9. Wollaston: the first confirmation of Huygens' construction?

Almost to the end of the eighteenth century the measurement of indices of refraction involved one of four techniques. I have repeatedly mentioned one of them, that of HUYGENS, in which images of lines and points are directly observed with the naked eye. That, however, was not the way in which indices of refraction were usually determined. A second common technique used focal measurements; a third (by means of a microscope) measured changes in the visual depths of objects viewed through the substance[39]. A fourth technique – quite an old one, invented by NEWTON – employed observations of a ray

[39] See DAVID BREWSTER, *A Treatise on New Philosophical Instruments for Various Purposes in the Arts and Sciences. With Experiments on Light and Colours* (Edinburgh and London, 1813).

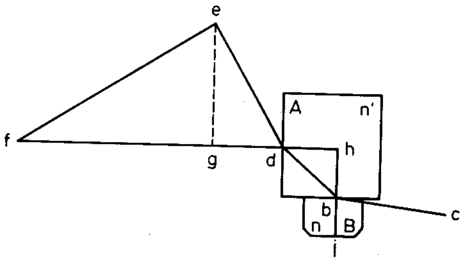

Fig. 19

refracted at minimum deviation through a prism[40]. All these differing techniques require image distances to be measured as exactly as possible in their several ways. In 1802 WILLIAM HYDE WOLLASTON described a device, recently invented by him, that eliminated measurements of image distances from index determination[41].

It is a well-known consequence of SNEL's law that, for any pair of contingent media, the maximum refraction occurs when the ray incident from the more refractive medium coincides with the surface of separation at its incidence (or, in realistic terms, scrapes it); the sine of the angle of refraction is then equal to the reciprocal of the index. That maximum angle is *vice versa* evidently the angle of internal incidence beyond which a ray is not refracted past the interface, but reflected back into the more refractive medium, that is, it is the angle of total internal reflection. Where, specifically, a prism has (relative to air) an index of n', and its external medium has a lesser index of n, then the sine of the angle of total internal reflection for the prism is equal to n/n'. If, therefore, one has a prism of known index it is possible to determine the index of the external medium by measuring the angle of total reflection. That is not in itself greatly useful if this is done by the traditional method of measuring image distances, for the accuracy of the resulting index will not be much greater than that obtainable by other methods. The brilliance of WOLLASTON's technique was that it eliminated the inaccuracies ensuing in all such direct methods, it using the eye only to judge the presence or absence of light.

In WOLLASTON's diagram (figure 19), A is a square prism of known index n'; B is a substance of unknown, smaller index n. If a ray cb enters B at nearly grazing incidence to the plane of separation between A and B, then it will be refracted at $<hbd$, the maximum of refraction, which is the angle of total internal reflection for a ray incident in A upon B. Consequently $\sin \widehat{hbd}$ is equal to n/n', and $<cbi$ is very nearly 90°.

Construct the lines ef, ed, meeting in e, where d is the point of emergence from the prism of the ray bd. Set ef/ed equal to n'. From the figure, bd/dh is

[40] The method of minimum deviation is especially simple and quite precise if one can accurately determine the angle of the prism: its precision results from the fact that, at minimum deviation, the refraction is insensitive to slight rotations of the prism.

[41] WOLLASTON, 'A method of examining refractive and dispersive powers by prismatic reflection', *Phil. Trans.*, **92** (1802), pp. 365–80.

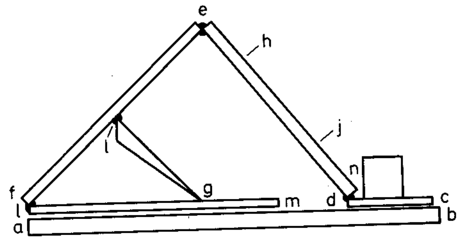

Fig. 20

equal to $1/\sin\widehat{hbd}$, where $\sin\widehat{hbd}$ is n/n'. Again from the figure, $\sin\widehat{efg}/\sin\widehat{edg}$ is equal to ed/ef. Moreover, since ed emerges from the prism, $\sin\widehat{edg}/\sin\widehat{bdh}$ is equal to n':

(9.1) $$bd/dh = 1/\sin\widehat{hbd} = n'/n,$$

(9.2) $$ef/ed = n' = \sin\widehat{edg}/\sin\widehat{hdb},$$

(9.3) $$\sin\widehat{efg}/\sin\widehat{edg} = ed/ef.$$

From (9.2) and (9.3) we find $<efg$ equal to $<bdh$. Hence, from the figure, ef/gf is equal to bd/dh, which is WOLLASTON's result. Consequently, from (9.1) we have that ef/gf is equal to n'/n, *i.e.* it gives the ratio of the refractive indices as long as the device is so constructed that ef/ed is the index of the prism, and the ray emerging along ed is at the limit of internal reflection.

To measure the index of B, then, WOLLASTON needed an apparatus in which ef, ed remained the same, while $<fen$ could be varied until a ray seen along ed marked the limit of total reflection. He constructed the device of figure 20: ab, dc, en, ef, lm are separate wooden boards which are hinged to one another at f, e and d; lg is a wedge of length $\frac{1}{2}ef$ that is hinged to ef at its mid-point. The device is designed to measure (figure 19) the distance fg from f to the point perpendicularly below e. A simple geometrical calculation shows that, in WOLLASTON's device, the moveable point g indeed remains vertically below e whatever the value of $<fen$. WOLLASTON cut ef to a length of 15.83 inches, and he cut ed 10 inches long, so that the index of the prism was 1.583 (hence made of flint glass). WOLLASTON evidently marked a scale, calibrated in hundredths of an inch, along board lm such that point g of the wedge pointed to the corresponding value of $n = gf/ed$ for a given $<efg$.

Now among the transparent solids which WOLLASTON examined was Iceland spar. Here he obtained a confusing series of 'indices' which depended upon the orientation of the spar relative to the plane of incidence, and he evidently asked THOMAS YOUNG for an explanation[42]. YOUNG referred him to HUYGENS' *Traité*. WOLLASTON, aware of YOUNG's recent revival of wave theory, reacted favorably to HUYGENS' account of double refraction:

[42] According to YOUNG, that is. See THOMAS YOUNG, 'Review of Laplace's Memoir ...', *Quarterly Review*, **2** (1809), p. 339.

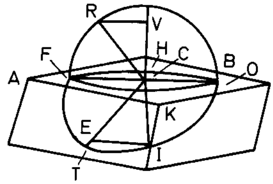

Fig. 21

The optical properties of Iceland spar have been so amply described by HUYGENS, in his *Traité de la Lumière*, that it could answer little purpose to attempt to make any addition to those which he has enumerated. But, as the law to which he has reduced the oblique refraction occasioned by it, could not be verified by former methods of measurement, without considerable difficulty, it may be worth while to offer a new and easy proof of the justness of his conclusion. For, since the theory by which he was guided in his inquiries, affords (as has lately been shown by Dr. Young) a simple explanation of several phenomena not yet accounted for by any other hypothesis, it must be admitted that it is entitled to a higher degree of consideration than it has in general received[43].

While reading the *Traité* WOLLASTON kept his new device constantly in mind, and this led him to peruse the *Traité* with some care. As we shall see, the constant presence of his device probably also colored WOLLASTON's perception of HUYGENS' deductions. He read deeply enough to discover the law of proportions, and to see that it could be used in conjunction with his device to test HUYGENS' construction. In WOLLASTON's diagram (figure 21), the plane of incidence FRO intersects the HUYGENS spheroid in a diameter FO which lies on the surface of the crystal. FTO is the section of the spheroid by the plane of refraction. This plane contains CT, the conjugate to FO, as well as FO itself. The refraction CI of an incident ray RC is determined by the law of proportions: $VR/EI = N/FC$.

WOLLASTON reasoned that his device was perfectly adapted to testing this law. When set to measure the angle at which the top of the crystal just became visible, the device was detecting a ray which had, in effect, emerged from the crystal at nearly perpendicular internal incidence. Consequently $FC = EI$, $VR/EI = VR/RC = \sin i$, where i is the angle of incidence. Hence $\sin i = N/FC$. Since $gf/ef = \sin i$ pointed to $gf/10 = \sin i/10$, WOLLASTON had measured the value of $(ef)N/10(FC)$. He took the inverse as the relative value of FC (relative, that is, to $(ef)N/10$).

Let us now consider WOLLASTON's experiments, of which seven in particular are of present interest:

exp. 1: The plane of incidence was normal to the principal section; $gf/10$ was 1.488, whose inverse is 0.67204.

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43 WOLLASTON, 'On the oblique refraction of Iceland crystal', *Phil. Trans.*, **92** (1802), pp. 381–6.

exp. 2: The plane of incidence was inclined at 50°57′30″ to the principal section; $gf/10$ was 1.518, whose inverse is 0.65876.

exp. 3: The plane of incidence was inclined at 39°2′30″ to the principal section; $gf/10$ was 1.537, whose inverse is 0.6506.

exp. 4: The plane of incidence was the principal section; $gf/10$ was 1.571, whose inverse is 0.6365.

exp. 5: One of the two obtuse solid angles was cut off, in such a fashion that the plane of separation formed equal angles with each edge of the crystal; $gf/10$ was 1.488 in all planes of incidence.

exp. 6: The crystal was cut by a plane that bisected an obtuse facet angle. With this as the plane of separation, when the plane of incidence coincided in direction with the plane of separation of exp. 5, then $gf/10$ was again 1.488; in other planes $gf/10$ increased to the extent that examination became impossible.

exp. 7: The ordinary index was too large to measure with flint glass used as the prism, but, using minimum deviation, WOLLASTON obtained 1.657, whose inverse is 0.6035.

As had HUYGENS, WOLLASTON measured the interfacial angle of the crystal, obtaining 105°5′. From this and spherical trigonometry he found that the inclination of the optic axis to the facet is 45°23′25″. To determine the normal deviation, WOLLASTON recurred to HUYGENS' technique of observation, and, using a crystal 1.145 inches in height, he found 6°16′.

WOLLASTON used exp. 1 to determine the semi-major axis of the spheroid, since, in this plane of incidence, FC was equal to CP; the experiment was therefore not used as a confirmation. In exp. 5, the plane of separation sectioned a circle whose radius was the semi-major axis of the spheroid, so that WOLLASTON should have measured a constant equal to the result of exp. 1, as he did. In exp. 6, the plane of separation contained the optic axis, so that, if the plane of incidence was in the same direction as the plane of separation of exp. 5, WOLLASTON should, as he did, have again measured the semi-major axis. These two experiments (5 and 6), however, at best confirmed that the refraction was peculiarly dependent upon the orientation of the plane of incidence with respect to the optic axis, and the orientation of the axis with respect to the plane of separation. It is not at all impossible to conceive of a generalised SNEL's law that could produce these results. (In fact, *these* results are not at all surprising, since they merely imply a constant index of refraction for the given planes; the problem is to explain experiments which yield different indices.) Moreover HUYGENS had already performed essentially the same experiments, and few, if anyone, had been convinced by them. We are now left with experiments two through four, and the measurement of the normal deviation, as possible confirmations of the theory.

According to WOLLASTON the fourth experiment, which measured the facet semi-axis, CG, was a confirmation, because, he claimed, a calculation of CG agreed with its measured values. This was an interesting claim because WOLLASTON used only *one* measurement of an extraordinary refraction to establish the parameters of the spheroid, and that one was insufficient to do so. He used experiment one, which provided the semi-major axis, CP. This was insufficient to generate the spheroid because it left the semi-minor axis indeterminate. Though HUYGENS had relied on a single measurement of an extraordinary refraction to determine the relative parameters of the spheroid, he had chosen the normal deviation with an arbitrary reference of 100000 for the normally deviated radius of the spheroid, CM. That, in conjunction with the inclination of the optic axis, was sufficient to determine the semi-axes of the spheroid in proportion to CM because CM is conjugate to the radius in the plane of the facet. HUYGENS had then performed a second experiment to obtain the value of N in proportion to CM, thereby fully determining all of the constants in his construction. WOLLASTON, unlike HUYGENS, did not have to measure N because his device immediately gave the spheroid semi-axes in proportion to it. But, as a result, he either had to make two measurements of extraordinary refractions using two different planes of incidence to establish his parameters, or else he had to cut the crystal in a parallel to the line of normal deviation for the natural facet and then measure the radius for a plane of incidence parallel to the principal section. Obviously this last requires using the measurement of the normal deviation obtained from HUYGENS' technique and would, moreover, be extremely difficult and inexact. How, then, did WOLLASTON determine the proportions of the spheroid?

What he did was to adopt as an assumption what HUYGENS had empirically demonstrated, *viz.* he assumed that the semi-minor axis of the spheroid is equal to the radius of the sphere that governs ordinary refraction:

> By assuming, as HUYGENS has done, the equality of this power with the maximum of the oblique refraction, we have sufficient data for construction of the spheroid by which the refractions are regulated; for we have 0,67204 (Exp. 1) as major axis of the generating ellipse, and 0,6035 (Exp. 7) will be the minor axis, parallel in position to the short axis of the spar[44].

HUYGENS had made no such assumption, and that WOLLASTON did destroyed his fourth experiment as a confirmation of the theory proper: the experiment confirmed only the proposition – which is not theoretically required – that the semi-minor axis of the spheroid and the radius of the ordinary sphere were equal.

I think that WOLLASTON was himself aware that this experiment confirmed only the equality of radius and semi-minor axis, but that he was for some reason convinced that the equality is a fundamental part of the theory. Thus, assuming the equality, WOLLASTON had 0.6035 for the spheroid's semi-minor axis, which is the inverse of the ordinary index, 1.657, measured in the seventh experiment. From this, WOLLASTON easily calculated the semi-minor facet axis, CG; the correct result is 0.6354, whose inverse is 1.5736. Now, although WOLLASTON correctly gave the reciprocal (1.5736), he incorrectly claimed that 0.6365, and not 0.6354, was the result of the calculation[45]; 0.6365 was, in fact, the result of experiment. It seems to me that this was no slip of the pen, that WOLLASTON

[44] *Ibid.*, p. 384.
[45] *Ibid.*, p. 385.

did not without reason (though unintentionally) replace the result of the calculation with the result of experiment: he expected to get a much better agreement than he had found precisely because he believed that the equality of semi-minor axis and radius was a fundamental part of the theory. Although he no doubt did not purposefully mislead the reader, his error here reflects his mistaken view of the basic requirements of HUYGENS' construction. This in turn indicates that he could not have plumbed the depths of the *Traité*, and that helps to explain why he based his second and third experiments on a law which is there invalid.

These two experiments, and the measurement of the normal deviation using HUYGENS' technique, were all that was left in the way of quantitative confirmation of the theory. Like the first, fourth, fifth and seventh experiments, the second and third were based upon the presumed validity of the law of proportions. For the natural crystal, that law is only valid when the plane of incidence is either parallel or perpendicular to the principal section. More generally, the law is valid whenever the intersection of the plane of incidence and the tangent plane to the spheroid which is below the plane of separation and parallel to it contains the point of tangency (experiments four and five), or whenever the plane of incidence is perpendicular to the principal section (experiments one and six). Neither situation obtained in the second and third experiments. And yet WOLLASTON here obtained his best fit between theory and experiment, better even than the agreement in the fourth experiment, which, as we have seen, tested the relationship between the radius of the ordinary sphere and the spheroid's semi-minor axis.

To understand just what occurred, we must have a precise prediction of the angles whose sine WOLLASTON was measuring in his experiments. We can use MALUS' formulae to do so[46]. WOLLASTON's device measured the angle of emergence from the crystal and into the prism of flint glass of a ray that had passed nearly parallel to the crystal facet. We can envisage the situation at the prism-crystal interface as one where the ray observed, and which is actually reflected from the surface of the crystal, in effect produces a refraction of 90° within the crystal. Consequently the tangent of the angle of refraction is infinite, and the denominator in MALUS' formulae must vanish:

$$1-(1.583)^2 \sin^2 i(CG^2 \cos^2 w + CP^2 \sin^2 w)=0. \tag{9.4}$$

Here i is the angle of incidence, and w is the angle between the plane of incidence and the principal section.

In WOLLASTON's first experiment, w was 90°; in the fourth experiment w was 0°. Consequently i should have been 69°32′ in the first, and 83°14′ in the fourth. WOLLASTON's measures for these cases were 1.488 and 1.571, respectively. These were equal to $gf/10$. Since gf/ef was equal to $\sin i$, and ef was 15.83, WOLLASTON had measured angles of 70°3′ and 82°56′, giving experimental errors

[46] Appendix III gives MALUS' formulae in terms of the spheroid axes we have been using to this point. To use these formulae for WOLLASTON's experiments one must multiply N by the factor 1/1.583 where 1.583 is the index of refraction of WOLLASTON's prism B.

(compared to modern values) of 31′ and 18′. In the second and third experiments, the incidences should have been 73°20′ and 76°9′; WOLLASTON obtained 73°31′, 75°50′, for errors of 11′ and 19′.

Using the values for CP, CG given by experiments one and four, WOLLASTON determined that the facet semi-axes cut by the planes of incidence in experiments two and three should be 0.6573 and 0.6500, whose reciprocals are 1.5215, 1.539 respectively. If the law of proportions were here valid, then the measured values of $gf/10$ should have been close to these values. And indeed they were, being 1.518 and 1.537 respectively. Translated into angular differences, WOLLASTON found that theory and experiment differ by 28′ and 19′. These differences are almost the same as the differences between the true incidences, calculated from MALUS' formulae, and the incidences WOLLASTON measured.

It therefore seems that the refractions can be predicted by the law of proportions to within the limits of accuracy of WOLLASTON's experiment even where the law fails. To test the point further, I have calculated the predicted incidences that produce a 90° refraction for nine planes of incidence, according to the incorrect law of proportions and according to MALUS' formulae, using, in both cases, modern parameters. In no case does the angular difference exceed 22′. Nor would WOLLASTON have ever found a much greater difference between theory and experiment, since, using his parameters in the law of proportions, the difference between predicted and true incidence is less than 30′[47].

WOLLASTON's device could measure only incidences that produced refractions of 90°; it could not measure the refraction at arbitrary incidence. This limitation was simultaneously the strength and the weakness of his experiment. It was a strength because he obtained numbers that could be directly interpreted as parameters of the ellipsoid. It was a weakness because only a generally invalid law endowed the measurements with meaning. Here, then, is an interesting example of a pre-existing experimental device which determined the way in which the experimenter perceived a theory. If WOLLASTON had not read the *Traité* with his device so strongly on his mind, he might never have thought to test the theory, because he would have realised that the law of proportions was generally invalid. The evidence proves unambiguously that WOLLASTON came to the *Traité* with definite expectations and hopes, which he fulfilled by reading it in a carefully selective manner.

What I am, *in fine*, led to is the astonishing conclusion that there was precisely one experiment in WOLLASTON's article that could have been thought of as a quantitative confirmation of HUYGENS' construction: WOLLASTON's calculation, using the observations of CS, CP, that the normal deviation should be 6°7′30″, compared with his observation of 6°16′. And to measure this angle WOLLASTON had recurred to HUYGENS' technique and not, of course, to his device. This acutely designed apparatus was, in effect, merely a kind of calculator for parameters and not itself a tester of theory.

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[47] Further calculations and comments are given in Appendix VI which confirm the conclusion that WOLLASTON's device cannot differentiate between the law of proportions and MALUS' formulae.