to depend on Aristotle's modification theory. However, this line of reasoning did not do justice to Hooke.

One objection to Westfall's interpretation is the fact that Hooke accepted the

⁶³ *Ibid.*, p. 11.

⁶⁴ Sabra (1967), p. 280. It is natural that Newton found it difficult to understand Hooke's explanation. He wrote that 'I cannot well conceive how they [dispersed rays] should coalesce into one *uniforme* motion'. He believed that dispersed rays should be further dispersed by a subsequent refraction. *Newton Corresp.*, 1, p. 177.

⁶⁵ The description below relies on the author's paper, Nakajima (1984).

immutability of rays of light. This was an essential element of Newton's new theory of light and colours, and it evidently contradicted Aristotle's modification theory.

What was this immutability? In the third proposition of Newton's letter of 1672 in which he announced the new theory, he wrote that "The species of colour, and degree of Refrangibility proper to any particular sort of Rays, is not mutable by Refraction, nor by Reflection from natural bodies"⁶⁶. Let us remember Newton's *experimentum crucis* which was proposed in his letter (Fig. 6-2). If the colours had been produced by the mixture of the light with darkness at the first prism, the second prism would have added more darkness, and the spectrum colours should have either shifted toward blue or suffered mutation to less brilliant colours. Newton's third proposition insisted that such mutation did not happen.

In his critical report on Newton's theory, Hooke referred to 'the third proposition, to which I do readily assent'⁶⁷. He could not have assented to it if he had subscribed to Aristotle's modification theory. Hooke even had his own theoretical explanation for the immutability of rays:

'that is, that part of the split ray, which is most bent, exhibits a blue, that which is least, a red, and the middle parts middling colours; and that those parts will always exhibit those colours till the compound motions are destroyed, and reduced by other motions to the simple and uniform pulse as it was at first'⁶⁸.

This explanation is clearly based on the wave theory of light, and would be easily understood from Sabra's interpretation based on Fourier analysis. Hooke did not accept Newton's theory of the heterogeneous constitution of white light. For him, white light was uniform in its physical existence. Nevertheless, it was possible for Hooke to imagine it as a superposition of heterogeneous pulses. When white light passes through a prism, it is resolved into its component pulses, each of which exhibits a specific colour and refrangibility.

It is worth pointing out that Hooke in his Royal Society report referred to the possibility that 'the compound motions are destroyed, and reduced by other motions to the simple and uniform pulse as it was at first'. He cited the case 'where the split ray is made by another refraction, to become intire and uniform'⁶⁹. White light dispersed

⁶⁶ *Phil. Trans.*, **6** (1672), no. 80, p. 3081.

⁶⁷ *Birch*, **3**, p. 13.

⁶⁸ *Ibid.*

⁶⁹ *Ibid.*

by refraction to a spectrum could be reconstituted to uniform white light by another refraction. If the white light had been contaminated with darkness mixed in at refractions, this could never happen.

A second objection to Westfall's interpretation is the fact that Hooke consistently explained colours as sequences of weak and strong pulses, not as mixtures of light and darkness. As well as the spectrum, an example of this is found in *Micrographia* where he considered colours which appeared on thin transparent bodies like Muscovy Glass (muscovite)⁷⁰.

Fig. 6-4 illustrates his reasoning. Its original version is taken from *Micrographia*, but the author has simplified it for the convenience of readers. Here white light *ab* falls on a thin transparent body from the upper right. Part of the pulse is reflected on the upper side *AB* of the body, and the other part from the lower side *EF*. The pulse *ef*, reflected from the lower side, is weaker than the pulse *cd*, reflected from the upper side *AB*. This is because it is refracted twice at the surface *AB*, and also because of the time spent passing and returning between the two surfaces *AB* and *EF*. The pulse *ef* lags behind *cd* due to the difference in the lengths of their paths. When the reflection reaches the eye, the strong pulse *cd* precedes the weak pulse *ef*. This meets Hooke's definition of the colour red. When the laminated body is thicker the pulse *ef* lags further behind, 'And in this case it [*ef*] will rather seem to precede the following stronger pulse [*c'd*]', then to follow the preceding one [*cd*}'. Weak pulse *ef* precedes strong pulse *c'd*', 'and consequently a *Blue* will be generated'⁷¹. When the laminated body is much thicker, the pulse *ef* eventually lags behind the pulse *c'd*', and red appears again. Thus colours appear periodically as the thickness of the plate increases.

Hooke wrote that from the example it is apparent:

'that for the production of Colours there is not necessary either a great refraction, as in the Prisme; nor Secondly, a determination of Light and shadow, such as is both in the Prisme and Glassball'⁷².

Only the disposition of strong and weak pulses determines the colour of light. Shadow or darkness do not play any essential role in the generation of colours of light.

Hooke's explanation of colours on a thin plate showed the typical character of

⁷⁰ *Micrographia*, pp. 64ff.

⁷¹ *Ibid.*, p. 66.

⁷² *Ibid.*, p. 54.

the wave theory of light. In that sense, Sabra was correct. But his historiography had its own defect. To Sabra, Hooke's version of the modification theory was almost equivalent to the wave theory of light. He supposed that Aristotle's modification theory had been completely discredited by Descartes.

However, this is contradicted by Newton's student notebook, which is usually called *Quaestiones*⁷³. He wrote:

'Coulors [*sic*] arise either from shadow intermixed wth light or or [*sic*] stronger & weaker reflection'⁷⁴.

This shows that Newton was familiar with Aristotle's modification theory. The theory still survived even after Descartes.

Indeed, the note following the citation above shows that Newton developed his new theory of colours on the basis of the Aristotle's modification theory⁷⁵. He had already adopted the corpuscular theory of light, and he wrote 'Note yt slowly moved rays are refracted more then swift ones...from ye slow ones bluew...from ye Swift one red'⁷⁶. Newton associated colour with the speed of light globules, and then with their refrangibility⁷⁷. His explanation preserved an important feature of Aristotle's modification theory, which supposed that red is the strongest light and blue the weakest. Newton associated red with the fastest (that is, strongest) light globules, and blue with the slowest. Later in his notebook, he associated the strength not with the speed but with the mass of light globules⁷⁸.

Then what was the modification theory of colours? Robert Boyle left an informative text in his *Experiments and Considerations Touching Colours* (1664):

'I encline to take Colour to be a Modification of Light...But whether I think this Modification of the Light to be perform'd by Mixing it with Shades, or by Varying the Proportion of the Progress and Rotation of the *Cartesian Globuli*

⁷³ CUL, MS Add. 3996, ff. 88-135. The manuscript is transcribed and published with commentary in McGuire & Tamny (1983). Before this, the following papers had analysed parts of manuscript: Hall (1948); Westfall (1962a, 1962c); Lohne (1965); Hendry (1980).

⁷⁴ MS Add. 3996, f. 105v.

⁷⁵ For further details, see Nakajima (1984), Part II. Newton was deeply influenced by Walter Charleton's *Phisiologia Epiculo-Gassendo-Charltoniana* (London, 1654), which introduced Gassendi's theory to England.

⁷⁶ MS Add. 3996, f. 122v.

⁷⁷ Hendry (1980), pp. 238-242; Westfall (1962), pp. 349-350.

⁷⁸ MS Add. 3996, f. 123r.

Cælestes, or by some other way which I am not now to mention, I pretend not here to Declare'⁷⁹.

This suggests that the modification of light was not restricted to the admixture of darkness, but was thought to be performed in several ways.

Newton himself left a similar text in his *Optical Lectures*, which he delivered at the beginning of the 1670s.

'As for the opinions of other philosophers, they have said that colours are generated from shadow and light mixed in various ways, or from a spinning of little balls or their various pressures, or, finally, from the various ways in which a certain aetherial medium is vibrated, assuming that light is produced by an impulse of the vibrating aether carried to the retina'⁸⁰.

Then what was the key feature of the modification theory? Newton continued:

'I would stray too far from the path were I to attempt to refute these vies individually. Nor is it necessary that I do so, since they all agree in a certain common error; namely, the modification of light by which it exhibits individual colours is not innate to it from its source but is acquired by being reflected or refracted'⁸¹.

Those theories which assumed that white is original and uniform, and that colours were produced by reflection or refraction, were commonly called 'modification' or 'qualification' theories. They included both Aristotle's theory of colour and the wave theory of light. Descartes' theory of colour was another version of modification theory, and it was differentiated from the other two in the time of Hooke and Newton.

Hooke argued that light was pulses of ether in space. When it was simple and uniform, its colour was white. When white light was dispersed by refraction with a prism or reflection with transparent thin plates, colours were produced. The particular colour was defined by the order of strong and weak pulses. If a strong pulse preceded and a weak pulse followed, red appeared. The opposite showed blue. Red and blue were primary colours, and 'all the varieties of colours imaginable are produc'd

⁷⁹ Boyle (1664), p. 90. This book is also printed in *Boyle New Works*, 4. See its p. 59.

⁸⁰ *The Optical Papers of Isaac Newton*, vol. 1, Alan E. Shapiro (ed.), (Cambridge, 1984), pp. 84-85.

⁸¹ *Ibid.*

from several degrees of these two colours'⁸². Newton argued against the uniformity of white light. For him white was the apparent colour of a heterogeneous mixture of rays of light, and colours were the innate property of rays of light.

The difference between Hooke and Newton was in their models of light. Hooke took the wave theory of light, whereas Newton believed the corpuscular theory. And in their controversy, the difference was manifested in their respective views of the theory of colour.

Though Westfall tried to devalue Hooke as peripatetic theorist of light, Hooke simply sided with wave theory of light. Actually, not Hooke but Newton developed his theory of light as an extension of the peripatetic conception. Hooke was very much open-minded in the fact that he accepted all the Newtonian experiment as true. He simply argued against the Newtonian corpuscular model.

Looking back with the advantage of modern optics, both Hooke and Newton were right because light has the duality of the nature of the wave and the corpuscle. In this respect, the controversy could have never come to an end: neither side was right or wrong. But in the following Part, we will see that we can identify a victor in another context even using the same historical materials.

Part B When Hooke's Telescope Encountered Newton's: The Controversy Revisited

In Part A, we saved Hooke from the stigma of being regarded as an ancient peripatetic philosopher. He quite rationally argued for the wave theory of light. In the preceding Chapters, we also saw that Hooke stood at the forefront of observational astronomy in the latter half of the seventeenth century. By combining these two new 'discoveries' about Hooke, we can grasp another thread of the Hooke-Newton 'optical' controversy in 1672.

The author has already written that in the controversy Hooke raised two points of dispute: the theory of colours, and the respective merits of reflectors and refractors. But previous research has focused only on the analysis of the former point. After the two 'discoveries', which elevate Hooke's image to that of an able and modern scientist, it is time to go back to the beginning of the controversy and start to look through it against the latter point.

But first we should clarify the general historical setting against which Newton first appeared to the Royal Society at the beginning of the 1670s. The Galilean

⁸² *Micrographia*, p. 74. Hooke sometimes took yellow, instead of red, as a primary colour.

Observational Paradigm had entered its second stage in 1655, when Huygens discovered Saturn's satellite with a telescope. Huygens and Wren sought the true model of Saturn's ring in the latter half of the 1650s. In the mid-1660s, Hooke and Cassini observed the rotation of Jupiter and Mars. Hooke used long telescopes of 12, 36, 60 feet and so on. Hevelius was interested in Hooke's observations, and asked Hooke to send him a long telescope. It was only two years before Newton's debut that Hooke fulfilled the request. As Fig 2-5 in Chapter 2 suggested, the telescope was a focal point of discussion at the Royal Society. Even King Charles II had a long telescope, and he was interested in telescopic observations. With his 36-foot-telescope at Whitehall, he observed Saturn and its satellites and those of Jupiter in 1661 with Huygens and others. Against this background, it was not so extraordinary that when Isaac Barrow took Newton's reflector to the Royal Society at the end of 1671, it was brought to Whitehall for the King's perusal⁸³. But we should bear in mind that within the learned world, Newton's reflector was regarded as only one of a number of interesting telescopes.

Newton was elected Fellow of the Royal Society on 11 January 1672. The record of the meeting reads:

'Mention was made of Mr. NEWTON'S improvement of telescopes by contracting them; and that that, which he had sent to the Society of that kind to examined, had been by the king, and considered also by the president, Sir ROBERT MORAY, Sir PAUL NIELE, Dr. CHRISTOPHER WREN, and Mr. HOOKE at Whitehall; and that they had so good opinion of it'⁸⁴.

This record brings out several points. First, Newton's reflector was regarded primarily as an attempt to contract telescopes⁸⁵. We have already seen some attempts to contract long telescopes. For example, in 1665 Hooke suggested filling the tube with liquid. The following year, he showed the Society another way of contracting telescopes by the use of reflecting mirrors⁸⁶. So it was natural that Newton's reflector was considered as a method of contracting telescopes, and in fact a reflector does have a shorter body than a refractor of the same magnifying power.

A second point is that those who scrutinised Newton's reflector had expert knowledge of telescopes. We have seen that Neile, Wren, and Hooke had a wealth of

⁸³ *Birch*, 3, p. 1.

⁸⁴ *Ibid.*

⁸⁵ Simpson (1981), p. 111.

⁸⁶ See Chapter 4, 'Hooke's Methods for Contracting Telescopes'.

experience of telescope-making and telescopic observations, and Moray exchanged information on telescopes with Huygens. Their judgement would have been of unimpeachable standard.

Another point is that their perusal of Newton's telescope at Whitehall predated its official examination at the Royal Society. It was at a Society meeting on 18 January, a week after the meeting referred to above, that 'Mr. NEWTON'S new telescope was examined and applauded'⁸⁷.

Hooke's Criticism and Newton's Response

Before Newton's telescope was examined at the Society, Oldenburg had written to Newton that his telescope 'having been considered, and examined here by some of ye most eminent in Opticall Science and practise, and applauded by them, they think it necessary to use some means to secure this Invention from ye Usurpation of forreiners'⁸⁸. It was decided at the Society meeting on 11 January that 'a description and scheme of it should be sent by the secretary in a letter to Mons. HUYGENS then at Paris'⁸⁹.

Pleased with the good response to the reflector, Newton sent a letter explaining his 'New Theory about Light and Colors'. It was read at the meeting of the Royal Society on 8 February 1672⁹⁰. Its purpose was to address the theoretical background of his reflector: the different refrangibilities of rays of light.

At the next meeting on 15 February, Hooke read a criticism of Newton's letter which he had prepared at the direction of the Society. He pointed out that Newton's theory was not necessary 'for the same phenomænon [Newton introduced] will be solved by my hypothesis, as well as by his'⁹¹. The author has explained in Part A that Hooke's criticism was directed at Newton's corpuscular theory of light. Hooke argued that the differing refrangibility of rays of light could be explained by his wave (or pulse) theory of light. So Hooke definitely agreed with the central theme of Newton's theory of light, without reservation. Newton could not have expected that response from such an advocate of refractors, because he was sure that the different refrangibility necessarily limited the scope for the improvement of refractors.

⁸⁷ *Birch*, 3, p. 4.

⁸⁸ Oldenburg to Newton, 2 January 1672, *Newton Corresp.*, 1, pp. 73-74 (p. 73).

⁸⁹ *Birch*, 3, p. 4. Oldenburg had already sent Huygens a short preliminary report on Newton's reflector. See Oldenburg to Huygens, 1 January 1672, *Oldenburg Corresp.*, 8, pp. 443-447 (pp. 444-446).

⁹⁰ Newton to Oldenburg, 6 February 1672, *Newton Corresp.*, 1, pp. 92-107, *Birch*, 3, p. 9; *Phil. Trans.*, 6 (1672), no. 80, pp. 3075-3087.

⁹¹ *Birch*, 3, pp. 10-15 (p. 11).

But Hooke did not agree with that claim, and maintained that improvement was still feasible. He wrote:

'I am a little troubled, that this supposition [Newton's theory of colours] should make Mr. NEWTON wholly lay aside the thoughts of improving telescopes and microscopes by refractions; since it is not improbable, but that he, that hath made so very good an improvement of telescopes by his own trials upon reflection, would, if he had prosecuted it, have done more by refraction. And that reflection is not the only way of improving telescopes, I may possibly hereafter shew some proof of. The truth is, the difficulty of removing that inconvenience of the splitting of the ray [dispersion], and consequently of the effect of colours, is very great; but yet not insuperable'⁹².

Hooke emphasised that he had frequently used both reflecting telescopes and reflecting microscopes. His judgement was firmly based on his own experience:

'I have made many trials, both for telescopes and microscopes by reflection, which I have mentioned in *Micrographia*, but deserted it as to telescopes, when I considered, that the focus of the spherical concave is not a point but a line, and that the rays are less true reflected to a point by concave, than refracted by a convex; which made me seek that by refraction'⁹³.

Though reflecting telescopes are not mentioned in *Micrographia*, it certainly contains references to the reflecting microscope⁹⁴.

In his report, Hooke referred to his trial of a reflecting telescope, which had been unsuccessful. He could not 'find any effect of it by one of six foot radius, which, about seven or eight years since, Mr. REEVE made for Mr. GREGORY, with which I made several trial'⁹⁵. As will be discussed later, James Gregory did employ Reeve to produce a reflecting telescope of his own design in 1662.

Newton received Hooke's report from Oldenburg on 19 February 1672, and the next day he wrote back to Oldenburg assuring him that 'You shall very suddenly have

⁹² *Ibid.*, p. 12.

⁹³ *Ibid.*

⁹⁴ *Micrographia*, sig. f2r. At the meeting of the Royal Society on 5 April 1711, Waller showed a reflecting microscope which Hooke had left. See Royal Society Journal Book, 10, p. 270.

⁹⁵ *Birch*, 3, p. 12.

my answer'. He also wrote that he was 'glad that so acute an objecter hath said nothing that can enervate any part of it [his discourse]⁹⁶. However it took almost four months for Newton to fulfil his promise. He finally sent his counterargument on 11 June, and the letter was read at the Royal Society immediately⁹⁷.

In this letter Newton presented his calculation of the comparative spherical aberration of a reflecting concave mirror and a refracting convex lens, both of the same aperture and focal length. He estimated 'that the errors of the refracting Convex are so far from being less, that they are more then sixteen times greater then the like errors of the reflecting *Concave*; especially in great apertures & that without respect to the heterogeneous constitution of light'⁹⁸.

But Newton did not explain why his comparison had been between a reflecting concave mirror and a refracting convex lens *with the same focal length*. Hooke took up this point in a letter, probably sent to Brouncker around June 1672, as follows:

'Next as to his answer about the comparison between object glasses made by Reflection & refraction I judge he doth both mistake me and the thing it self. for had he examind t[he] refraction & reflection from the same sphæricall surface he might easily have found that refraction doth make the rayes of one point coalesse from the severall parts of the area of the object-glasse in a much less area then Reflection'⁹⁹.

Hooke urged that Newton should have compared a reflecting concave mirror and a refracting convex lens *with the same radius of curvature*¹⁰⁰. They had different presuppositions for calculation, each of which had its own rationale.

Hooke raised the same objection at the Society meeting of 3 July, three weeks after Newton's counterargument was produced. He said that he found 'that a refracting object-glass collected more rays to a point than a reflecting one, both being of the same sphere'¹⁰¹. At the next meeting, Hooke added that 'he had made a refracting object-glass upon the same sphere with a reflecting one, and found, that it represented

⁹⁶ Newton to Oldenburg, 20 February 1672, *Newton Corresp.*, 1, pp. 116-117 (p. 116).

⁹⁷ Newton to Oldenburg, 11 June 1672, *ibid*, pp. 171-193; *Birch*, 3, p. 52.; *Phil. Trans.*, 7 (1672), no. 88, pp. 5084-5103.

⁹⁸ *Newton Corresp.*, 1, pp. 172-173.

⁹⁹ Hooke to Lord Brouncker (?), c. June 1672, *ibid*, pp. 198-204 (p. 199).

¹⁰⁰ *Ibid*. When r is the radius of curvature and f is the focal length, for a concave mirror $f=r/2$. For a convex lens, f is roughly proportional to the square of r , but also depends on the refractive index of the glass and its thickness.

¹⁰¹ *Birch*, 3, p. 56.

the object brighter, of the same charge and the same aperture with a reflecting one'¹⁰².

Hooke's determined criticism seems to have caused Newton considerable offence¹⁰³. From late 1671, Newton had been revising his Cambridge optical lectures for publication, as Collins wrote to Newton in April 1672 that 'A little before Christmas the Reverend Doctor Barrow informed me you were buisy... in preparing 20 Dioptrick Lectures for the Presse'¹⁰⁴. However in May, *i.e.* after Hooke's criticism, Newton answered Collins that he would suspend publication 'finding already by that little use I have made of the Presse, that I shall not enjoy my former serene liberty'¹⁰⁵.

Gregory's Reflecting Telescope

As we saw, in Hooke's first report criticising Newton he mentioned his trial of Gregory's reflecting telescope. This suggests that the idea of using reflection for telescopes dates back considerably before Newton. Indeed, in 1632, Bonaventura Cavalieri (1598-1647) described a reflecting telescope in his book *Specchio ustorio* (Bologna, 1632)¹⁰⁶. Four years later, Mersenne recorded two types of reflecting telescope in his *Harmonie universelle* (Lyon, 1636). Descartes was critical of Mersenne's idea, and wrote that the reflecting telescope could not surpass the refracting telescope.

In continental Europe these ideas appeared sporadically, but in Britain there was a continuing process of development of the reflecting telescope, started in the 1660s by James Gregory (1638-1675)¹⁰⁷. In 1663 he made public the idea of the Gregorian reflector. It used two concave mirrors. The Gregorian was one of three leading types of reflector, the others being the Cassegrainian (one convex and one concave mirror) and the Newtonian (one concave and one plane mirror).

James Gregory was born the youngest son of a clergyman in Drumoak, near Aberdeen. His family and their descendants held more than twenty chairs at Scottish universities and at Oxford over six generations. His elder brother David, the father of the famous Newtonian David Gregory, encouraged him to study mathematics, optics

¹⁰² *Ibid.*, p. 57.

¹⁰³ In addition to the controversy with Hooke, Newton had to endure criticism from Huygens, Pardies, Linus, and others. These controversies are described later.

¹⁰⁴ Collins to Newton, 30 April 1672, *Newton Corresp.*, 1, pp. 146-150 (p. 146); Westfall (1963), pp. 83-84.

¹⁰⁵ Newton to Collins, 25 May 1672, *ibid.*, pp. 161-162 (p. 161).

¹⁰⁶ For the history of reflecting telescopes, see Ariotti (1975) and Simpson (1981), pp. 34ff. Both Ariotti and Simpson mentioned Niccoló Zucchi's reflecting telescope of 1616, which was not made public until 1652.

¹⁰⁷ Simpson (1992), p. 77. For the life of Gregory, see also Turnbull (1939); Simpson (1981); *DSB*, 'James Gregory', 5, pp. 524-530.

and astronomy. James's early research was published under the title of *Optica promota* in 1663, during his short stay in London in 1662-63. He then left for Italy where he studied under Stefano degli Angeli, a student of Cavalieri and Torricelli, until 1667. James Gregory published *Vera circuli et hyperbolae quadrantura* in Padua in that year. On his return to London in 1668, he found that the book was received enthusiastically. He was elected Fellow of the Royal Society in June¹⁰⁸. In late 1668, James was nominated to the new chair of mathematics at St. Andrews University, probably through Moray's intercession. In 1674, a year before his early death, he moved to Edinburgh University to accept a newly-endowed professorship of mathematics. Until then, almost his only channel of advanced mathematics was his correspondence with John Collins in London.

Optica promota contains ninety propositions, of which the first fifty-nine are on catoptrics and dioptrics. They were based on Gregory's reading of al-Haytham, Witelo and so on. He described an intuitive demonstration of a law of refraction, and discussed the formation and disposition of optical images produced by mirrors and lenses¹⁰⁹.

Following proposition 59, the Gregorian reflecting telescope was introduced under the heading of *Epilogus* (Fig. 6-5, left)¹¹⁰. Here he compared three kinds of telescopes: purely dioptric, purely catoptric, and a mixture of mirrors and lenses. The defect of the first was its length and the poor quality of the image, and that of the second was the loss of too much light by reflection. His telescope was of the third kind, intended to avoid the disadvantages of the other two. It consisted of three 'glasses' a primary concave parabolic mirror; a secondary small elliptic concave mirror placed at C, the focus of the primary; and an eyepiece lens at LL. He allowed for the use of spherical mirrors unless the ideal aspheric mirrors could be achieved¹¹¹.

While Gregory was in London in 1662-63, he tried to construct his telescope. He employed Richard Reeve, the most famous optical instrument-maker in London at that time. A. D. C. Simpson presumed that Gregory was introduced to Reeve by Moray¹¹².

About a decade later, Gregory wrote to Collins as follows:

¹⁰⁸ *Birch*, 2, p. 293.

¹⁰⁹ Simpson (1981), p. 45; Simpson (1992), p. 79; Turnbull (1939), pp. 454ff.

¹¹⁰ Gregory (1663), pp. 92-95.

¹¹¹ Simpson (1981), pp. 47-48; Simpson (1992), pp. 80-81.

¹¹² Simpson (1981), pp. 50-51; Simpson (1992), pp. 84-85. He disputes the popular story that Collins introduced Gregory to Reeve, in part because Gregory began his correspondence with Collins in early 1668, six or seven years after the trial of the telescope.

'as for my experiment wth Mr Rives, he could not polish the large concave upon the tool: and I (not knowing anie advantage of the catoptrick telescops above the dioptrick save onlie the shortnes and similitude betwixt the circle & parabola, which is greater then that betwixt the circle & hyperbola) imagined that this great defect in the figure wo[u]ld easily counterbalance these two pettie advantages. Upon this account, & being about to go abroad; I thought it not worth the pains to trouble myself anie further with it, so that the tube was never made'¹¹³.

If we suppose that Hooke used the mirrors which were left with Reeve, it is reasonable to accept Hooke's claim in 1672 that 'I find any effect of it [reflector] by one of six foot radius, which, about seven or eight years since, Mr. REEVE made for Mr. GREGORY, with which I made several trial'¹¹⁴. So we can conclude that Hooke's argument against Newton's reflector was based on his own personal experience with Gregory's.

It is worth noting that the Newtonian reflector itself was envisaged as a modification of the Gregorian, or at least was conceived under its influence¹¹⁵. In May 1672, about three months after Hooke's report on Newton's article, Newton wrote to Oldenburg as follows:

'when I first applied my selfe to try the effects of reflexions, Mr Gregory's *Optica Promota* (printed in ye yeare 1663) being faln into my hands, where there is an Instrument described...wth a hole in ye midst of ye object-Metall to transmit the light to an eye glasse placed behind it; I had thence an occasion of considering that sort of constructions, & found their disadvantages so great yt I saw it necessary before I attempted any thing in ye Practique to alter the designe of them, & place ye eye glasse at the side of the Tube rather then at the middle'¹¹⁶.

Hooke's Trials of Reflectors in the 1670s

Even though Hooke's opposition to the Newtonian reflector was not

¹¹³ Gregory to Collins, 23 September 1672, *Newton Corresp.*, 1, pp. 239-241 (p. 240). The letter is also printed in Turnbull (1939), pp. 241-244.

¹¹⁴ *Birch*, 3, p. 12.

¹¹⁵ Simpson (1981), pp. 68-69.

¹¹⁶ Newton to Oldenburg, 4 May 1672, *Newton Corresp.*, 1, pp. 153-155 (p. 153).

unreasonable, he could be reproached for his hasty judgement. He could have investigated some of the possibilities for improving reflectors. In fact while he was arguing against reflectors, Hooke was applying himself to construct practical reflectors of both the Gregorian and Newtonian type.

Hooke's attempts to make better reflecting telescopes date back even before Newton was elected Fellow of the Royal Society on 11 January 1672. The Journal Book of the Society on that date reported that Hooke said 'that he did endeavour to make such a Telescope himself, and to find out a Metall not obnoxious to tarnishing'¹¹⁷. Hooke had already embarked on, or was embarking on, the construction of a Newtonian telescope with Cock, who had taken Reeve's place.

The attempt was begun at the direction of the Society. Collins records that the Royal Society 'gave Mr Cox order to make one after the same manner of [Newton's] Contrivance 4 foot long'¹¹⁸, much bigger than the 6 1/3 inch reflector Newton sent to the Society.

The first results were reported to the Society meeting on 25 January 1672, only a week after the Society examined Newton's reflector. It is recorded that:

'There was produced a reflecting telescope of four feet long, of Mr. NEWTON's invention; which, tho' the metalline concave was not duly polished, yet did pretty well, but was undercharged'¹¹⁹.

At the next meeting, an improved telescope was shown. 'It was recommended to Mr. HOOKE, to see it perfected as far as it was capable of being'¹²⁰. However, no report was made on the four-foot telescope for more than a month, while Cock was exhorted to perfect a six-foot Newtonian telescope in February¹²¹. But on 14 March:

'Mr. COCK was ordered to make, for the use of the society, a telescope of Mr. NEWTON's invention, of the length of four or five feet; which he promised to

¹¹⁷ Royal Society Journal Book, 4, p. 223. This remark was not transcribed to *Birch*. For the description below, see Simpson (1981), pp. 230ff.

¹¹⁸ *Newton Corresp.*, 1, p. 4. This is a Collins memorandum annexed to his copy of Newton's letter to a friend on 23 February 1669. Collins enclosed the letter and memorandum in his letter to Gregory dated on 23 February 1672. For details, see Turnbull (1939), pp. 218-223 (especially, pp. 221 & 223).

¹¹⁹ *Birch*, 3, p. 4.

¹²⁰ *Ibid.*, p. 8. The citation clearly proves Hooke's collaboration with Cock. See also Simpson (1981), p. 238.

¹²¹ *Ibid.*, p. 233. At the meeting on 15 February, 'Mr. HOOKE was put in mind of the six foot tube of Mr. NEWTON's invention' (*Birch*, 3, p. 15).

have ready within a fortnight'¹²².

On 18 April Cock showed the Society a concave mirror for the telescope. He said that 'he was not able to make [it] all over of the same hue'¹²³. The mirror was made of steel, differing from Newton's own, which was based on an alloy of copper and tin¹²⁴. Cock presented an improved mirror a month later. Hooke was asked to test it, but 'he said it was falsely polished'¹²⁵.

On 8 July 1672 Newton wrote to Oldenburg that he 'should be glad to heare whether Mr Cock hath finished the four foot Telescope', for he had been informed of Cock's trial in April¹²⁶. Two days after Newton's inquiry, before the summer recess began, Cock was pressed by the Society to finish the four-foot telescope¹²⁷. Within a week, Newton enquired again of Oldenburg whether the telescope was completed. In the same letter, he tried to guess the material of Cock's speculum:

'I suppose it is made by melting Steel with a little Antimony perhaps without separating the sulphureous from the metalline part of that mixture. And so though it may be very hard & capable of a good polish, yet I suspect whether it be so strongly reflective as a mixture of other Metalls'¹²⁸.

Oldenburg replied in three days, that Cock told him concerning the four-foot telescope that:

'the object-speculum (being a compound of copper, tin, tin-glasse, antimony and a little arsenick) is of about 6 inches diameter, wrought upon a tool of about 14 or 15 foot [spherical diameter], and drawing 4 foot [focal length], more or lesse. He adds, that tis a very good mettall, shewing the moon very well, but other objects faint'¹²⁹

¹²² *Ibid.*, p. 19.

¹²³ *Ibid.*, p. 43.

¹²⁴ Newton's specula are discussed in the next section.

¹²⁵ *Birch.*, **3**, p. 49.

¹²⁶ Oldenburg to Newton, 9 April 1672, *Newton Corresp.*, **1**, pp. 135-136 (p. 135); Newton to Oldenburg, 8 July 1672, *ibid.*, pp. 212-213 (p. 212).

¹²⁷ *Birch.*, **3**, p. 57.

¹²⁸ Newton to Oldenburg, 13 July 1672, *Newton Corresp.*, **1**, pp. 217-218 (p. 217).

¹²⁹ Oldenburg to Newton, 16 July 1672, *ibid.*, p. 219. For the focal length of a spherical mirror, see note 100. See also Simpson (1981), p. 233.

Cock also mentioned a steel speculum. He said, Oldenburg wrote, that it was 'a pure Venice-steel, forged with much Care; not melted, nor compounded with any thing; of 3 inches diameter, but bearing not so good a polish'¹³⁰.

Hooke was also engaged in making other reflecting mirrors of about nine feet focal length in 1672¹³¹. On 23 August, he entered in his diary that 'Pollish 9 foot speculum well. Saw Moon at night through it very big and distinct'¹³². At the Society meeting on 30 October, Hooke mentioned another speculum of similar focal length for a Newtonian reflector:

'An account being demanded of what trials had been made for the improvement of the reflecting telescope of Mr. NEWTON, Mr. HOOKE said, that hitherto he had wanted a mould of a sufficient bigness for a speculum, designed by him, of fifteen inches diameter, for a tube of ten feet long; but that he hoped to have, in a week or fortnight, such a mould cast, wherein a speculum of that bigness might be well wrought and polished'¹³³.

Hooke wrote in his diary on 19 November that 'Cox brought home tool for reflection, gave him 1sh'¹³⁴. On the next day, 'Mr. HOOKE intimated' to the meeting of the Society 'that the great tool for grinding the reflecting glass was now ready; and he was exhorted to put it to the trial'¹³⁵. On 27 November, Hooke said 'that he had tried the said tool so far, as to find it pretty just'¹³⁶. At the following meeting, he hoped 'that he might be able to bring in the large reflex speculum at the next meeting'¹³⁷. But Hooke's diary suggests that he experienced difficulty in preparing a good mirror. On 30 December, he recorded that 'Cox here, he told me of metall warping which I found true'¹³⁸. On 22 January of the next year, Hooke reported on their work to the Royal Society:

'Mr HOOKE produced an essay of a reflecting objective speculum, being the segment of a sphere of thirty six feet [nine-foot focal length], which he hoped,

¹³⁰ Oldenburg to Newton, 16 July 1672, *Newton Corresp.*, 1, p. 219.

¹³¹ Simpson (1981), pp. 239ff.

¹³² *Diary 1672-1680*, p. 5.

¹³³ *Birch*, 3, p. 58.

¹³⁴ *Diary 1672-1680*, pp. 13-14.

¹³⁵ *Birch*, 3, p. 62.

¹³⁶ *Ibid.*, p. 63.

¹³⁷ *Ibid.*, p. 69.

¹³⁸ *Diary 1672-1680*, p. 18.

when perfectly polished, would perform as much as a refracting object-glass for an hundred feet tube. He was desired to see it brought to perfection'¹³⁹.

Hooke showed the mirror to the meeting on 5 February. But the diary of the day recorded that 'Carryd Speculum to [the Society meeting at] Arundell house, not good'¹⁴⁰. The work seems to have continued until 20 February when 'Cox had home his tool'¹⁴¹.

New trials for the speculum started in August 1673, now not for Newtonian but for Gregorian telescopes¹⁴². On 5 February the next year, Hooke produced a reflecting telescope of his own design to the meeting of the Royal Society. 'This was performed by a way propounded by MERSENNUS, and repeated in Mr. GREGORY's Optics; but was thought to have been never actually done before'¹⁴³.

One of Hooke's undated manuscripts refers to a Gregorian telescope he made. Simpson argued that it would almost certainly be the one Hooke presented in 1674¹⁴⁴. As Fig 6-6 shows, Hooke mounted a small eyepiece lens in the middle of the object mirror. Hooke wrote that he looked 'directly at the Object, and see it very distinct, and magnified'¹⁴⁵.

While Hooke was trying reflectors, he was also conducting his optical controversy with Newton. It is true that Hooke argued against Newton's telescope, but he had a broad enough mind to pursue his rival's ideas for about a year. Hooke hoped to make a reflector comparable with a one hundred-foot refractor, but we have no record that he was able to achieve it.

Newton knew his reflector was only a prototype. But he believed in its future possibilities:

'for I [Newton] doubt not but in time a Six foote Tube may be made after this Method which will Performe as much as any 60. or 100. foote tube made after the Common way'¹⁴⁶.

However, Hooke's difficulties discussed in this section implied serious technical

¹³⁹ *Birch*, 3, p. 72.

¹⁴⁰ *Diary 1672-1680*, p. 26.

¹⁴¹ *Ibid.*, p. 29.

¹⁴² Simpson (1981), p. 241.

¹⁴³ *Birch*, 3, p. 122.

¹⁴⁴ Simpson (1981), p. 242.

¹⁴⁵ *Philosophical Experiments*, pp. 269-270 (p. 269).

¹⁴⁶ Newton to a Friend, 23 February 1669, *Newton Corresp.*, 1, pp. 3-9 (p.3).

obstacles to realising Newtonian reflectors at a practical level. While Hooke and Cock were working to improve the Newtonian reflector, Collins sent Gregory a pessimistic report on the contrivance:

'Mr Hooke is in a consumption and unlikely to recover, Cox the Glasse grinder thinks that neither his Devices nor this new Telescope will obtaine repute in the World, the mettall suddainly tarnishing'¹⁴⁷.

The Problems of the Newtonian Telescope

One criticism made of Newton's telescope at the Royal Society was that it was 'difficult to find ye Object' with it¹⁴⁸. But this was a minor inconvenience, which could easily be solved by the use of auxiliary sights¹⁴⁹.

There were more serious problems with its speculum. The alloy Newton used was known as bell-metal: six parts of copper and two parts of tin. Alchemists were familiar with it because of its similarity to silver¹⁵⁰. Newton added arsenic to prevent pores forming on the surface. When he sent his second reflector to London, he used a small proportion of silver instead of arsenic¹⁵¹. It is estimated that the speculum made with this alloy reflected only about twenty percent of the incident light when used in a telescope¹⁵². The loss of light was greater than that from lenses¹⁵³. Indeed when Newton's telescope was first brought to London, it was recognised immediately that the image it gave was 'not so lightsome' though 'clere without Colours'¹⁵⁴. To make matters worse, the high proportion of copper resulted in rapid tarnishing of the mirror¹⁵⁵. In hindsight, Newton's reflector was far from being a practical telescope.

Newton obviously knew that the mirror was of crucial importance. He was confident that he could perfect his telescope 'provided a *Reflecting* substance could be found, which would polish as finely as *Glass*, and *reflect* as much light, as glass

¹⁴⁷ Collins to Gregory, 26 December 1672, Turnbull (1939), pp. 248-249 (p. 249).

¹⁴⁸ Oldenburg to Newton, 2 January 1672, *Newton Corresp.*, 1, pp. 73-74 (p. 73).

¹⁴⁹ Newton to Oldenburg, 6 January 1672, *ibid.*, pp. 79-81 (pp. 79-80).

¹⁵⁰ King (1955), p. 74.

¹⁵¹ Newton to Oldenburg, 18 January 1672, *Newton Corresp.*, 1, pp. 82-83 (p. 82);

Newton to Oldenburg, 29 January 1672, *ibid.*, pp. 84-85 (p. 84). Synopses were published in *Phil. Trans.*, 7 (1672), no. 81, pp. 4006-4007.

¹⁵² Simpson (1981), p. 82. See also p. 126 of the next note.

¹⁵³ Newton to Oldenburg, 26 March 1672, *Newton Corresp.*, 1, pp. 123-126 (pp. 124 & 126). An extract of the letter was printed in *Phil. Trans.*, 7 (1672), no. 82 (pp. 4032-4034 (p. 4032)).

¹⁵⁴ Draft letter of Collins to Vernon, 26 December 1671, Royal Society MS/ 81, item 13.

¹⁵⁵ King (1955), p. 74.

transmits'¹⁵⁶. He also recognised the problem of tarnishing, because when he sent his reflector to London he advised Oldenburg that 'if the concave metall contract any dullnesse by moysture or otherwise, it ought to bee taken out an rubbed wth gentle leather'¹⁵⁷.

The Royal Society also recognised the problem with the mirror. We have already seen, for example, that Hooke endeavoured 'to find out a Metall not obnoxious to tarnishing'. At the meeting on 25 January 1672, the Society discussed the possibility of using opaque glass made by Boyle, or fine black stone from Iceland, for reflecting concave mirrors¹⁵⁸.

On 4 March 1672, Oldenburg wrote to René-François de Sluse (1622-1685), a mathematician of Liège, about Newton's reflector 'by which he [Newton] can markedly shorten long telescopes without spoiling their effectiveness'. Oldenburg reported:

'A craftsman is now engaged in making a telescope of this kind six feet long, which we hope will excel an ordinary telescope of fifty feet, if only a metal can be made so compact and solid that it will reflect the ray as it should'¹⁵⁹.

In this mention of Cock's trials, Oldenburg confirmed the importance of obtaining adequate reflection.

Auzout discussed the problems of Newton's speculum too. Though Auzout's letter is lost, from Newton's response in March 1672 it is known that he also raised the problems of poor reflection and tarnishing. Newton answered Auzout via Oldenburg as follows:

'And for the practique part, it is in some measure manifest by the instruments already made to what degree of vivacity & brightness a metalline substance may be polished. Nor is it improbable but that there may be new ways of polishing found out for metall, wch will far excell those that are yet in use. And when a metall is once well polished it will be a long while preserved from tarnishing if diligence be used to keep it dry & close shut up from Air'¹⁶⁰.

¹⁵⁶ Newton to Oldenburg, 6 February 1672, *Newton Corresp.*, 1, pp. 92-107 (p. 95).

¹⁵⁷ *Newton Corresp.*, 1, p. 80.

¹⁵⁸ *Birch*, 3, pp. 4 & 7-8.

¹⁵⁹ Oldenburg to Sluse, 4 March 1672, *Oldenburg Corresp.*, 8, pp. 571-577 (pp. 572 & 575).

¹⁶⁰ *Newton Corresp.*, 1, pp. 126-127.

In the same letter, Newton proposed an improvement for better reflection (Fig. 6-7). Here the secondary flat mirror was replaced by a triangular prism. The total reflection at its longest base deflected the light from the primary mirror through a right angle toward the eyepiece lens. This was exactly the design that Newton later printed in *Opticks* in 1704¹⁶¹.

Newton was always optimistic about the potential of his reflector. However, its realisation was another matter. Hooke endeavoured to make practical reflectors for about a year, but in vain. As Helden wrote, 'Newton's reflector is a clear example of a scientifically conceived telescope, perfect in principle, that could not be realized on a useful scale by contemporary craftsmen'¹⁶². In fact practical reflectors were not achieved until the 1720s, as we see at the end of the present Chapter.

The Exchange between Gregory and Newton

Collins sent an account of Newton's telescope to James Gregory on 23 February 1672¹⁶³. About three weeks later, Collins also reported to him the optical controversy between Hooke and Newton¹⁶⁴. In his answer on 9 April, Gregory wrote that:

'I was exceedingly surprised with these experiments of Mr Newton; they will cause great changes throughout all the body of natural philosophy, by all appearance if the matter of fact be true, which I have no ground to question. I would gladly see what Mr Hooke can say against the doctrine raised upon them'¹⁶⁵.

Gregory first commented on Newton's reflector itself in his letter to Collins on 6 August. He wrote that Newton's telescope 'may have an advantage above that which I mentioned in my *Optica Promota*, because the eyeglass is so near to the plane mirror; yet the obliquity of the mirror hindereth somewhat'¹⁶⁶.

Newton and Gregory began their exchange via Oldenburg when news of the

¹⁶¹ Newton (1704), pp. 79-80. See also the figure attached to it (Book I, Fig. 29).

¹⁶² Helden (1974a), p. 49.

¹⁶³ Collins to Gregory, 23 February 1672, Turnbull (1939), pp. 218-223. A copy of Newton's letter with Collins's memorandum was enclosed with it, as mentioned in note 118 above. The letter was also printed in *Newton Corresp.*, 1, pp. 3-9.

¹⁶⁴ Collins to Gregory, 14 March 1672, *ibid.*, pp. 118-120 (p. 118); Turnbull (1939), pp. 224-226 (p. 224).

¹⁶⁵ Gregory to Collins, 9 April 1672, Turnbull (1939), pp. 226-231 (p. 226).

¹⁶⁶ Gregory to Collins, 6 August 1672, Turnbull (1939), pp. 240-241 (p. 241). A fragment of the letter is printed in *Newton Corresp.*, 1, p. 228.

Cassegrain telescope reached the Royal Society¹⁶⁷. When Newton's reflector was announced in *Journal des Sçavant*, a priority claim for Cassegrain was made in France by one M. Henride Bercé de Chartres. Oldenburg sent the description from Bercé's letter to Newton¹⁶⁸. From the illustration attached to it (Fig. 6-5, right), it is clear that the structure of the Cassegrainian telescope was more akin to the Gregorian than the Newtonian, and Oldenburg asked Newton to 'compare it with what Mr Gregory, the Scot, hath printed'¹⁶⁹. Bercé asserted four advantages of Cassegrainian over Newtonian reflectors: the aperture of the tube could be of any size; the reflections were made in the direction of the axis and were therefore undistorted and clear; better vision because the observer was screened from unnecessary light by being behind the object mirror at CD; and it was easier to aim¹⁷⁰.

Two days later, Newton replied to Oldenburg with critical comment on the Cassegrainian reflector. He emphasised that he put 'ye eye glasse at the side of the Tube rather then at the middle' after analysing the construction of the Gregorian telescope¹⁷¹. Then he listed seven disadvantages of the Cassegrainian. Major points of criticism were: the loss of light with a small convex speculum was greater than that from an oval plane because 'light is most copiously reflected from any substance when incident most obliquely'; the errors of the convex speculum would be exaggerated by the distance between it and the eyeglass; and the errors of the primary concave would 'be more augmented by reflexion from ye convex then from the plane'¹⁷².

Gregory responded to Newton's comment in a letter to Collins on 23 September 1672. After examining the disadvantages Newton had listed, Gregory proposed that the Cassegrainian reflector could be modified by changing the secondary convex mirror to a flat one. This would avoid almost all the disadvantages of Cassegrainian reflector, and it would even have advantages over the Newtonian because:

'the distance EF [see Fig. 6-5, right] groweth almost the one halfe lesse; and therefor the errors of the concave CD ar[e] also diminished upon the plane F by one halfe. ther[e] is yet another advantage of this telescope, yt it will be

¹⁶⁷ For details, see Simpson (1981), pp. 212ff. Almost nothing is known about Cassegrain except for his reflector. See *DSB*, 'Cassegrain', 3, pp. 97-98.

¹⁶⁸ Oldenburg to Newton, 2 May 1672, *Newton Corresp.*, 1, pp. 150-152. Bercé's claim was translated from French to English, and was printed with Newton's answer below it in *Phil. Trans.*, 7 (1672), no. 83, pp. 4056-4059.

¹⁶⁹ *Newton Corresp.*, 1, p. 150.

¹⁷⁰ *Ibid.*, p. 151; *Phil. Trans.*, 7 (1672), no. 83, pp. 4056-4057.

¹⁷¹ *Newton Corresp.*, 1, p. 153.

¹⁷² *Ibid.*

little more then halfe the length of Mr Newtons, & doe the same effect'¹⁷³.

However, Gregory also saw an advantage of the original Cassegrainian design. He wrote that 'this telescope may be worth the trying, seing the eye glasse and speculum F being movable, the speculum CD can have, by their help, anie desirable charge [magnification]'¹⁷⁴.

Towards the end of the letter Gregory referred to the question of direct and oblique reflections:

'I suppose ther is no question that direct rayes have the advantage of oblique, seing a ball thrown directlie on a rough wall hath a more regular reflection then when it is thrown obliquelie'¹⁷⁵.

Newton answered Gregory in his letter to Collins on 10 December 1672. After expressing his appreciation of Gregory's proposal for a modification of the Cassegrainian reflector, Newton argued that the Gregorian was rather superior to Cassegrainian reflectors. He wrote that its advantage was 'the little concave Ellipsis comes nearer to a sphericall figure then the convex Hyperbola'. Newton continued that 'his [Gregory's] present proposall excels them both, of making that speculum plane'¹⁷⁶.

Newton proceeded to his doctrine that light incident on a plane mirror was better reflected when oblique than when direct. This was the theoretical basis for the oblique setting of the secondary mirror in his reflector. Newton explained his reasoning as follows:

'I am convinced by severall observations that reflexion is not made by the solid parts of a body, as is commonly presumed, but by the confine of the two Mediums whereof one is within, & the other without the body. And as stones are reflected by water when thrown obliquely wch force their way into it when thrown directly downwards so the rays of light (whether corporeall like stones or not) are most easily & copiously reflected when incident most obliquely'¹⁷⁷.

¹⁷³ *Ibid.*, pp. 239-240.

¹⁷⁴ *Ibid.*, p. 240.

¹⁷⁵ *Ibid.*

¹⁷⁶ Newton to Collins, 10 December 1672, *Ibid.*, pp. 247-255 (p. 248). The letter is also printed in Turnbull (1939), as are the letters mentioned below.

¹⁷⁷ *Newton Corresp.*, 1, p. 248.

Newton then moved on to discuss the disadvantages of the modified version of the Cassegrainian reflector: its secondary mirror would have to be large if it was flat, and 'that would cause too many of the best rays to be intercepted'; and 'an Artificer can scarcely polish the great concave so truly when perforated in the middle'¹⁷⁸. Newton argued against Gregory's claim that his proposal would suffer less from errors produced by the concave mirror, estimating that 'the errors in both cases will be alike, provided the speculum F be accurately plane'¹⁷⁹. But he admitted the advantage that its 'Tube will be little more then half the length of mine'¹⁸⁰.

Newton also criticised the original Cassegrainian reflector. He referred to its 'supposed advantage' that 'they may have any desirable charge by altering the distances of the eye-glass & specula', but insisted that it 'agrees more conveniently to my design of the instrument'¹⁸¹. And he pointed out that the original Cassegrainian design had the tendency to be overcharged: to avoid this, he proposed the use of an additional eyeglass, which also had the advantage of correcting the inversion of the image¹⁸².

Towards the end of the letter, Newton mentioned Hooke's attempt to make a reflector with the mirror Gregory had left with Reeve. He attributed Hooke's failure to lack of diligence¹⁸³.

Gregory sent his answer to Newton via Collins on 7 March 1673. He agreed he had been mistaken in estimating the error¹⁸⁴, and that he was 'almost convinced that oblique reflection causeth more light than the direct'. Nevertheless, he wrote that 'I am not fully persuaded that it is more regular'¹⁸⁵. Gregory asked Newton to show how he would be able to alter the charge or magnification of his reflector¹⁸⁶.

Newton answered Gregory about a month later, and wrote about the problem of reflection as follows:

'a well polished plate reflects at ye obliquity of 45 degrees more truly then direct ones seems to me very certaine. For ye flat tuberculæ or shallow valleys, such as may be ye remains of scratches almost worne out will cause ye

¹⁷⁸ *Ibid.*, p. 249.

¹⁷⁹ *Ibid.*

¹⁸⁰ *Ibid.*, p. 250.

¹⁸¹ *Ibid.*

¹⁸² *Ibid.*

¹⁸³ *Ibid.*, p. 251.

¹⁸⁴ Gregory to Collins, 7 March 1673, *ibid.*, pp. 258-262 (p. 258).

¹⁸⁵ *Ibid.*

¹⁸⁶ *Ibid.*

least errors in ye oblique rays wch fall on all sides the hill'¹⁸⁷.

Newton also explained how to change the charge of his reflector. A prism with spherically convex surfaces was used for a secondary mirror (Fig. 6-8)¹⁸⁸.

In his response on 13 May 1673, Gregory complained that he could not understand Newton's explanation of reflection. On the method of altering the charge, he wrote:

'his [Newton's] way of varying the charge is indeed exceedinglie ingenious, but I think three surfaces too lyable to the errors of the artificers hand. the opacitie of the glasse prisme together also with the irregularitie, which he hath discovered in refraction, may help to darken and confuse the sight'¹⁸⁹.

From the last sentence, it is clear that Gregory accepted Newton's theory of colour dispersion, and therefore the two natural philosophers did not contend over the differing refrangibilities of rays of light. The major part of their exchange was devoted to the comparison of Newtonian, Gregorian and Cassegrainian reflectors, and of oblique and direct reflection.

Their exchange ended with this letter. Gregory left for London after he had sent it. Turnbull wrote that their correspondence 'may have led to an actual meeting of Gregory and Newton at Cambridge in September 1673' on Gregory's way back to St. Andrews¹⁹⁰.

The Controversy between Huygens and Newton

The Royal Society habitually informed Huygens of important scientific matters. Oldenburg sent a short description of Newton's reflector to Huygens on 1 January 1672 (O.S.)¹⁹¹. The Society decided to send an official description of the invention to him to secure English priority¹⁹². This was composed by Oldenburg and checked by Newton, and enclosed with a letter from Oldenburg to Huygens dated 15

¹⁸⁷ Newton to Collins, 9 April 1673, *ibid.*, pp. 269-272 (p. 269).

¹⁸⁸ *Ibid.*

¹⁸⁹ Gregory to Collins, 13 May 1673, *ibid.*, pp. 278-280 (p. 278).

¹⁹⁰ Turnbull (1939), p. 4; *Newton Corresp.*, 1, p. 277, note 2.

¹⁹¹ Oldenburg to Huygens, 1 January 1672, *Oldenburg Corresp.*, 8, pp. 443-447 (pp. 444 & 445-446).

¹⁹² *Birch*, 3, p. 1; Oldenburg to Newton, 2 January 1672, *Newton Corresp.*, 1, pp. 73-74 (p. 73).

January¹⁹³.

Huygens answered Oldenburg's letters on 3 February (O.S). He wrote:

'I imaged that he [Newton] proposed to shorten ordinary telescopes by the reflection of his mirrors. I now see that his plan was much better and that he has well considered the advantage, which a *Concave speculum* has above *Convex glasses* in collecting the parallel rays'¹⁹⁴.

As an expert user of telescopes, Huygens did not forget to add that the key to the success of the reflector would be 'to find a matter for this speculum that will bear so good and even a polish as Glasses'¹⁹⁵.

When Newton's article on his theory of colour was printed in *Philosophical Transactions*, Oldenburg sent Huygens a copy¹⁹⁶. Huygens first commented on the theory in his answer on 30 March 1672 (O.S.). He wrote:

'I was very glad to find in the last [copies] what Mr. Newton writes about the effect of lenses and mirrors with respect to telescopes, where I see he has noticed as I have the defects in refraction of convex objective lenses, caused by the inclination [inclinaison in original French text] of their two surface. As for his new theory of colours, it seems to me very ingenious, but it must be seen whether it is compatible with all experiments'¹⁹⁷.

Huygens thus suspended judgement on Newton's colour theory. It should be noted that, in French, 'inclinaison' means the tilt or gradient only, while 'inclination' means both gradient and nature. The former was derived from the latter before 1661 to distinguish geometrical meaning¹⁹⁸. Therefore, the letter shows that he believed the advantage of Newton's reflector lay in avoiding the effects of 'inclinaison' or surface gradient of lenses, that is, he thought it avoided spherical aberration. Huygens did not

¹⁹³ *Ibid.*, pp. 74-76; *Oldenburg Corresp.*, **8**, pp. 470-473.

¹⁹⁴ *Ibid.*, pp. 517-522 (pp. 517 & 520).

¹⁹⁵ *Ibid.*, pp. 517-518 & 520.

¹⁹⁶ Oldenburg to Huygens, 11 March 1672, *ibid.*, pp. 584-586 (pp. 584-585).

¹⁹⁷ Huygens to Oldenburg, 30 March 1672, *ibid.*, pp. 635-638 (pp. 635 & 637). Newton received an edited short extract from the letter in French. See Oldenburg to Newton, 9 April 1672, *Newton Corresp.*, **1**, pp. 135-136 (p. 135).

¹⁹⁸ See Alan Rey, *Dictionnaire historique de la langue française* (Paris, 1992, repr. 2004), p. 1806; Oscar Bloch et Walther von Wartburg, *Dictionnaire etymologique de la langue française* (Paris, 2004), p. 335. For its current use, see for example, *Larousse Concise Dictionary French=English* (Paris, 2004), p. 271.

yet comprehend the implications of Newton's theory of colours, which was originated from his consideration of the nature of lenses.

Huygens and Newton began a substantial exchange when Huygens commented further on Newton's reflector in his letter to Oldenburg on 21 June 1672. Huygens wrote that 'touching Mr. Newton's telescopes, he ought, it seems to me, to try to perfect them himself and to make them bigger in size than those of seven or eight inches'¹⁹⁹. In effect, Huygens emphasised the importance of developing the telescope into a practical instrument.

After pointing out the technical difficulty of finding a suitable metal for the speculum, Huygens referred to Newton's 'new hypothesis of colours', and said it is 'very probable'²⁰⁰.

'But I do not agree with what he says about the aberration of the rays through convex lenses. For I found in reading what he wrote that this aberration according to his principle ought to be twice what he makes it, that is to say one twenty-fifth of the aperture of the lens, which however seems contrary to experiment. So that perhaps this aberration is not always proportional to the angles of inclination of the rays'²⁰¹.

When Oldenburg sent Newton a summary of the letter, he implicitly encouraged Newton to write back himself to Huygens. Oldenburg suggested that Newton might write in English, because Huygens was 'a great master of the English tongue'²⁰². Within a week, Newton did write in English to Huygens, including an illustration explaining how to calculate chromatic aberration²⁰³.

In his answer on 17 September, Huygens accepted that his calculation of the aberration had been wrong. But he remained unconvinced about Newton's theory of colours, and wrote that:

'the thing could very well be otherwise, and it seems to me that he ought to content himself if what he has advanced is accepted as a very likely hypothesis. The more so since even if it were true that the rays of light were, by their

¹⁹⁹ Huygens to Oldenburg, 21 June 1672, *Oldenburg Corresp.*, 9, pp. 116-120 (pp. 117-118).

²⁰⁰ *Ibid.*, pp. 117 & 119.

²⁰¹ *Ibid.*

²⁰² Oldenburg to Newton, 2 July 1672, *Newton Corresp.*, 1, pp. 206-208.

²⁰³ Newton to Oldenburg, 8 July 1672, *ibid.*, pp. 212-213; Oldenburg to Huygens, 18 July 1672, *Oldenburg Corresp.*, 9, pp. 165-167.

origin, some red, others blue, etc. there would still remain the great difficulty of explaining by the mechanical philosophy what this diversity of colours consist of'²⁰⁴.

Huygens was concerned about the lack of any mechanical model to explain the intrinsic colours of rays.

Huygens added more comment on Newton's theory of colour in his letter of 4 January 1673. He was interested by Newton's counterargument in *Philosophical Transactions* against Hooke's critical report. An extract of Huygens's letter was also printed in *Philosophical Transactions*. It reads:

'I have seen, how Mr. Newton endeavours to maintain his new Theory concerning *Colours*. Me thinks, that the most important Objection, which is made against him by way of Quare is that, Whether there be more than two sorts of Colours. For my part, I believe, that an *Hypothesis*, that should explain mechanically and by the nature of motion the Colors *Jellow* [*sic*] and *Blew*, would be sufficient for all the rest, in regard that those others, being only more deeply charged (as appears by the Prismes of Mr. Hook) do produce the dark or deep-Red and Blew; and that of these four all the other colors may be compounded'²⁰⁵.

From this, several points are clear. First, Huygens believed that the light was a mechanical motion (or wave). Secondly, he maintained that all colours were compounded from two primary colours, yellow and blue. Although Hooke supposed that the primary colours were red and blue, their theories showed a conspicuous similarity. A third point is that Huygens accepted the differing refrangibility of light. However, like Hooke, he believed this was not an innate property, but that light bent (charged) to two extreme directions acquired the colours red and blue. Huygens also wrote that 'As for the composition of *White* made by all the Colors together, it may possible be, that *Yellow* and *Blew* might also be sufficient for that'²⁰⁶.

At the end of the letter, Huygens again confirmed Newton's calculation on

²⁰⁴ Huygens to Oldenburg, 17 September 1672, *ibid.*, pp. 247-251 (pp. 247-248 & 249); Oldenburg to Newton, 24 September 1672, *Newton Corresp.*, 1, pp. 242-244 (p. 243).

²⁰⁵ *Phil. Trans.*, 8 (1673), no. 96, pp. 6086-6092 (p. 6086); Huygens to Oldenburg, 4 January 1673, *Oldenburg Corresp.*, 9, pp. 380-384 (pp. 380 & 383); Oldenburg to Newton, 18 January 1673, *Newton Corresp.*, 1, pp. 255-257 (p. 255).

²⁰⁶ *Phil. Trans.*, 8 (1673), no. 96, p. 6086.

chromatic aberration. But he argued that when he experimented with a twelve-foot object glass the effect was weaker than expected, and so Huygens presumed that ‘the difference of the Refrangibility doth not, it may be, always follow the same proportion in the great and small inclinations of the Rayes upon the surface of the Glass’²⁰⁷.

Newton sent his answer on 3 April 1673²⁰⁸, and it was printed in *Philosophical Transactions*. He argued against Huygens’s theory of two primary colours, and wrote that ‘all Colors cannot practically be derived out of the *Yellow* and *Blew*’²⁰⁹. Even if the light were a motion in the ether, it would be puzzling that only two types of motions existed in space. Newton then said that he was little interested in ‘hypothesis’, a word he used only advisedly²¹⁰. He admitted that he had mentioned an experiment, ‘by which I have produced *White* out of two colors alone’, but ‘the two colors were compounded of all others, and so the resulting *White*...was compounded of them all’²¹¹. Although the explanation was true, it must have been confusing for those to whom Newton’s theory seemed alien.

In the last part of his letter, Newton explained why the effect of chromatic aberration seemed smaller than expected. He had estimated the error at about one fiftieth of the diameter of the glass. But actually:

‘the rays, whose error is so great, are but very few in comparison to those wch are refracted more justly. For the rays wch fall upon the middle parts of the glass are refracted wth sufficient exactness, as also are those that fall neare the perimeter & have a mean degree of refrangibility’²¹².

In addition, the most refracted rays were much weakened by being scattered over a wider space.

Huygens sent a short answer two months later²¹³. Again an extract was printed in *Philosophical Transactions*²¹⁴. Apparently he was not persuaded by Newton’s answer on the composition of white light, and wrote ‘seeing that he [Newton]

²⁰⁷ *Ibid.*, p. 6087.

²⁰⁸ Newton to Oldenburg, 3 April 1673, *Newton Corresp.*, 1, pp. 264-267.

²⁰⁹ *Phil. Trans.*, 8, (1673), no. 97, pp. 6108-6111 (p. 6108).

²¹⁰ Bechler (1974).

²¹¹ *Phil. Trans.*, 8, (1673), no. 97, p. 6109.

²¹² *Ibid.*, pp. 6111-6112.

²¹³ Huygens to Oldenburg, 31 May 1673, *Oldenburg Corresp.*, 9, pp. 674-676; Oldenburg to Newton, 7 June 1673, *Newton Corresp.*, 1, pp. 285-287.

²¹⁴ *Phil. Trans.*, 8, (1673), no. 97, p. 6112.

maintains his opinion with so much concern, I list not to dispute'²¹⁵. Huygens accepted Newton's estimation of chromatic aberration. He continued:

'but then he is also to acknowledge, that this aberration of the rays is not so disadvantageous to Optic glasses as he seems to have been willing to make us believe, when he proposed *Concave speculums* as the only hopes of perfecting Telescopes. His invention certainly was very good; but, as far as I could perceive by experience, the defect of the Matter renders it as impossible to execute, as the difficulty of the Form obstructs the use of the *Hyperbola* of M. Des-Cartes: So that, in my opinion, we must stick to our Spheric Glasses, whom we are already so much obliged to, and that are yet capable of greater perfection, as well by increasing the length of Telescopes, as by correcting the nature of Glass it self'²¹⁶.

Thus Huygens stressed the importance of attainable practical telescopes over any theoretical superiority. Moreover, as he argued, practical long telescopes with spherical lenses had been made and had produced excellent astronomical observations, and there was still room to improve them further.

In another fortnight, Huygens wrote to Oldenburg that he 'did not wish for further dispute' with Newton²¹⁷. Unaware of this letter, Newton wrote an answer to Huygens via Oldenburg on 23 June²¹⁸. He explained in detail the difference between homogeneous and heterogeneous light, and insisted that large reflectors could be made. Of course Huygens never responded to the letter.

The Fate of the Newtonian Reflector

In addition to the controversies already discussed, Newton also contended with Ignace Gaston Pardies (1636-1673), Linus (1595-1675), and others on his new theory about light and colours. Although preceding research has given considerable weight to Newton's controversies with them, their points of criticism were trivial: Pardies did not understand that Newton set the prism in his experiments of colour dispersion in such a manner that incident and refracted light would make equal angles

²¹⁵ *Ibid.*

²¹⁶ *Ibid.*

²¹⁷ Huygens to Oldenburg, 14 June 1673, *Oldenburg Corresp.*, 10, pp. 28-33 (pp. 29 & 31).

²¹⁸ Newton to Oldenburg, 23 June 1673, *Newton Corresp.*, 1, pp. 290-297; Oldenburg to Huygens, 27 June 1673, *Oldenburg Corresp.*, 10, pp. 58-69 (pp. 62-66). A synopsis of the letter was printed in *Phil. Trans.*, 8 (1673), no. 96, pp. 6087-6092.

to it, and Linus mistakenly thought that the spectrum of the sun was parallel to the length of the prism²¹⁹. Much more attention should have been paid to Newton's exchanges with Huygens, Gregory, Hooke, and so on, who knew telescopes and optics very well²²⁰.

Table 5 compares the opinions of these eminent natural philosophers on Newton's telescope and optics:

Table 5 Responses to Newtonian Telescope and Optics

	Hooke	Huygens	Gregory
Different Refrangibility	Positive	Positive	Positive
Model of Light	Negative	Negative	No Argument
Newtonian Telescope	Negative	Negative	Comparison with Other Reflectors

All accepted Newton's claim of the different refrangibility of rays of light. But Hooke and Huygens did not agree with Newton's corpuscular model of light, nor with Newton's estimation that the different refrangibility was so serious a barrier as to prevent the improvement of refractors. They believed it was much more difficult to scale up Newton's reflectors to a practical size for astronomical observations.

Even after their controversies with Newton, neither Hooke nor Huygens changed their belief in the wave (pulse) theory of light, nor did they lose their faith in refractors. For example, Hooke's draft of his lecture in May 1681 reads as follows:

'Light then is nothing else but a peculiar Motion of the parts of the Luminous Body, which does affect a fluid Body that incompass the Luminous Body, which is perfectly fluid, and perfectly Dense, so as not to admit of any farther Condensation; but that the Parts next the Luminous Body being moved, the whole Expansum of that fluid is moved likewise...The propagation of this Motion into, and through the incompassing Medium is that we call Light in

²¹⁹ Westfall (1980), pp. 240-243, 248-250, 267-268 & 274-276; Westfall (1966b).

²²⁰ In Chap. 2 of *Isaac Newton's Papers and Letter on Natural Philosophy* (Cambridge, Mass., 2nd ed., 1978), dealing with 'Newton's papers on the improvement of the telescope and on physical optics', Cohen published four letters by Pardies and Linus, but only two of Huygens's and none of Gregory's.

the Medium, or the space between the Inlightning and Inlightned Body'²²¹.

Hooke did continue to use refractors, as when he tried to detect the parallax of a fixed star; this was related in his *Attempt* published in 1674, two years after the controversy against Newton. Two years after that, his *Helioscopes* discussed the use of reflection to contract refractors.

Huygens's consistent adherence to the wave theory of light is clear from his well-known work, *Treité de la lumière*, which was published in 1690 at Leiden. It is worth mentioning that Huygens wrote in it that:

'there exists in refraction itself a property which hinders the perfect concurrence of the rays, as Mr. Newton has very well proved by the experiment'²²².

Huygens knew that the wave theory of light did not exclude the different refrangibility of rays.

Huygens's devotion to the improvement of refractors is seen in his book project of 1673, 'Projet du contenu de la dioptrique'²²³. The work was to consist of eight chapters, of which Chapters 4-6 were allocated to refraction with lenses. More than a decade later, Huygens detailed an aerial telescope in *Astroscopia compendiaria*. The following year, 1685, William Molyneux visited Huygens, and 'had the favour of being shown the whole Contrivance by the Excellent Author himself in his Garden at Hague'²²⁴. These confirm Huygens's continued commitment to the improvement of refractors.

Among the members of the Royal Society, several activities in connection with long refracting telescopes were recorded even after 1680. For example, when Wren described his plan for an observatory at the Tom Tower, Christ Church, in 1681, he included among the necessary instruments 'a pole to rayse large Telescopes & manage them'²²⁵.

Wren also proposed to the Council of the Society on 16 February 1703 the use

²²¹ *Posthumous*, pp. 113 & 117.

²²² *Oeuvres*, **19**, pp. 451-537 (p. 524); English Translation by Silvanus P. Thompson, *Treatise on Light* (New York, Dover, 1962), p. 106.

²²³ *Oeuvres*, **13**, pp. 738-745.

²²⁴ Molyneux (1692), p. 224; Smith (1738), Book 3, pp. 281-301; *Oeuvres*, **8**, p. 528.

²²⁵ Bennett (1982), p. 42; Wren to Bishop Fell, 3 December 1681, Carøe (1923), pp. 31-32 (p. 31). Wren also included a quadrant with telescopic sights (*ibid.*).

of the massive structure of St. Paul's to set up a telescope with a 123-foot lens²²⁶. The lens had been made by Constantijn Huygens in June 1686, and was donated to the Royal Society in 1692²²⁷. It is known that some of the fellows rented the lens for their own use. For example, Derham borrowed it in February 1713, and in 1717 it was borrowed by John Pound for the aerial telescope at Wanstead. His collaborator, James Bradley, returned it to the Society in June 1728²²⁸.

Endeavours to grind long focal-length lenses were continued in England in the 1690s. For example, 'Halley proposed an Engine for grinding long glasses for Telescopes' on 24 February 1692 at the Society meeting²²⁹.

Astronomers continued to use long telescopes throughout the seventeenth century and into the eighteenth²³⁰. Newton's telescope 'had almost faded from view by the beginning of the 18th century, but it was not forgotten'²³¹. Although Newton defended the theoretical advantage of his reflector on the basis of his new theory of colours,

'The Royal Society's initial response to the telescope's merits had not such philosophical base...in seeking Royal support for the invention the Society saw a practical or strategic potential, somewhat akin perhaps to that developing for Morland's speaking trumpet. But to realise this potential it had to be demonstrated that the principles and techniques could be extended to larger instruments; only then could it be claimed that the invention was a success'²³².

If we adhere to the traditional historiography, which restricts its view solely to the development of the theory of optics, the Newtonian reflector should be assigned great importance. That historiography is perfectly valid within the framework of the history of optics. However, on a broader view we have to remember that *the telescope is a tool for telescopic observations*. Curiously, on the same day that Newton's reflector

²²⁶ Bennett (1982), p. 42; Royal Society Council Minutes, **2** (copy), p. 169.

²²⁷ *Oeuvres*, **15**, pp. 23-25; Constantijn á frère Christiaan Huygens, 18 janv. 1662, *ibid.*, **10**, pp. 231-233 (p. 231); Helden (174a), p. 46, note. 44. As Helden argues here, some historians mistakenly wrote that the lens was made by Christiaan Huygens in 1656.

²²⁸ Simpson (1981), pp. 175, 196 & 292.

²²⁹ Royal Society Journal Book, **8**, p. 99.

²³⁰ King (1955), p. 65.

²³¹ Simpson (1981), p. 287. Evidence that Newton's reflector was not completely forgotten is seen in Harris (1704), p. 773 (Telescope, Reflecting). But it also referred to the great inconvenience of the reflector, the 'Object *Speculum* being subject to Tarnish, so that it will not continue long good'.

²³² Simpson (1981), p. 114.

was first reported to the Royal Society, Oldenburg produced a letter from Cassini on his observation of a new satellite of Saturn²³³. If we consider the Newtonian reflector in this perspective, a different historiography appears.

When Newton's reflector was introduced to the Royal Society, it was regarded as a contrivance to construct long telescopes. The magnifying power of the reflector which Newton sent to the Royal Society was comparable to that of a five- or six-foot refractor. It was far less than that of the sixty-foot refractor Hooke often used, not to mention Hevelius's 150-foot refractor in his *Machina coelestis, pars prior* of 1673. Both Hooke and Huygens saw that it was essential to scale up Newton's reflector to a practical level if it were to be of any real use. Hooke himself tried to do so, but in vain. The problem was its speculum, which did not reflect enough light and which tended to tarnish. In November 1679, Newton wrote that 'Mr Cock has cast two pieces of Metal for me in order to a further attempt about ye reflecting Tube'²³⁴. This suggests that Newton continued to strive to improve his reflectors, but his progress was not reported. Therefore it was inevitable that 'Newton's telescope had almost faded from view by the beginning of the 18th century'.

A study by William Albury suggests even the possibility 'that in 1690 the members of the Royal Society were generally indifferent to the optical theories of Newton'²³⁵. Albury showed that in 1690 Halley, who had greatly assisted with the publication of Newton's *Principia*, did not mention Newton's theory of colours when Huygens's *Traité de la lumière* was discussed at the Society. Halley defended Huygens's theory of colour throughout the early 1690s, while Hooke criticised it. This could imply that Halley was not acquainted with Newton's theory of colours.

Against this background, Hooke's reaction to Newton's reflector was not at all extraordinary for an experienced user of refracting telescopes. Although Hooke criticised Newton harshly, he did not hesitate to accept Newton's discovery of the different refrangibility of colours. What Hooke could not agree with was Newton's hidden corpuscular model of light, and his unfounded hope of making practical reflectors. Thus Hooke's criticism of Newton was entirely rational in its true historical context.

Newton, who wrote that 'an objecter hath said nothing that can enervate any

²³³ *Birch*, 3, p. 3.

²³⁴ Newton to Hooke, 28 November 1679, *Newton Corresp.*, 2, pp. 300-304 (p. 303).

²³⁵ Albury (1971), p. 447. Halley's proposal, previously referred to, for a lens-grinding engine confirms his ignorance of Newton's theory of colours. Cantor (1978) warned against the orthodox historiography that Newton's optics dominated even in the eighteenth century in England.

part of it [his theory]', was insensitive to the role of telescopes in contemporary astronomy. Indeed, Newton's *Opticks* of 1704, a theoretical work on the study of light, did not describe any telescopic observations, but nevertheless devoted its Book I, Part I, Proposition VII, Problem II to 'To shorten Telescope'²³⁶. Furthermore Newton is not, of course, known for having made any substantial contribution to astronomical observations.

The Revival of the Newtonian Reflector

Before closing this Chapter, let us take a brief look at the process of the revival of Newtonian reflectors, and their first realisation for practical astronomical observations²³⁷.

The reflector in general 'was of no importance in the seventeenth century'²³⁸, and 'was as yet little more than an interesting toy'²³⁹. The Newtonian gained an advantage over other reflectors by being the first for which a workable prototype was achieved²⁴⁰.

Change began in about 1720, when John Hadley (1682-1744), elected Fellow of the Royal Society in 1717, constructed a Newtonian reflector of a useful size²⁴¹. He used a metal speculum 6 inches in diameter and of about 5 feet 3 inches focal length. It magnified the object almost 200 times²⁴². The telescope was shown to the Royal Society meeting on 12 January 1721, and sent to Greenwich Observatory for Halley's use. He was impressed by its performance, and wrote that 'he believed it might be found to excel "even the great telescope at Wanstead"', to which Pound had applied Huygens's 123-foot object lens²⁴³. So the reflector seems finally to have progressed to a stage where it could rival a leading refractor.

Commercial success with reflectors was achieved by James Short (1710-1768). He was the son of an artisan of Edinburgh. Short became a student and protégé of Collin Maclaurin (1698-1746), a Newtonian mathematician at Edinburgh University²⁴⁴. He visited London in 1736, and his telescopes greatly impressed the Fellows of the Royal Society. The following year he was elected a Fellow. He moved to London, and

²³⁶ In the widely-used fourth edition, Proposition VIII, Problem II.

²³⁷ The following description relies mainly on Simpson (1981), Chap. 4.

²³⁸ Helden (1974a), p. 49.

²³⁹ King (1955), p. 65.

²⁴⁰ Simpson (1981), p. 64.

²⁴¹ *Ibid.*, p. 288.

²⁴² *Ibid.*, pp. 289-291.

²⁴³ Simpson (1981), pp. 291-292; King (1955), pp. 77-78.

²⁴⁴ Simpson (1981), p. 314. For Short, see also *DSB*, 'Short', **12**, pp. 413-414.

established his workshop in Surrey Street, off the Strand, in 1738²⁴⁵. Short first worked with a back-silvered glass mirror, but moved on to a metal speculum of copper and tin²⁴⁶. He supplied his reflectors to major observatories in Europe, and when he died he left a fortune of £20,000²⁴⁷. His success was due to his ability to make parabolic mirrors²⁴⁸. By 1740, 'the reflecting telescope can be said to have "come of age"²⁴⁹.

However, the majority of the telescopes Short made were Gregorian reflectors. This was because with the Gregorian 'a secondary concave mirror could be made compared with a convex' comparatively easily, and 'because it adapted itself to terrestrial observations' because of its uninverted image²⁵⁰. Hadley, who made the first practical Newtonian, also turned to the Gregorian after making only two Newtonian reflectors²⁵¹. In 1736, when the Royal Society arranged for a series of comparisons between reflectors, several major makers submitted Gregorian and Cassegrainian examples²⁵². As Jesse Ramsden (1735-1800) showed in 1779, the Cassegrainian combination of a concave and convex mirror tended to correct the aberrations of each surface, while they were compounded in the Gregorian. In addition, the Cassegrainian reflector was shorter than a Gregorian of equal power²⁵³.

Of course, the Newtonian reflector was not neglected, as Short made one for Greenwich Observatory²⁵⁴. But the Newtonian was only one of the three reflector types used in practice. These reflectors in turn co-existed, and still co-exist, with refractors. In June 1758, John Dollond (1706-1761) read to the Royal Society a paper on his invention of the achromatic lens, and announced that the problem which Newton had raised was solved²⁵⁵. Refractors are still used for positional astronomy, because they give stable images of objective stars.

²⁴⁵ Simpson (1981), pp. 318-319.

²⁴⁶ *Ibid.*, pp. 316 & 321.

²⁴⁷ *Ibid.*, pp. 314 & 320.

²⁴⁸ *Ibid.*, p. 321.

²⁴⁹ *Ibid.*, p. 323.

²⁵⁰ King (1955), p. 85.

²⁵¹ Simpson (1981), pp. 293-294.

²⁵² *Ibid.*, p. 311.

²⁵³ King (1955), p. 75.

²⁵⁴ *Ibid.*, p. 86.

²⁵⁵ *Ibid.*, pp. 148ff. See also *DSB*, 'Dollond', 4, pp. 148-149. It is an irony of the history of science that the same Dollond invented a divided object-glass micrometer, applicable to reflecting telescopes. See, *ibid.*, pp. 150-152.