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Corpuscular Optics and the Wave Theory of Light: The Science and Politics of a Revolution in Physics

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The adoption of the wave theory of light over the corpuscular system in the early nineteenth century was a turning point in the history of modern physics. It drastically altered the field of optics and posed the first serious difficulty in the action-at-a-distance world view that had dominated European physical science from the time of Newton. From the standpoint of Kuhn's *Structure of Scientific Revolutions*, the transition to waves seems to qualify as a 'scientific revolution', perhaps the first in post-Newtonian physics.[1]

The purpose of this paper is to look at that scientific revolution from a particular point of view, focusing on a side of the story which has received little attention: the activities of the corpuscular school in France in the era immediately preceding and contemporary to Fresnel's articulation of the wave theory.[2] This study will suggest two important revisions in our understanding of the wave revolution: First, it will argue that the decade preceding Fresnel was not one of confusion or difficulty for the corpuscular school but rather one of expansion and success. The work of Laplace, Biot, Malus and Arago in the first decade of the nineteenth century led to a revival of interest in optics in France, and, in the context of Laplace's Newtonianism, a renewed commitment to the 'emission theory' as well. That theory was called upon to explain a host of newly-discovered phenomena related to double refraction and polarization, and, by and large, it was successful. By 1815 a modified and sophisticated version of the emission system was an essential part of French physical orthodoxy.

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Secondly, it will show that the fight to establish the wave theory was carried out on a political as well as an intellectual level, and that both struggles were necessary for the final rapid, total victory of the undulationists. We shall see that the consistent strategy of the corpuscular school in this era was to ignore developments in the wave theory, and that it required the publicizing efforts of Arago as much as the research of Fresnel to bring the issue to a head in the Paris scientific community. Ultimately the controversy was resolved not through the conversion of the corpuscularians, but rather through the ascendancy of an anti-Laplace faction to dominant positions in French science.

These new perspectives suggest an important modification to Kuhn's theory of scientific revolutions. We find that a state of crisis need not characterize a field as a whole prior to a scientific revolution. In France in 1815, emissionists such as Biot, Haüy, Poisson and Laplace, had few if any qualms about the corpuscular theory, while Arago and Fresnel on the other hand saw it as hopelessly confused and inadequate. The source of these differences lay in the social situation of the protagonists; the first four were solid members of the scientific establishment, whereas the latter two were to some degree outsiders. Thus, social and political factors, which Kuhn rightly regards as crucial for the *resolution* of scientific revolutions, seem equally important for the *initiation* of revolutions as well.

The interpretation of the wave revolution presented here requires the development of new material not previously treated in the history of optics. The corpuscular theory of polarization, and particularly Biot's extension of that theory to account for colouration effects (chromatic polarization, rotatory polarization, Newton's rings), has never been seriously or sympathetically treated in the secondary literature. Yet this theory was perhaps the most significant development in optics in the period 1810-15, and formed the subject of the only direct debate between the wave and corpuscular schools in France. Moreover, it was in response to Biot's theory that Fresnel worked out the concept of transverse waves, without which his final undulatory system could never have been fully elaborated. Biot's work thus plays a central role in the wave revolution, and its absence from the secondary literature requires us to discuss it (along with Fresnel's treatment of the same material) at considerable length.

I. CORPUSCULAR OPTICS IN THE ERA BEFORE FRESNEL (1805-15)

Laplacian Physics and the Revival of Interest in Optical Research: 1805-8

To understand the context of the wave-corpuscle debate in Restoration France (the period about 1815-25) we must first consider certain developments from the previous decade. This was an enormously important decade for French physics. Under the leadership of Pierre Simon Laplace and Claude Louis Berthollet, two acknowledged centres of power and prestige in the First Class of the Institut de France,[3] French physical scientists developed in this era a programme to mathematize experimental physics; to encourage the use of precision instruments, of quantitative techniques and of algebraic equations in the collecting and interpreting of data. The programme was a continuation of the exact quantitative work of Laplace and Antoine Laurent Lavoisier on heat,[4] of Charles Augustin Coulomb on electricity and magnetism,[5] of Jean-Charles Borda on weights and measures,[6] of Gaspard Monge on surface tension,[7] and of the abbé Rene-Just Haüy on crystallography[8] in the 1780s and 1790s.[9]

The reform programme formed a main activity of the famous Societe d'Arcueil, a sort of advanced research seminar in the physical sciences which met every two weeks at Berthollet's summer home and laboratory in Arcueil.[10] Led by Berthollet and Laplace, the Society was composed of the best young physicists and chemists in Paris: Jean-Baptiste Biot, Simeon Denis Poisson, Joseph Louis Gay-Lussac, Étienne Louis Malus, Dominique Francois Arago, Pierre Louis Dulong and A.M.T. Petit. These men were all trained in laboratory work and mathematics at the newly-founded École Polytechnique, and possessed the combination of empirical and algebraic expertise needed to become the new 'physiciens géometres', in Biot's words; the mathematical physicists who were 'able to skillfully combine calculation with experiment to attain by the simplest methods the ultimate in precision'.[11] From its informal beginnings in 1802 to its demise around 1813 this group was the crucible in which the most important new developments in the physical sciences were first worked out in France.

In addition to the reform programme, the physicists of Arcueil were involved in the articulation of a specific theoretical structure for physics – a structure which Robert Fox has called 'Laplacian physics'[12] and which I have called 'the short-range force paradigm'.[13] The core of this new physics was an attempt to explain all the phenomena of the physical world which did not result from the forces of gravitation, electricity and magnetism, in terms of short-range intermolecular forces. The models for this type of explanation were Laplace's theories of atmospheric refraction[14] and capillary action[15] from Book X of the *Traité de mécanique céleste* and its supplements. Robert Fox has discussed Laplacian physics at great length recently, so I shall not repeat that discussion here.[16]

Corpuscular optics was the cornerstone of Laplacian physics, offering many research subjects for the Arcueil group to pursue. In fact, Laplace's interest in short-range forces seems to have been the cause of a noticeable burst of activity in optical research by French physicists in the years 1805-8. Biot was the first to follow up this line of activity, taking his inspiration directly from the discussion of atmospheric refraction in Book X of *Mécanique céleste*. Laplace there had examined the motion of a light corpuscle through successive spherical layers of the earth's atmosphere and had derived a 'fundamental differential equation of the refraction of light' which he could integrate under various hypotheses concerning the variation of density of the atmosphere with altitude and temperature. The equation was based on the unproven assumption that 'the refractive forces of the strata of the atmosphere are proportional to their densities'.[17] Laplace asked Biot and Arago to verify this assumption in a detailed study of refraction in gases.[18] Biot and Arago not only verified Laplace's assumption, but also made the first direct measurement of the refractive power of air, and compiled an accurate table of the refractive power of oxygen, nitrogen, hydrogen, ammonia and carbon dioxide. They concluded their work with a long discussion of the possibility of using refractive powers as a guide to the chemical constitution of different substances.

Following this piece of research, Biot used the same apparatus to determine the refractive index of water vapour and the effect of humidity on atmospheric refraction.[19] Thénard and he then measured the refractive powers of aragonite and calcium, and of the gaseous products of their reactions with acids, in an attempt to determine the difference in chemical composition of these two nearly-identical minerals.[20] Finally, Biot, in 1809, wrote a long memoir on mirages which he studied under various conditions of atmospheric temperature, pressure and humidity, and which he explained in terms of the corpuscular theory.[21]

Malus's Work and the Corpuscular Theory of Polarization (1808-12)

While Biot was completing his investigations of gaseous refraction, another more fruitful line of research was being opened up by a classmate of his from the École Polytechnique – the engineer Étienne Louis Malus. Malus in 1807 presented to the First Class of the Institut de France an 'Analytical Treatise on Optics'[22] in which he developed mathematical techniques for predicting the paths of light beams under various conditions of reflection and refraction. Although ostensibly a work in geometrical optics, Malus's study was characterized by the treatment of light rays as the paths of particles travelling through space, and was thoroughly corpuscularian in tone and language. The work was well received by the First Class, and ultimately printed in its *Mémoires . . . (des) Savans Étrangers.*[23]

From geometrical optics Malus moved immediately to more physical considerations. In a memoir, 'On the Measure of the Refractive Power of Opaque Bodies,'[24] he used the corpuscular theory to criticize William H. Wollaston's method of measuring refractive indices of non-transparent substances.[25] Wollaston would attach a sample of an unknown substance to a prism of known refractive power and measure the angle of total internal reflection in the latter. The sine of that angle was equal to the reciprocal of the relative index of refraction of the two substances.

Malus argued that this procedure, while acceptable for transparent media, could only be used with opaque substances if the formula for determining the refractive power was modified. Such a modification was necessary because, according to Laplace's theory of internal reflection in Book X of *Mécanique céleste*, opaque substances reflect light molecules in a different manner from transparent ones.[26] In the latter, light molecules penetrate a short distance into the second medium before being turned around; in the former, reflection takes place entirely within the first medium. Thus different formulae are required for each case. To demonstrate this argument Malus determined the refractive power of bees' wax in the liquid (transparent) and solid (opaque) states, using the two different formulae indicated by his (and Laplace's) theory. The results were identical, as they ought to be for a single substance in the corpuscular theory.

Malus's next piece of work was his famous research into double refraction for the Institut prize competition of 1808,[27] a work whose background and content I have discussed in a recent article.[28] Malus's study had two important consequences for the history of optics. First, through a series of careful, accurate experiments backed up by sophisticated mathematical analysis, Malus confirmed the law for the direction of the second ('extraordinary') ray in Iceland spar that had been proposed by Huygens in the seventeenth century.[29] Huygen's law, which was directly derived from the wave theory of light, had been ignored or discredited for more than a century, until William Wollaston in a study of double refraction in 1802 confirmed it for several cases.[30] Malus, who was definitely aware of Wollaston's work when he began his studies (see footnote 30), reconfirmed Huygen's law in a more general and rigorous fashion and extended its application to other doubly-refracting crystals. His work inspired Laplace to re-derive Huygen's law from the corpuscular theory,[31] and the new derivation quickly became part of French physical orthodoxy.[32]

A second result of Malus's work, which was to have profound effects on the corpuscular theory, was his discovery of polarization by reflection.[33] It had long been known that doubly-refracted light exhibits certain asymmetric properties which we now regard as effects of linear polarization. These asymmetries were first recognized by Huygens[34] who admitted his inability to account for them. They were also discussed by Newton in the *Opticks*[35]. Malus found that these esoteric effects could be produced by simple reflection from any transparent medium, and therefore had to be the result of some fundamental modification of light in its interaction with matter. 'Polarization', as Malus called this modification, quickly became a major topic of research for scientists interested in optics. (Malus, Arago, Biot, David Brewster and Thomas Seebeck all worked extensively on polarization in the years immediately following Malus's discovery.)

In addition to stimulating research, the discovery of polarization brought forth a major expansion of the corpuscular paradigm. Most immediately, it served to greatly strengthen the theory. Polarization was completely incompatible with the wave theory of light, as it was imagined in the early nineteenth century[36] (See Part II for a further discussion of this point.) Light corpuscles, on the other hand, could be imagined in all sorts of asymmetric shapes which could produce the observed effects. Newton, who first discussed polarization from a corpuscular viewpoint, had proposed as an explanation for polarization by double refraction that 'every ray of light . . . has two opposite sides originally endued [*sic*] with a property on which the unusual refraction depends, and the other two opposite sides not endued with that property'.[37] The orientation of these sides with respect to the crystal axis would determine whether the ray was refracted ordinarily or extraordinarily, and whether it became polarized parallel to or perpendicular to the principal section of the crystal.

Malus transposed the asymmetries to the particles themselves. He imagined each light corpuscle to possess three axes, 'a, b and c,' and supposed 'that the a-axis is always in the direction of the ray, [and] the b and c axes, under the influence of repulsive forces, become perpendicular to the direction of these forces'.[38] These axes, however, were not merely representations. They were actual physical entities which could be acted upon by forces in matter to produce the polarization effects observed in reflection and double refraction:

> . . . then all the phenomena of total reflection, partial reflection and the most extraordinary circumstances of double refraction become consequences of one another, and can be deduced from the following single law, to wit:
> If we consider in the translation of the light molecules their motion around their three principal axes, a, b, c, the quantity of molecules whose b or c axes become perpendicular to the direction of the repulsive forces will always be proportional to the square of the sine of the angle these lines will have to describe about the a-axis in order to take up this new direction. . . .[39]

This 'sine-squared' law, which Malus initially determined experimentally from the intensity of light beams that had undergone two double refractions, or two reflections at the polarizing angle, was one of his most satisfying accomplishments. It united under a single force and a single mathematical formalism several properties of light which at first had seemed unrelated: internal reflection, external reflection and extraordinary refraction.

Malus did not develop his corpuscular model for polarization any further. In fact, in his later memoirs (from the two years prior to his death in 1812),[40] he spoke only of light rays and seemed to be moving away from molecular pictures. However, his ideas and the mathematical formulae from the 'Theory of Double Refraction' were taken up and extended into new areas by Biot.

Extension of the Corpuscular Theory of Polarization to Colouration Effects: The Work of J.B. Biot, 1812-18

Biot developed a discovery originally made by Arago in 1811. Looking for the effects of polarization in 'Newton's rings', Arago exposed thin sheets of mica to polarized light and examined the transmitted beam through a rhomb of doubly-refracting material. To his great surprise the two images he observed were coloured complementarily and these colours could be made more or less intense by rotating the rhomb or the mica lamina. Arago realized that the mica sheet had modified the incident light beam in such a way that it was neither totally polarized nor totally unpolarized. Rather, the 'rays of different colours passing through the mica have their axes placed in different positions'.[41] He called this modification 'depolarization', and proposed the following physical model to account for it:

> If the properties of these rays depend, as has been supposed, on the particular disposition of molecules of which it is formed then for [polarized] rays the axes of the molecules of diverse colours are parallel while for [depolarized] rays there are molecules of different tints whose axes are in different directions.[42]

Biot followed up Arago's experiments in 1812 with two long memoirs on depolarization (read to the Institut in June and December 1812, respectively).[43] At first he believed that the effect was due to a partial double refraction in the thin crystals (i.e. that only molecules of a certain colour or colours underwent extraordinary refraction).[44] He applied Malus's sine-squared formalism to this case and derived two formulae for the intensities of the two coloured images. Unfortunately these formulae predicted the wrong experimental results for several simple situations. Having no alternative physical model in mind, Biot tried to manipulate the formulae. Finally he found a pair of equations which accounted for all of the effects he had observed and correctly predicted several new ones:

$$F_o' = O\cos^2 a + E\cos^2(2i - a); \quad (1)$$
$$F_e' = O\sin^2 a + E\sin^2(2i - a). \quad (2)$$

In these formulae, F_o' and F_e' are the intensities of the ordinary and extraordinary rays in the analyser, O and E represent two complementary colours, and i and a are the azimuths of the axes of the thin depolarizing crystal and the analysing rhomb with respect to the plane of initial polarization.

The colours O and E were determined from the thickness of the depolarizing crystal in the following manner. Biot noticed that when crystals of increasing thickness were observed under identical conditions, the colours ran through the spectrum in the order of the colours of Newton's rings of progressively larger diameter. Newton had, in the *Opticks*, prepared a table relating the colour of each ring to the width of air gap producing it.[45] Biot found that if he multiplied the thickness of his crystals by an appropriate conversion factor, and looked up the colour corresponding to that thickness in Newton's table, he could predict the tint of the extraordinary ray with a fair degree of accuracy.

In his second memoir,[46] Biot proposed an interpretation of the new formulae. Equations (1) and (2) imply quite clearly that, in Biot's words, there are

> two directions of polarization in the light transmitted (by the thin crystal); that of the colour O in the azimuth zero (the original plane of polarization) and that of the colour E, not along the axis (at i) or perpendicular to it (at $i + 90^0$) but on the other side of the axis at an equal amplitude (at 2i).

To see this, consider the case in which $\underline{a} = 0$. Then $\underline{F}_o' = 0 + E \cos^2 2\underline{i}$ and $F_e' = E \sin^2 2\underline{i}$. The colour 0 is at azimuth zero, the colour E is at azimuth $2\underline{i}$.) On the molecular level this can only mean that:

> The molecules which compose the colour 0 conserve their axis of polarization in the plane of the meridian [the original direction of polarization] and the molecules which comprise the colour E turn their axes . . . [to] an angle of 2i with this same plane.[47]

Moreover, and here is the crucial part,

> . . . the colours 0, E . . . are different according to the thickness, and, by observing their repetition as the thickness changes, one sees that *the same light molecules are by turn brought to one or the other polarization following alternations which are periodic for each.*

Thus the light molecules oscillate periodically between two azimuths of polarization:

> These oscillations are the same amplitude for luminous molecules of all colours, but their speeds are unequal. The violet molecules turn faster than the blues and these faster than the greens and so on until the red molecules which are the slowest. This inequality of speeds causes there to be at any given thickness of the lamina different colours at the two limits of oscillation, and that is what produces the two coloured beams that one observes when one analyzes the transmitted light.[48]

This hypothesis of oscillating light molecules (also known as the theory of 'mobile polarization') was generally condemned as fanciful or speculative after the triumph of the wave theory in the 1820s.[49] It is important for us, however, to see it in perspective, as a perfectly straightforward conclusion from equations (1) and (2) and the fact that the colours repeat themselves with increasing crystal thickness. Given a commitment to the corpuscular theory of light and to Malus's interpretation of polarization, it is hard to see how Biot or any other scientist in his place could have arrived at very different results. Indeed, Arago's reference to 'the general assent of natural philosophers to the laws which have been the foundation of the theory of *moveable polarization*' (in his article on 'Polarization' for the *Encyclopedia Britannica* [5th edn, 1833], 851) indicates that most physicists of the era agreed with Biot's conclusions.

As he delved deeper into the phenomena of chromatic polarization Biot found new links with other aspects of corpuscular optics. In particular he saw an analogy between his hypothetical oscillating light molecules and Newton's famous 'fits of easy reflection and transmission'. Newton had proposed that:

> every ray of light in its passage through any refracting surface is put into a certain transient constitution or state which, in the progress of the ray returns at equal intervals and disposes the ray at every return to be easily transmitted through the next refracting surface, and between the returns to be easily reflected by it.[50]

These states or fits were different for each colour and thus determined which tints would be reflected and which transmitted at a given surface. Although not a physical mechanism in the same sense as Biot's oscillations, Newton's fits none the less performed a totally analogous function; separating out molecules of different colours in a periodic manner. Biot recognized the analogy and placed a great emphasis on it in his theory:

> This perfect identity in the succession of colours and their progressive mixtures, in the periods and their intensities and finally in the smallest circumstances of the changes in their nuances shows the agreement that exists between the laws of periodicity that link these two phenomena.[51]

Biot not only stressed the analogy between his theory of oscillations and Newton's theory of fits; he took great pains to make the latter known to his contemporaries in France. In volume IV of his *Traité de* *physique mathématique et expérimentale* (Paris, 1816), Biot devoted a whole chapter, nearly thirty pages, to a detailed exposition of the theory of fits. This discussion was considerably longer than Newton's, and involved an extensive re-working of Newton's ideas into algebraic form. It also involved a fundamental revision of Newton's mechanism for the fits. For Newton the exact physical nature of the fits was unknown. He did allow himself and his readers to speculate that the fits might arise from an interaction of vibrations in the refracting medium with the motion of the rays of light. But Newton made it quite clear that he had no particular commitment to this or any other hypothesis.[52]

For Biot, on the other hand, the physical nature of the fits was considerably more well-defined. Fits resulted not from vibrations in the medium but from 'modifications impressed upon the light molecules in their passage across the first refracting surface'.[53] Biot even proposed, at the close of his study, a specific physical mechanism to account for the fits:

> . . . all the phenomena which depend on the fits of easy reflection and transmission could be represented with the most perfect fidelity by attributing to the light molecules two poles, one attractive, the other repulsive, which they would present alternatively to the surfaces of bodies, by turning with a uniform motion around their centre of gravity. The light molecules and the surfaces of bodies would then be in the situation of two magnets which approach one another with their like or unlike poles. When the light molecules, near the surface, presents its attractive pole, the attraction which results increases its speed and augments its tendency to be transmitted . . . When, on the contrary, the molecule presents its repulsive pole to the surface, the repulsion which results weakens greatly the tendency to penetrate the surface due to the direction of motion, and the corpuscle becomes more susceptible to being reflected. In this schema the interval of the fits would be nothing other than the time elapsed between two successive returns of the light molecules to the same phase of their rotation, and the lengths of the fits would be the spaces described by the molecule during one interval.[54]

This model of corpuscular 'poles' rotating about their centres of gravity and being attracted or repelled by forces in matter was clearly closer to Biot's theory of oscillations or Malus's picture of polarization than to Newton's original idea of fits (although Biot did try to attribute the model to Newton).[55]

Biot's last major contribution to the corpuscular paradigm was his theory of optical rotatory polarization. Arago had noticed[56] that thin crystals of quartz cut perpendicular to their axes produced coloured images similar to those found in mica. Biot repeated Arago's experiments in 1813 and realized that he was dealing with an entirely distinct phenomenon from depolarization or mobile polarization.[57] Each colour, Biot noticed, had its plane of polarization rotated through a different angle, and those angles became greater with increasing crystal thickness. This seemed to mean that the quartz crystal impressed upon the light molecules 'a rotary motion about their centre of gravity' (this rotation would take place in a plane perpendicular to the direction of propagation, and would therefore be different from the rotation that supposedly produced the fits). The rotation would be of different speeds for different molecules, 'the violet molecules turning faster than the blue, the blues faster than the yellows, and so on down to the reds which will be the slowest of them all'.[58]

In 1818 Biot published a complete treatment of rotatory polarization.[59] Using monochromatic light of different colours he showed that the angles of rotation of the plane of polarization of the colours were proportional to the thickness of the crystal and 'reciprocally proportional to the square of their fits or to the length of their vibrations in the undulatory system'.[60] This inverse square law is known today as 'Biot's law'. Biot devised a rigorous method for determining the relative contributions of each colour to the two beams in the analyser using an integral form of Malus's sine-squared law and a colour-mixing formalism derived by Newton.[61] He showed that optical rotation was produced by liquids like turpentine and various sugar solutions, and that some substances rotated the plane of polarization to the left, while others rotated it to the right. Finally, he demonstrated that optical rotation was a property of the molecules of matter themselves, independent of their state of aggregation, and that it could thus be used to determine the nature of unknown compounds, especially of organic solutions.

Summary: The Corpuscular Paradigm from 1805 to 1815

This discussion has merely outlined some of the activity in optical research in the second decade of the nineteenth century. Polarization by refraction; the determination of the polarizing angle for various media; Brewster's law; polarization by multiple reflection from metals; the production of a single polarized ray by minerals like agate and tourmaline; 'positive' and 'negative' extraordinary refraction; the production of chromatic polarization by pressure or rapid crystallization in transparent media; the use of polarized light to locate crystal axes; and the discovery of bi- and tri-axial crystals, were all also discovered or investigated in this era by researchers working largely within the corpuscular paradigm.[62] None the less, the work of Biot, Malus and Laplace illustrates several important points about the corpuscular theory of light in the era immediately preceding Fresnel. First, the theory was for its practitioners not just a mode of representation, but an interpretive tool which they used consistently and successfully in their research. Malus's critique of Wollaston based on Laplace's theory of internal reflection; Biot's proposal that the 'action' of bodies on light be used to determine chemical composition; Laplace's theory of extraordinary refraction; Malus's and Biot's interpretation of polarization phenomena in terms of the motions of axes of light molecules — these are all examples of the active use of the corpuscular theory in the interpretation of new data.

Secondly, the theory was capable of modification, extension and reorientation around the concept of molecular motions and short-range forces. Thus Laplace in the *Mécanique céleste* made particle velocity and intermolecular forces the determinants of whether or not a molecule of light is reflected at a surface; Malus and Laplace postulated a new force to account for extraordinary refraction; Malus gave light molecules axes which could be rotated into polarized or unpolarized positions; and Biot attributed to the axes various rotational and oscillatory motions to account for mobile polarization, rotatory polarization and the fits of easy reflection and refraction.

Thirdly, the theory was quite successful in accounting mathematically for the phenomena, in relating different effects to one another through a single causal mechanism and in suggesting new areas for study. Malus's and Laplace's derivations of the law of double refraction; Malus's sine-squared law; and Biot's theories of mobile and rotatory polarization and his use of Newton's fits all illustrate this point.

The theory did, of course, have its weaknesses. The various rotational and oscillatory motions attributed to the light corpuscles were ad hoc hypotheses, added to explain particular effects, and not rigorous deductions from first principles. They accounted for most of the phenomena, but failed to predict any new results. Moreover, several important effects in chromatic polarization, such as the colours observed when one inclined the polarizing crystal, could not be explained adequately by the theory. None the less, in the context of Laplace's Newtonianism, the great need for some theoretical structure with which to order the vast quantity of new polarization data, and the inability of the wave theory to handle polarization at all before 1819, the corpuscular explanation was quite an attractive one.

The strengths of the corpuscular theory in the decade 1805-15 gave its partisans a growing confidence. Haüy, in his Newtonian *Traité élémentaire de physique* in 1806 (2nd edn) expressed his preference for the corpuscular theory somewhat cautiously:

> The two great hypotheses have each in its favour authorities of great weight. However, if one compares them in all respects one cannot refuse preference to that of Newton.[63]

Biot, in his equally Newtonian *Traité de physique mathématique et expérimentale* in 1816, was far more assertive:

> The ensemble of these [polarization and refraction] phenomena seems thus today to place the system of emanations *beyond a doubt*, if one can call a 'system' that which is so natural a consequence of the facts and serves to reproduce them with such ease and exactness. [my emphasis][64]

Although it cannot be proved conclusively, it is likely that these texts reflected the views of other emissionists in the era.

II. THE CHALLENGE OF WAVES AND THE CORPUSCULAR RESPONSE

Gaining Attention: The Role of Arago

When, in 1815, Fresnel began his work on diffraction, he faced a highly successful, well-articulated corpuscular opposition fresh from a decade of successes. The fact that the opposition had complete control over the research, publishing and educational institutions in Paris made it all the more effective in imposing its world view on the scientific community as a whole. As Robert Fox has put it:

> Laplacian influence was still just as great in French scientific education as it had long been in research, and, because of strong administrative centralization, it was just as easily exercized. An examination of syllabuses, textbooks, and sets of lecture notes of the period shows that pure Laplacian physics was being taught as standard doctrine both in science courses in the lycees, and, what is even more important, in the courses that mattered most for the future of French physical science, those at the École Polytechnique.[65]
> . . . by their zeal and power within the scientific community of France Laplace and Berthollet had established a situation in which they were able to give at least certain of the main branches of physics and chemistry a remarkable uniformity.[66]

One of the ways in which this 'remarkable uniformity' was achieved was by not publicizing rival interpretations. For example, consider the French response to the work of Dr Thomas Young. Young, as is well known, developed an extended series of arguments for the wave theory of light, which he published in a book of *Lectures on Natural Philosophy and the Mechanical Arts*,[67] and in the *Philosophical Transactions of the Royal Society of London*[68] in the early 1800s. In the course of his work Young discovered the principle of interference – that two light waves arriving at a given point from two different but coherent sources, or from a single source via two different routes, can either reinforce or cancel one another to produce regions of brightness and darkness. He used this principle to account for diffraction, the colours of thin plates and his famous double slit experiment. His work provoked a sharp attack from Henry Brougham,[69] a leading British corpuscularian.

In France, however, Young's work was almost completely ignored. Not a word about it can be found in Haüy's *Traité de physique* of 1806 or Biot's longer *Traité* of 1816. Nor did the French corpuscularians respond in print to Young's detailed critique of Laplace's theory of extraordinary refraction in the *Quarterly Review* for 1809[70] or his criticism of Biot's theory of mobile polarization in the same magazine in 1814.[71] In fact, Biot seems to have summed up the French attitude toward Young in a letter to Brewster in 1816:

> I very much esteem the merit and talent of this distinguished scientist, but you will permit me to tell you that his witness has no more weight here than the authority of Aristotle against the observations of Galileo on motion . . . Even if his theory, *which I do not know at all*, were true. . . . [emphasis is mine] [72]

Clearly the corpuscular response to criticism and to developments in the wave theory was to ignore them – at least in public.

Privately, things may have been somewhat different. Malus, for example, is known to have read Young's *Lectures on Natural Philosophy* and to have taken notes from them.[73] In 1811 he wrote in a memoir that:

> These new phenomena have made us take a step forward to the truth, by confirming the insufficiency of all hypotheses imagined by physicists to explain reflection of light.[74]

He also wrote to Young privately that his experiments on polarization 'show equally the insufficiency of the two hypotheses'.[75] But Malus none the less entitled his last paper, 'On the Influence of the Forms of Molecules of Light in different Optical Phenomena',[76] thus indicating that he had not abandoned the corpuscular theory. Arago, in his biography of Malus, described him as a 'declared and immovable partisan of the theory of emissions',[77] and Arago, Malus's close friend, would have known of any change in the latter's mind.

Publicly the corpuscularians presented a united front. In reality, however, they had already lost one member from their ranks, and that individual, Francois Arago, was to play a decisive role in the contest over the wave theory. Arago abandoned the corpuscular theory some time in the period 1813-14. In his later works he claimed that his studies of polarization in Newton's rings had led him to see the insufficiency of corpuscular explanations as early as 1811.[78] Yet, in December, 1812, Arago presented a paper in which he argued that 'the properties of rays with coloured axes arise from the fact that the *molecules* of different colours have their axes of polarization directed in different planes'.[79] Thus, his actual conversion to waves must have come about after 1812. Arago's first public commitment to the wave theory dates from 1814 when he published a note on the scintillation of stars in which he employed the principle of interference.[80]

Certain developments in the First Class of the Institut may have helped push Arago toward the wave theory. In March 1812, when Biot read his first memoir on depolarization to the Institut, Arago protested that he had made the same discoveries but had not had the time to write them up. A commission from the First Class examined Arago's notebooks and verified his claim to priority.[81] In December of the same year Biot read his second memoir on the same subject and attempted to forestall a similar protest by Arago by claiming that:

> When I wished to study polarization I invited M. Arago, in the presence of the Bureau of Longitudes to have his memoirs initialed by the permanent secretaries of the Institut in order to decide what facts or theories he had discovered at the time.[82]

Arago denied that such a request had ever been made, and Biot's claim was ultimately withdrawn. The two incidents, however, began a feud which lasted for nearly three decades. Arago felt that Biot had pre-empted his discovery, and had prevented him from fully exploiting its riches. Since Biot was, in fact, notorious for 'claim jumping', and incurred the wrath of Malus and Brewster for similar actions,[83] Arago's feelings were undoubtedly justified. From 1812 onwards Arago was constantly at odds with Biot, and became increasingly alienated from the Laplace faction in the Institut.

It would be an exaggeration to claim that Arago's feud with Biot caused him to abandon the corpuscular theory. On the other hand, it seems highly unlikely that the two events were totally unrelated. It is probable that Arago's estrangement from the Laplace group made him more open and receptive to new ideas than he might otherwise have been. This is important because Arago soon became Fresnel's chief supporter in the Institut, and the instrument through which the wave theory was publicized within the French scientific community. Without the active support of an 'insider' like Arago, it is possible that the little-known engineer Fresnel would have received no better hearing from the French scientific establishment than had Young.

Indeed, in 1815, when he wrote his first memoir on diffraction, Fresnel was a very obscure figure in Parisian scientific circles. A graduate of the École Polytechnique (in 1806) and the École des Ponts et Chaussées, he had spent several years in the provinces as a civil engineer. In physics he was largely self-taught, and almost completely out of touch with the latest developments in the field. In May 1814, for example, he wrote his brother Léonor in Paris:

> I have seen in the *Moniteur* of several months ago that Biot read to the Institut a very interesting memoir on *the polarization of light*. Try as I may, I cannot guess what that is.[84]

Despite this lack of contact with recent work in optics (or perhaps because of it?) Fresnel was immediately attracted to the wave theory. His letters from 1814 indicate that months before he had done any serious research on the subject, Fresnel was leaning toward the undulatory system.[85] Just what led to this conviction is hard to discover, but Fresnel's isolation from Paris and the corpuscular dogma was no doubt an important factor. As Robert Silliman has put it:

> After many months of fruitless effort, Fresnel was anxious to make a mark on science, and he may have seen more possibilities in taking the part of a rebel. As he was far from Paris, the centre of scientific orthodoxy, and not too well informed, this role may have been all the easier.[86]

Given his ignorance of the latest developments in optics, Fresnel could not conceivably have made a dent in the French scientific establishment without some expert guidance. Fortunately, in the summer of 1815 he was able to come to Paris and update his scientific intelligence, largely through conversations with Arago (whom he met, apparently, through his uncle Léonor Merimee). Arago suggested that Fresnel read the works of Young, and probably called his attention to diffraction and interference as two promising areas in which to work. (Fresnel had shown no awareness of either subject in his letters prior to the summer of 1815.) Unfortunately, Fresnel could not read English, so his first memoir on diffraction,[87] which he sent to the Institut in October 1815, repeated much of Young's earlier work.

At this point Arago again interceded in a crucial manner, by becoming reporter for Fresnel's memoir. Since most memoirs sent to the Institut by aspiring non-members were not read by the body as a whole, the reporters alone decided for the larger group which works were worthy of notice or of publication in the *Mémoires . . . (des) Savans Étrangers.* The personality or views of the reporter could thus be decisive in determining the fate of a scientific paper. Had Fresnel's memoir been assigned to Biot, for example (who was, after all, the Institut's expert in optics in 1815), he could have ignored it (as reporters often did when they were busy) or reported it out as 'interesting but containing nothing new' (which was, in fact, the case). It is thus possible that the illustrious career in optics which began in late 1815 might have been stifled at the start by a less friendly reporter than Arago.

Arago, however, was disposed to fight for the wave theory in 1815. He had recently performed a set of experiments with A.M.T. Petit which refuted the corpuscular conclusions of his earlier work on gaseous refraction with Biot.[88] He now (8 November 1815) wrote Fresnel, urging him to confirm those results in his work which were especially incompatible with the corpuscular theory; namely, Fresnel's finding that the diffraction bands form hyperbolic curves behind the diffracting obstacle. Arago's purpose was, quite explicitly, to assemble evidence for the wave theory:

> The results which you have obtained in this area seem very important; perhaps they can serve to prove the truth of the undulatory system, so often and so poorly combatted by physicists who have not made the effort to understand it.[89]

When Fresnel sent off the new data as requested, Arago interceded with Prony, the head of the corps of engineers, to obtain for Fresnel a leave of absence to come to Paris to pursue his research. There the two men worked together on experiments which strengthened the wave theory of diffraction. They also undertook a study of interference effects in polarized light which led to an undulatory account of mobile polarization, and ultimately to Fresnel's introduction of the concept of transverse waves (see below). On 25 March 1816, Arago read to the Institut his highly complimentary report on Fresnel's work. His report concluded with the following summary:

> The simplest circumstances surrounding the formation of interior fringes are inexplicable, or at least, unexplained, by the ordinary theory. That of M. Fresnel shows at once how they are propagated, what size they must have at different distances . . . , how the sizes vary in inverse ratio to the diameter [of the obstacle]. It also explains why the position of these fringes is independent of the distance to the luminous point, a result even more remarkable since this distance has a great influence on the order of the external fringes. . . If we add that in this same theory the sizes and places of the bands of different colours are deduced from the general formula by simply replacing with the values furnished by the observation of coloured rings . . ., that there is, finally, no diffraction experiment which cannot be, I will not say explained, but even calculated by it, then one cannot but admit, whatever opinion one has on the basis of the question, that the hypothesis of M. Fresnel is worth following and deserves the attention of physicists and mathematicians.[90]

The month of his report Arago also published Fresnel's memoir on diffraction in the *Annales de chimie et de physique*,[91] the newly-revived journal of which he and Gay-Lussac had recently become co-editors. Thus, the Parisian silence on the wave theory was broken. A year later Arago was able to obtain for Fresnel a permanent assignment to Paris which gave him the time and resources to pursue his research on the wave theory. As Maurice Crosland has aptly put it: 'The acceptance of the wave theory in France owes as much to Arago's support as it does to Fresnel's experimental insight.'[92]

The First Battle: Diffraction

Arago's report effectively threw down the gauntlet for the wave theory on the question of diffraction. His challenge, however, was immediately met by the corpuscularians. The very month of Arago's report and the publication of Fresnel's memoir (March 1816), Biot read an extended corpuscular memoir on diffraction that he and a student, Claude Pouillet, had prepared. Although it cannot be proved that the memoir was a direct response to Fresnel, the timing is so close that it is hard not to draw that conclusion. (Biot began reading the memoir on 4 March, added a note to it on 11 March, read a second memoir on 18 March and added notes 'on new experiments' on 25 March and 15 April.)[93]

The memoir, which was written up as a supplement to volume four of Biot's *Traité de physique*,[94] was largely a descriptive work which detailed the phenomena, assigned a few numbers and discussed the peculiarities of the patterns of different diffracting obstacles. The only theoretical sections related the regularity of the spacing of diffraction bands of different colours to the regularity of Newton's rings, and concluded that the former, just as the latter, must be produced by fits of easy reflection and refraction. This reasoning by analogy was not as rigorous as the wave explanation, in which diffraction and the colours of thin plates both result from the same mechanism: interference. But it did at least indicate that the corpuscular theory could handle diffraction. The two scientists made a table of the spacing of the diffraction bands and showed how the location of different colours could be predicted from Newton's table of the tints corresponding to different widths of air gaps. This was a procedure totally analogous to Biot's calculation of the colours produced in 'mobile polarization'.

In reporting their experiments, Biot and Pouillet made one curious but important error. Tracing the paths of the diffraction bands behind the diffracting obstacle, they somehow found these paths to be straight lines,[95] where Newton, Young and, most recently, Fresnel had observed them to be hyperbolae. This was a crucial finding because curved paths were inexplicable in the corpuscular theory where diffraction was produced by an interaction between light molecules and the deflecting object. Once the molecules left the region of the diffractor, the forces upon them ceased and they moved in straight lines according to Newton's first law of motion. Any deviation from the linear path was inadmissible. Yet, as we have seen, curved paths were an essential finding of Fresnel; one which Arago had recently made a central argument against the corpuscular theory. The fact that Biot and Pouillet found the paths to be straight lines was, at best, an accidental reading of desired results into confusing data and, at worst, a wilful distortion for reasons of propaganda.

Having shown that the undulationists were not the only ones who could explain diffraction, the corpuscularians decided to put the issue to test by calling a prize contest on the subject. On 10 February 1817, a commission composed of Gay-Lussac, Jacques Charles, Berthollet, Biot and Laplace (all but the first 'émissionaires') proposed diffraction as the subject of the biannual physics prize competition. The wording of the contest made it clear that the commissioners sought a corpuscular explanation of diffraction:

> . . . no one [Biot wrote for the commission] has yet sufficiently determined the motion of the rays near the bodies where their inflexion operates. The nature of these motions . . . contains the secret of the physical means by which the rays are inflected and separated into different bands of different intensity and direction.[96]

Since, in the wave theory, diffraction is produced by interference some distance behind the diffractor, the terms 'inflexion' and the 'separating of rays' have meaning only in a corpuscular context. A letter from Léonor Merimée to his nephew, Fresnel, confirms this interpretation quite strikingly;

> . . . You have undoubtedly received . . ., a letter from your defender, Arago, who met me as he was coming out of a rude contest with the *émissionaries*, who found it appropriate to put in question the *diffraction of light* and proposed a prize for whoever could explain it the best according to the doctrines they have adopted. Arago, taken by surprise, confronted the enemy attack, called in his allies and succeeded in stopping the invasion; that is, he obtained that your memoir will be mentioned in the programme.[97]

It is not at all clear why the corpuscularians chose to make diffraction the subject of a prize competition in 1817. Probably they were merely repeating a tactic which had worked in 1808 for double refraction. Certainly the parallels between the two prize contests are striking. In both cases new results indicated that the wave theory was making progress in an area of optics. In both cases young corpuscular protégés (Malus in 1808, Pouillet in 1817) were working on the problem. In both cases the prize commission expected a corpuscular solution.

The difference in 1817, of course, was Fresnel who, despite Arago's favourable report and the printing of his memoir in the *Annales de chimie* in 1816, seems to have been underestimated by the commission. Fresnel took advantage of the contest to re-work his treatment of diffraction, to iron out some physical difficulties presented by the first version of his theory, to derive a mathematical law for the resultant of two waves arriving at the same point, and to add some trenchant criticisms of the corpuscular theory. His finished memoir[98] was a definitive treatise which derived nearly all of the phenomena rigorously from a few simple principles. The commission which judged the competition in March 1819 – Biot, Laplace, Poisson, Arago and Gay-Lussac – must have had no choice but to award the prize to Fresnel,[99] despite the fact that three of its members were staunch corpuscularians. (There was another entry to the contest, but it has been lost and there is no record of its author.)[100]

In winning the prize competition, the wave theorists scored an important public victory. Their success stripped the emission school of its main defence against the principle of interference: its ability to ignore it. Once the Institut had pronounced in favour of Fresnel, the interference explanation of diffraction had to be acknowledged, at least publicly, by French corpuscularians. And indeed it was. Haüy in the 1821 edition of his *Traité de physique*, and Biot in the third edition of his *Précis expérimentale de Physique* three years later, both gave variants of a wave explanation of diffraction where neither had done so in the earlier versions of their works.[101] However, Biot still expressed hope that the corpuscular theory would one day be able to handle diffraction phenomena:

> The principle of interference is, up to now, the only one with which one can explain the particularities of diffraction, and in that this phenomenon is favorable to the undulatory system. However, if one looks into the details of this explanation, one feels that it offers rather a representation of the phenomena than a rigorous mechanical theory. That is why it would be a beautiful and important discovery to match this phenomenon with ideas about the materiality of light which give such clear notions and such precise measures of so many other motions of light rays.[102]

The Second Battle: Polarization

While the diffraction battle was being won easily by the wave theorists, a more hotly-contested encounter was developing in the corpuscular strongholds of 'mobile' and fixed polarization. Polarization itself was inconceivable in the wave theory as it was thought of in the early 1800s. The reason for this was that the only kind of wave physically possible in a fluid medium such as the ether is a longitudinal or compression wave. Such a wave vibrates in the direction of its propagation. Polarization effects, on the other hand, indicate an asymmetry in a direction *perpendicular* to the motion of the wave. So serious is the difficulty of reconciling these two facts that Young, on learning of Malus' discovery, wrote in 1811 that it 'presents greater difficulties to the advocates of the undulatory theory than any other facts with which we are acquainted'.[103] Four years later, in 1815, he was even more discouraged:

> With respect to my own fundamental hypotheses respecting the nature of light, I become less and less fond of dwelling upon them, as I learn more and more facts like those which Mr Malus has discovered.[104]

Young did not give up, however. In a review of Biot's work on chromatic polarization in 1814 he proposed that the phenomena might be explained more simply by applying the principle of interference to the motion of light in a thin crystal.[105] Specifically, suppose that the gypsum lamina used by Biot causes the incident polarized ray to be doubly refracted but does not separate the two beams from one another. Then the extraordinary ray in the crystal travelling slower than the ordinary beam will be retarded with respect to it and will therefore be capable of interfering with it. The conditions necessary for interference depend only on the difference in speeds between the two rays, the thickness of the crystal and the wavelength of the light being used. Since these have all been determined experimentally it is easy to predict which wavelength, and hence which colour, will interfere destructively for a given thickness of crystal. This Young did for one particular case and found good agreement with Biot's experimental results.

Young's explanation, however, was far from complete because it did not account for the role of polarization in producing the phenomena. Why were the colours not produced by unpolarized light? Why were they not visible to the naked eye alone? What function did the analyser play in producing the colours? These questions, which could not be answered by Young's explanation, were soon resolved by Fresnel.

Fresnel began thinking about polarization and the colouration effects in thin crystals in the summer of 1816.[106] At that time he tried to produce interference between the ordinary and extraordinary rays of doubly-refracted light and had no success. He began to suspect that 'rays polarized in opposite (perpendicular) directions do not exert the same influence on one another as unmodified rays, or as rays polarized in the same direction'.[107]

To test this hypothesis Fresnel and Arago devised a number of experimental arrangements to produce interference between beams of polarized light. They found that in every case two rays polarized perpendicularly did not interfere, while two rays polarized in the same direction interfered if they originated from a single polarized source.

Having demonstrated the non-interference of perpendicularly-polarized light beams, Fresnel turned to a consideration of thin doubly-refracting crystals. He placed two identical laminae in front of the holes of a Young double-slit apparatus and examined the interference pattern produced when the axes of the laminae were aligned parallel and perpendicular to each other. His results indicated that each thin crystal produced two rays of light, one polarized parallel to the crystal axis and one polarized perpendicular to it, just as thick crystals do. This finding directly contradicted Biot's theory of 'mobile' polarization, for which the directions of polarization were aligned quite differently.

Fresnel went on, in the memoir containing these results which he presented to the Académie on 7 October 1816,[108] to sketch a complete wave theory of thin crystal phenomena. He assumed, like Young, that the lamina produced double refraction similar to that observed in larger crystals. The two rays emerging from the lamina would be out of phase with one another, but, being polarized in perpendicular directions, they would not interfere even though they travelled the same paths. On arriving at the analyser the two rays would each be split into ordinary and extraordinary components. The two ordinary components would be out of phase and capable of interfering, since they were now polarized in the same direction. The same would be true for the two extraordinary components. The colours seen would thus be the result of destructive interference *inside* the analyser. The only question that remained was why were the ordinary and extraordinary rays in the analyser coloured *complementarily* when the components of each had undergone the *same* phase shift? To explain this Fresnel added an extra assumption, namely that the extraordinary ray in the analyser was retarded by half a wavelength. This resolved the difficulty.

Fresnel was able, in his memoir, to derive formulae for the intensity of the ordinary and extraordinary rays in the analyser using Malus's sine-squared law. From these he predicted various phenomena which were 'all confirmed by experiment' (namely Biot's) and which 'prove sufficiently that they represent the facts in the wave theory as well as Biot's do in the Newtonian system'. In fact, Fresnel went on, Biot's formulae

> have the advantage over mine of indicating in each case which of the two images must correspond to the reflected or refracted rings. But the explanation from the theory of waves is much more in conformity with the general principles of polarization in crystallized substances than M. Biot's.[109]

Fresnel's readiness to acknowledge the worth of Biot's work probably helped make the latter more conciliatory toward the new theory and more willing to consider its merits. Fresnel, in September 1818, described Biot as 'the one of the three' Newtonian judges of the diffraction contest 'whose conversion to the wave theory appears to me the most advanced'.[110] That same year Biot included some work by Fresnel in his memoir on rotatory polarization and made a reference to wavelengths in his famous law of that phenomenon.[111]

The main criticism Fresnel had of Biot's theory in his early memoir was its inability to explain why thin crystals should polarize light in a different manner from thick crystals. This difference became acutely embarrassing when one tried to account for an experiment done by Biot in 1813. In this experiment Biot placed two *thick* crystals with their axes crossed at ninety degrees in a beam of polarized light, and found that they produced the same colours in an analyser as a single *thin* crystal of thickness equal to the difference between the two. Since each thick crystal alone gave rise to 'fixed' polarization it was hard to imagine how the two together could produce 'mobile' polarization. Biot himself recognized this difficulty but was never able to resolve it satisfactorily.[112]

Fresnel's memoir on chromatic polarization, along with several smaller pieces, became the subject of another important report read by Arago[113] in June 1821. Arago's account of Fresnel's work changed the tone of the debate over chromatic polarization quite drastically. Where Fresnel had been somewhat conciliatory, attacking mainly Biot's interpretation of the two-crystal phenomena (which Biot himself admitted was quite defective), Arago took the opposite approach. He wrote:

> M. Fresnel has proposed . . . to prove that the ingenious theory of mobile polarization is on many points insufficient or inexact.[114]
> This skillful physicist [Fresnel] first announces that the laws of mobile polarization given by Biot do not agree with observation except in very particular cases.[115]

This last statement was a misrepresentation of Fresnel's stated intentions in his memoirs and was instrumental in provoking the polemic which followed the report. Its basis was an experiment, described by Fresnel in a separate note,[116] in which *monochromatic* rather than white light was passed through a thin crystal and analysed with a rhomb of Iceland spar. This was an experiment which Biot, strangely, had never thought to perform, although he had used monochromatic light in his work on rotatory polarization. The results showed quite clearly that Biot's supposition that the poles of light molecules of a particular colour all line up along the azimuth 0 or 2i after passing through a thin crystal was incorrect. For crystal thicknesses which satisfied the conditions for total interference the monochromatic beam did indeed *appear* to be polarized along one of the two azimuths. (This was, as Fresnel pointed out later, only an appearance.) At intermediate thicknesses, however, a portion of the light beam could be found at either azimuth, and for certain values the beam seemed totally unpolarized. Neither of these two results was consistent with Biot's theory.

Arago's report provoked a debate in the Académie which became so bitter that the discussion had to be adjourned. On a motion by Laplace Biot was given a week to prepare a formal answer.[117] His response,[118] which was written up and published by Arago in the *Annales de chimie et de physique*, was hardly enlightening.

Biot's primary concern seems to have been to dissociate the Académie from Arago's remarks. He asked the body to 'suppress the expression of motives attributed to M. Fresnel; motives for which I can find justification only in a few words contained in a note . . .'.[119] He also tried to have all or part of the report suppressed entirely. Although these efforts ultimately failed, Biot did induce Arago to modify the conclusions of the report before they were adopted by the Académie.[120]

Biot's second concern was, of course, to defend mobile polarization against Fresnel's criticisms. In this, too, he was largely unsuccessful. At one point, for example, Biot tried to show that Fresnel's formulae for the intensity of the two beams in the analyser could be rewritten in such a way as to take on identical form to his own. He then argued that this formal identity made it impossible for Fresnel's theory to predict any 'particularity' which did not agree with mobile polarization. This argument, however, was invalid because the coefficients in the two sets of equations had entirely different meanings, and the agreement only held strictly for those thicknesses for which interference was most pronounced.

To the experiment with monochromatic light which seemed to be so incompatible with his theory, Biot offered two explanations. First, the light molecules need not all be 'exactly in the same phase of their fits'[121] when they enter the lamina, and, therefore, they need not all be at the same limit of oscillation when they emerge. Secondly, the red filter used by Fresnel might not have been perfectly monochromatic. Both arguments were plausible, but the first contradicted numerous statements in the early versions of Biot's theory, while the second ignored the fact that Biot had used the same filter in his experiements on rotatory polarization without questioning its monochromatic nature.

With regard to the various criticisms made by Fresnel relative to the production of colours by two thick doubly-refracting crystals, Biot could only concede defeat. He admitted his failure to predict or observe certain phenomena seen by Fresnel, and conceded that the transition from 'mobile' to 'fixed' polarization was still a mystery to him. ('I did not know, I still do not know how this transition operates.')[122] But he insisted that the difference in polarizing properties of thin or thick crystals was an empirically-based fact.

In general Biot tried to hide the difficulties in his theory behind a smokescreen of empiricism:

> I am still persuaded that [mobile polarization] is accurate as a *representation* of these phenomena, and I do not attach to it any other value.[123]
> I have never claimed in my research to establish anything other than experimental laws.[124]

These statements are hard to reconcile with Biot's attitude in the early 1810s, when he entitled a memoir 'On a New Type of Oscillation that Light Molecules Experience in passing through Certain Crystals'[125], and a chapter in his *Traité* 'On the Oscillatory Motion of the Axis of Polarization Deduced from the Preceding Phenomena'.[126] In both these cases, as in many others, the oscillation of light molecules was treated by Biot not as a representation of the phenomena, but as a necessary physical consequence of the laws he had discovered. To claim otherwise in 1821 was somewhat less than candid.

Biot's response to Arago was, in sum, not a very effective one. Although it spoke to some of the points raised by Fresnel, it did not answer any of them satisfactorily. Moreover, it completely avoided the heart of the issue: Fresnel's demonstration from the interference and non-interference of differently-polarized light that thin crystals act in a manner identical to thick ones. This demonstration, if correct, directly disproved Biot's entire theory. Yet nowhere did Biot even discuss these experiments or the conclusions Fresnel drew from them.

Arago and Fresnel had little trouble answering Biot's weak arguments. Arago, in an 'examination of the remarks by M. Biot',[127] pointed out sarcastically that Biot had not really responded to Fresnel's demonstrations and had succeeded only in obscuring the situation. Fresnel, in a 'note on Biot's remarks',[128] showed that Biot's formulae only agreed with his in certain specific cases. He also argued that the angular dependence on the azimuths 0 and 2i was an appearance resulting from the fact that two polarized beams can always be mentally combined into a single resultant. Biot, by dividing his original beam into an O-component and an E-component, had, in effect, made this mental decomposition in advance. His formulae thus merely told him how the phenomena were to be represented in terms of his own division of the rays.

Biot and Fresnel's explanations of chromatic polarization were thus a classic example of two scientists looking at the same phenomena through different paradigms and seeing different things. Not only did Biot 'see' oscillating molecules where Fresnel 'saw' interfering waves, but he even 'saw' a plane of polarization at a different orientation from that in which Fresnel saw it. Thus the corpuscular theory not only structured Biot's interpretations, but the supposedly-empirical formulae by which he represented his data as well.

Mopping-up Operations

Arago's publication of this polemic,[129] which can only have discredited Biot and the corpuscular system, marked the start of a series of triumphs for Fresnel and the wave theory. In a note contemporary to the controversy, Fresnel[130] first announced his enormously fruitful hypothesis of transverse waves. This hypothesis stemmed directly from Fresnel's research on polarization and was, in fact, the only way to explain such phenomena in the undulatory system. As Robert Silliman has put it:

> Without exception the most natural translation of the phenomena into mechanical terms was to suppose the existence of forces acting at right angles to the direction of the rays. Consistently the experimental findings became explicable only on the assumption of transverse vibrations. To Fresnel this meant only one thing, that light waves are indeed transverse, and he made bold to say so.[131]

The adoption of transversality led to a flood of new results for Fresnel. He was soon able to show how a transverse wave incident on an isotropic medium would be split into a reflected and refracted beam, each partially polarized. From the principle of continuity at the boundary and the conservation of *forces vives* he could predict the direction of each ray in space, its intensity, its degree and direction of polarization and the polarizing angle for the given medium. Thus, all the phenomena of reflection and refraction — with the single exception of dispersion — became the direct consequences of a single theory.

Fresnel then turned his attention to refraction in an anisotropic medium — one whose elasticity varies in different directions. Such a medium, of course, represents a doubly-refracting crystal. Fresnel showed that an anisotropic medium would react differently to components of vibrations parallel and perpendicular to its principal section. This difference would be great enough to separate the two components and cause them to travel different paths. Thus, the splitting of a ray into two beams and the polarizing of the two beams in perpendicular directions were shown to be parts of a single process which was a direct consequence of the action of anisotropic media on transverse vibrations.

Fresnel showed that for crystals with one axis the wave front of the second ray took on the ellipsoidal form required by Huygen's law. He also showed that in crystals with two axes, which had been discovered by Brewster in 1813, and shown by Brewster and Biot to comprise the vast majority of doubly-refracting media, *neither* ray should be refracted ordinarily (i.e. according to Snell's law).[132] This surprising conclusion, and its experimental confirmation, was one of Fresnel's greatest triumphs. Laplace, on hearing Arago's report of this work, 'declared that he put this research above everything that had been communicated to the Académie in a long time'.[133]

By early 1822 Fresnel had a nearly complete case for the wave theory of light. Reflection, refraction, double refraction, polarization, diffraction, interference, the colours of thin bodies and of thin crystals in polarized light, rotatory polarization (as the interference of two circularly-polarized beams) were all accounted for. Moreover, these phenomena could all be deduced from a few simple principles – transversality, interference, the dependence of colour on wavelength, the conservation of *vis viva* and continuity – and a few readily obtainable pieces of data (indices of refraction and wavelengths). Only one important phenomenon – dispersion – was still unexplained. Fresnel thus had an impressive case for his theory, and he publicized it in a long review article which was appended to the French translation of Thompson's *Chemistry*.[134]

The outstanding problem that remained for the wave theory was to determine how transverse waves could possibly be propagated in a fluid ether. In 1821, Fresnel offered a model of an ether whose atoms were loosely bound by weak forces offering little resistance to large displacements or the motion of macroscopic bodies, but capable of transmitting infinitesimal transverse vibrations from atom to atom.[135] This model did not satisfy Arago who broke with Fresnel over the question of transverse waves,[136] or Young, who pointed out in 1827 that Fresnel's ether resembled not a fluid but an elastic solid.[137] Fresnel, however, was not dissuaded by such objections. His goal was not to deduce transverse waves rigorously from a mechanical ether, but rather to reverse the problem and argue that since light waves can be polarized they must be transverse, and that the ether would have to be reconstructed accordingly. His reasoning can perhaps best be seen in his response to the last important challenge from a French corpuscularian, that of Poisson in 1823.

Poisson had been a commissioner for the diffraction contest in 1819 and for Fresnel's memoir on double refraction in 1822,[138] and had been active in the discussions about the nature of light in French scientific circles in the late 1810s and early 1820s. Fresnel, in a note on Newton's theory of 'fits',[139] cited Poisson frequently as the source of objections to the wave theory.

In March 1823, Poisson published criticisms of the wave theory in a letter to Fresnel and a memoir in the *Annales de chimie et de physique.*[140] The letter dealt with aspects of Fresnel's treatment of diffraction which are not relevant to the present discussion. The memoir contained the results of a mathematical derivation of the motion of a wave impinging obliquely on the interface between two fluid media of unequal density. Poisson showed that 'the ordinary law of refraction is rigorously demonstrated' but that several other phenomena, such as the intensity of reflected light and the shape of the wave front in double refraction, did not agree with his calculations. Most seriously, however, he claimed to demonstrate

> . . . that whatever the initial disturbance in the fluid, when the spherical waves emanating from it reach distances which are very large compared to their wave length, the motions of the molecules [of the medium] are sensibly perpendicular to their surfaces . . .. It is thus impossible that the molecules, even if they were initially moving perpendicular or inclined to the rays of the waves, could conserve such directions, as has been supposed in order to explain the singular phenomenon of the non-interference of rays of light polarized in perpendicular directions.[141]

In other words, according to Poisson, transverse waves, if they are set up at all, will quickly die out in a fluid medium. Fresnel's answer was simple and direct:

> The equations of motion of elastic fluids, in which you think one has to find all the forms of vibration to which these are susceptible, are nothing more, at base, than mathematical abstractions. They suppose these fluids to be composed of contiguous [unconnected] elements, compressible in proportion to the pressure exerted. This hypothesis represents well their static properties, but not their dynamic ones. For example, one cannot even deduce viscosity from it, since it supposes a contiguity [lack of connectedness] between the molecules which does not exist. Thus it is wrong for you to think that you can decide from the agreement or disagreement between the phenomena of optics and the consequences drawn from your equations, if light does indeed consist of vibrations of a universal fluid whose

nature is unknown to you. . . .

The hypothesis of transverse vibrations . . . is necessary to conceive of the polarization itself, for if one only admits, with you, vibrations perpendicular to the wave fronts or parallel to the rays, everything becomes the same around these rays and they must have the same properties on all sides. I am surprised that such a simple reflection did not relieve you of the hope of representing optical phenomena with the definition of light waves you have adopted. It is because you erred on this point at the start that your formula for the intensity of reflected light . . . do not agree with the facts; that you find the [wrong] form for the ellipsoid in crystals. . . .[142]

This was the essence of Fresnel's argument: the *phenomena* of optics indicate that light waves are transverse; therefore, the *theory* of elastic fluids, which seems to prove that transverse waves are mechanically impossible, must be revised or declared inappropriate to a discussion of the nature of light. In arguing this way Fresnel was standing conventional wisdom on its head. This turned out to be an enormously fruitful reversal, however, since it freed Fresnel to develop the powerful argument from the phenomena outlined above and also stimulated a number of brilliant mathematicians to tackle the problem of elastic media in the 1820s and 1830s.

Personal and Political Factors

The debate between Biot and Arago in June 1821 marked a turning point in the strategy of the wave theorists. Up to then they had been cautious in advancing their ideas; afterwards, they took the offensive, and with great success. This change in approach seems to coincide with certain personal and social developments in the Parisian scientific community in the early 1820s, and it is to these that we must look in order to gain a complete picture of what was happening.

On the personal side, the decade-old feud between Biot and Arago had erupted again in early 1821, after a few years of relative quiescence. In 1817 the two men's wives and friends had arranged a *rapprochement* which even, according to Biot's biographer Lefort, included provisions to avoid future competition over positions and honours. But an unexpected vacancy in the post of inspector of studies at the military schools of St Cyr and la Fleche in late 1820 wrecked the fragile accord. Biot and Arago both went after the position and when Biot won, Arago, who was probably in greater need of the post, 'could not forgive him'.[143] The aggressiveness of Arago's report in June 1821 may have been a result of his new-born animosity.

On the political level, an anti-Laplace faction had coalesced in the Académie in the late 1810s and by 1821 was exerting considerable influence on the affairs of the society. (Robert Fox has discussed this development at length elsewhere,[144] and I have little to add to his analysis, so I shall not go into it in great detail here.) This group included Arago, Fresnel, Ampère, Joseph Fourier, P.L. Dulong and, until his death in 1820 A.M.T. Petit. On the scientific side, Fresnel and Arago challenged corpuscular optics with the wave theory of light (supported by all six members of the group); Dulong and Petit, in their famous memoir on specific heats, challenged the caloric theory with a vibrational model[145] and Ampère, in 1820, challenged the orthodoxy of electro- and magneto-static fluids with an electrodynamic theory which made reference at times to an ether.[146] This last development may have had direct bearing on the polemic between Biot and Arago. In April 1821, Biot wrote a review article on the research done in France on electromagnetism since the announcement of Oersted's discovery by Arago six months earlier.[147] Biot's article slighted and misrepresented Ampère's work, with whose conclusions Biot disagreed, and undoubtedly aroused considerable resentment among the anti-Laplace group. This resentment may have been a factor in provoking the debate over mobile polarization two months later.

Politically, the anti-Laplace faction had won positions of power in the scientific community by the early 1820s. Arago had been co-editor of the *Annales de chimie et de physique* since 1816, and had published articles by Fresnel, Fourier and Ampère which might never have been printed in the Laplace-dominated *Mémoires de l'Académie des Sciences.* H.M.D. de Blainville, a non-Laplacian, became one of the editors of the *Bulletin des sciences* of the Société Philomathique in 1821, replacing Poisson. When Fresnel replaced Biot in the same position in 1822 the anti-Laplacians gained control over a second important journal in the French scientific world. In the Académie, Arago (1809), Ampère (1814) and Fourier (1817) were already members, and Dulong and Fresnel were both elected in 1823. Finally, in the decisive test of strength, Fourier, in 1822, beat Biot for the permanent secretaryship of the Académie by the lop-sided margin of 38-10,[148] despite Biot's sixteen years' seniority in the Institut and well-known literary ability (an important function of the permanent secretary being the writing of *éloges*). This defeat, in which Arago took a leading role,[149] led Biot to retire from active scientific life in 1823 for nearly seven years.

By 1823, therefore, the anti-Laplace faction was in control of the Académie, and Arago, its leader, was in the dominant position that Laplace had occupied a decade earlier. As a critic of Arago has put it:

> Laplace was reduced to silence, M. Biot absented himself from the Institut for several years and M. Arago remained master of the battlefield.[150]

The consequence of this control was a rapid and almost complete acceptance of the wave theory by the Parisian scientific community. Unlike England, where Brewster and others held out vocally for the corpuscular theory through the 1830s, in Paris no debate on the issue seems to have taken place after 1825.[151] Indeed, almost the entire generation of physicists and mathematicians who came to maturity in the 1820s – Pouillet, Savart, Lame, Navier, Liouville, Cauchy – seem to have adopted the theory immediately. Pouillet is perhaps the best example. As protégé and collaborator of Biot in 1816, he was enthusiastically corpuscularian, helping his teacher work on diffraction and Newton's rings from an emission point of view. Fourteen years later, in an important textbook dedicated to Biot, he explained all of the phenomena of optics in terms of the undulatory system and rejected his former paradigm explicitly:

> There are the two systems. Now, if one cannot affirm without reservation that one of the two is essentially true, one can at least affirm that one of the two is not; that is the emission system.[152]

Pouillet's textbook was based on his lectures at the Paris Faculté des Sciences, where, as Biot's replacement in physics, he taught the wave theory from 1827.[153] Ampère, according to the historian of the Collège de France, was also teaching wave optics from the chair of experimental physics at the Collège, which he occupied from 1824.[154] Finally, Dulong, at the École Polytechnique, was presenting a sketch of the wave theory as early as 1821, although alongside a discussion of the corpuscular hypothesis.[155] Thus, by 1827 the wave theory was being taught at all three principal institutions for advanced education in the physical sciences in Paris. No better means for assuring its adoption by subsequent generations of French physicists could possibly have been devised.

Among the old guard, Biot and Poisson remained in the emission camp, but neither contested the wave theory openly after their battles with Fresnel and Arago in the early 1820s. Both, moreover, ultimately converted to the new theory before their deaths; Poisson in the late 1830s[156] and Biot possibly by 1846,[157] certainly by 1858.[158] Indeed, Biot seems to have moved to an agnostic position as early as 1830, as the following footnote to the third edition of his translation of Fischer's *Physique mécanique* indicates:

> In any event this [material] constitution of light must be envisaged only as an hypothesis, to which up till now a great number of facts have been assimilated. For, in reality, nothing assures us that light is composed of material particles, and many analogies would tend to represent it as the sole effect of vibrations transmitted, in the manner of sound, in a very elastic medium.[159]

Conclusion: The Process of a Scientific Revolution

According to Kuhn, normal scientific research is characterized by widespread acceptance, in the relevant scientific community, of a shared theoretical framework ('paradigm' or 'disciplinary matrix') which guides research by selecting problems, tools and methods and providing standards of accuracy and acceptability. In the course of normal research, anomalies are generated which cannot be fitted into the dominant paradigm. When these anomalies become severe or numerous enough, the field enters a state of 'crisis' characterized by a questioning of the fundamental concepts which had previously been taken for granted. The crisis is resolved only by a 'scientific revolution' in which a new paradigm is generated and comes to dominate the field. The resolution of the revolution is a socio-political event in the scientific community: paradigms are incommensurable, so a choice between two cannot be made on purely rational grounds. Rather, a new group, usually of younger men who are able to assimilate the new paradigm because they have no commitment to the old one, achieves hegemony in the field and brings its ideas to the forefront of research.

It should be clear that the account of the wave revolution given above closely corresponds to Kuhn's description in many respects. The existence of a commonly-held paradigm, the resolution of the revolution through social and political factors, the incommensurability of the corpuscular and wave descriptions of the phenomena, all agree quite well with Kuhn. Where this study departs from Kuhn is in its account of the process by which an anomaly, or group of anomalies, leads to a crisis. Kuhn is not very clear on this matter, but he implies that a crisis comes into being when the field as a whole perceives an anomaly as 'more than just another puzzle of normal science'.[160] In our study we have found, however, that different scientists in the same field — Biot, Fresnel and Arago — had very different views of the anomalies.

Of the three, only Arago behaved as Kuhn's model suggests he should – perceiving a crisis in the corpuscular theory after long attempts to wrestle with its problems. Biot, on the other hand, perceived the problems but not the crisis, while Fresnel perceived the crisis before he was fully aware of the anomalies. In each case social factors combined with intellectual ones to influence the scientist's perceptions. Biot's education in Laplacian physics, his role as Laplace's heir apparent in the Académie, and his published commitment to the corpuscular theory, all predisposed him in one direction. Fresnel's isolation from Paris and his frustration as an outsider reinforced his leanings toward the opposite side. Arago's defection from the Arcueil group was a factor in his adoption of the wave theory.

The divergent behaviour of the three scientists under question is not forbidden by Kuhn's model. Indeed, Kuhn himself points out that 'every problem that normal science sees as a puzzle can be seen from another viewpoint as a counter instance and thus as a source of crisis'.[161] But such divergence is not explicitly provided for by Kuhn. To him the criteria which determine whether a counter-instance will provoke a crisis are intellectual ones, relating to the depth, centrality and pervasiveness of the anomaly itself, and not to the historical actors. Yet it is precisely those actors who judge the importance of the intellectual factors, and their judgements can and will conflict. Thus diffraction, for example, was seen as a central problem by Arago and Fresnel, while to Biot it was a relatively minor concern. It is not the nature of the anomaly alone, therefore, but the situations of the scientists involved in the dispute as well, which determines the course of events.

Seen from this point of view, the process of revolution takes on a very different, and more realistic, appearance. A crisis is generally not first perceived by the scientific community as a whole, but rather by a small segment, often estranged or isolated from the centre of power and orthodoxy. The smaller group then sets out to convince the larger body that some fundamental issues need to be re-examined. It does this by engaging in three activities of a social and intellectual nature: solving problems (especially ones which the dominant paradigm cannot handle); publicizing its views; and winning recognition for its work.

All three activities are essential, but the second and third, being somewhat out of the control of the small group, are most problematic. The experiences of Young and Fresnel are most instructive here. Young had no trouble getting his papers published in the relatively open

British scientific community, but completely failed to convert anyone to his ideas. Fresnel, on the other hand, required the political leverage of Arago to win a hearing from the more closed scientific circle in Paris. Once this was achieved, however, he won relatively rapid recognition.

The difference in the matter of recognition lay in the state of professionalization of the two communities. Recognition requires agreed-upon standards as to what constitutes an adequate solution to a problem. In the relatively amateurish state of early nineteenth-century British physics there was no such agreement, so Young's critics could disregard him for philosophical reasons without confronting his mathematical arguments. In France, the Laplace school and the École Polytechnique had combined to set certain standards which were commonly accepted, even by warring parties in a scientific revolution. If one could predict numerically the results of experiments from theoretical reasoning expressed in the form of mathematical equations, then one was acknowledged to have achieved *a* solution to the problem at hand. (Not necessarily the only solution, or the best solution, however.) Thus the judges in the diffraction contest could award Fresnel the prize on the basis of his mathematics and his experiments, without committing themselves to his theory.

If the small group is successful, and begins to receive recognition for solutions to specific problems, two things may happen to bring the revolution to a culmination. First, the specific solutions may begin to fit together into a new paradigm which can then become the rallying point for a frontal attack on the old system. Secondly, the successes of the small group may win it new adherents as well as positions in teaching, research and publishing institutions from which it can spread its ideas and convert the next generation of students. If both events take place relatively quickly, as in the case of the wave theory after 1821, immediate success is assured. However, neither event need necessarily occur. Dulong and Petit's proposed vibrational theory of heat, for example, never came into being, despite considerable interest in the problem.[162]

The established group of normal scientists will probably continue with research-as-usual as long as possible. Only when the revolutionaries have demonstrated some striking success in an area of importance will it feel compelled to respond, and even then it may try to confine the problem to an isolated area. (Biot's activities from 1815 to 1821 fit this description nicely.) Finally, when the new paradigm has emerged full-bloom and begun to win adherents in the larger community, the group will experience a crisis and begin to react to the situation. Some of its members will try to re-work the old theory, some will join the new one, some will retire from active scientific life, while others will shift their research interests to safer areas.

In summary, this model for the onset of a scientific revolution sees as crucial the activities of a group of revolutionaries, and the responses of the larger scientific establishment. It thus adds a badly-needed social dimension to the scenario provided by Kuhn. If the model is correct, it should be applicable to other scientific revolutions than the wave-corpuscle debate, and the author hopes that historians of science in other fields will be motivated to test it out.

NOTES

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1. T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). Kuhn explicitly cites the transition to waves as a 'scientific revolution' on p.89.

2. For examples of recent work on the wave theory see R. Silliman, 'Augustin Fresnel (1788-1827) and the Establishment of the Wave Theory of Light' (doctoral dissertation, Princeton University, 1967); D.B. Wilson, 'The Reception of the Wave Theory of Light by the Cambridge Physicists . . .' (doctoral dissertation, Johns Hopkins University, 1968); G. Cantor, 'The Changing Role of Young's Ether', *British Journal for the History of Science*, Vol 5, No. 17 (1970), 44-62; R. Silliman, 'Fresnel and the Emergence of Physics as a Discipline', *Historical Studies in the Physical Sciences*, Vol. IV (1974), 137-62.

3. For a detailed treatment of Laplace and Berthollet as leaders of the French scientific community, see M. Crosland, *The Society of Arcueil* (London: Heinemann, 1967), 56-79.

4. A.L. Lavoisier and P.S. Laplace, 'Mémoire sur la chaleur', *Mémoires de l'Académie Royale des Sciences* (1780, pub. 1783), 355-405.

5. C. A. Coulomb, 'Mémoires sur l'électricité et le magnetism', ibid. (1785-1789, pub. 1788-1792). Collected in *Collection de Mémoires relatifs à*

la physique publiés par la société Francaise de Physique, Vol. I, *Mémoires de Coulomb* (Paris, 1885).

6. For Borda's contribution to the construction of standards and measuring instruments for the metric system, see J.B. Biot, 'Borda', *Biographie universelle,* Vol. V (Geneva, 1812), 151-54; and G. Bigourdan, *Le système metrique des poids et mesures* (Paris, 1901).

7. G. Monge, 'Mémoire sur quelques effets d'attraction et de repulsion apparent entre les molécules de matière', *Mémoires de l'Académie Royale des Sciences* (1787), 506 ff.

8. R.J. Haüy, 'Mémoire sur la structure des spaths calcares', *Journal de physique,* Vol XX (1782), 33-48; 'Sur la double refraction dans le spath d'Islande', *Mémoires de l'Académie Royale des Sciences* (1788), 34-60.

9. E. Frankel, 'J. B. Biot and the Mathematization of Physics in Nineteenth-Century France', *Historical Studies in the Physical Sciences,* Vol. VIII (to be published).

10. *Mémoires de physique et de chimie de la Société d'Arcueil* (hereafter referred to as *Mém. Arcueil*), Vol. I (1807), 1. See also Crosland, op. cit. note 3.

11. Biot, op. cit. note 6, 154.

12. R. Fox, 'The Rise and Fall of Laplacian Physics', *Historical Studies in the Physical Sciences,* Vol IV (1974), 89-136.

13. E. Frankel, 'Jean-Baptiste Biot, The Career of a Physicist in Nineteenth Century France' (doctoral dissertation, Princeton University, 1972).

14. P.S. Laplace, *Traité de mécanique célèste,* Vol. IV, Livre X, 'Sur divers points relatives au système du monde . . . ', 1-349.

15. Ibid., 'Supplément au Livre X', 349-417; 'Supplément à la theorie de l'action capillaire', 419-98.

16. See note 12. Also R. Fox, 'The Laplacian Programme for Physics', *Boletin de la Academia Nacional de Ciencas de la Republica Argentina,* Vol. XLVIII (1970), 429-37.

17. Laplace, op. cit. note 14, 244.

18. J.B. Biot and F. Arago, 'Mémoire sur les affinités des corps pour la lumiere et particulièrement sur les forces refringentes des différents gaz', *Mémoires de la Premiere Classe de l'Institut de France* (hereafter referred to as *Mem. Inst.*), Vol. VII (1806), 301-85.

19. J.B. Biot, 'Mémoire sur l'influence de l'humidité et dj la chaleur dans les refractions atmospheriques', ibid., Vol. VIII (1807), pt. 2, 39-66.

20. J.B. Biot and L. Thénard, 'Mémoire sur l'analyse comparée de l'arragonite et du carbonate de chaux rhomboidal', *Mém. Arcueil,* Vol. II (1809), 176-206.

21. J.B. Biot, 'Recherches sur les refractions extraordinaires qui s'observent très-près de l'horizon', *Mém. Inst.,* Vol. X (1809), 1-266.

22. E.L. Malus, 'Traité d'optique analytique', *Mémoires presentés à l'Institut . . . par divers Savans* (hereafter referred to as *Mém. Savans Étrangers*), Vol. II (1811), 214-302.

23. Paris, Institut de France, *Procès-verbaux des séances de l'Académie des Sciences* (hereafter referred to as *Procès-verbaux*) (Paris, 1931), Vol. III (1804-1807), 607. (19 October 1807).

24. E.L. Malus, 'Sur la mesure du pouvoir refringent des corps opaques', *Mém. Savans Étrangers,* Vol. II (1811), 214-302.

25. W.H. Wollaston, 'A Method of Examining Refractive and Dispersive

Powers by Prismatic Reflection', *Philosophical Transactions of the Royal Society of London* (1802), 365-80.

26. Laplace, op. cit. note 14, 241.

27. E.L. Malus, 'Théorie de la double refraction', *Mém. Savans Étrangers,* Vol. II (1811), 303-508.

28. E. Frankel, 'The Search for a Corpuscular Theory of Double Refraction: Malus, Laplace and the Prize Competition of 1808', *Centaurus,* Vol. XVIII, No. 3 (1974), 223-46.

29. C. Huygens, *Traité de la lumière* (The Hague, 1690), Chapter 5.

30. W.H. Wollaston, 'On Double Refraction in Iceland Crystal', *Phil. Trans.* (1802), 381-95. Note that this paper immediately follows the one cited in note 25. Thus Malus must have been aware of Wollaston's work on double refraction when he began his work on refraction, several months before the prize competition was announced.

31. P. S. Laplace, 'Sur les mouvements de la lumière dans les milieux diaphanes', *Mém. Inst.,* Vol. X (1809), 300-42.

32. See for example J.B. Biot, *Traité de physique mathématique et experimentale,* Vol. I (1816), xvi-xvii.

33. E.L. Malus, 'Sur une propriété de la lumière reflechie par des corps diaphanes', *Mém. Arcueil,* Vol. II, 143-58.

34. Huygens, op. cit. note 29.

35. I. Newton, *Opticks* (New York: Dover edn.,1952), 360, 362.

36. (T. Young), 'Review of Laplace's Memoir . . .', *Quarterly Review* (1809), 337. Reprinted in G. Peacock (ed.), *Miscellaneous Works of the late Thomas Young, MD, FRS,* Vol. I (London, 1855), 233.

37. Newton, op. cit. note 35, Query 28, 360-62.

38. Malus, op. cit. note 27, 496.

39. Ibid. Also see E.L. Malus, 'Sur une propriété des forces repulsives qui agissent sur la lumière', *Mém. Arcueil,* Vol. II (1809), 260-61.

40. 'Mémoire sur de nouveaux phénomènes d'optique', *Mém. Inst.* (1810), pte. 2, 105-11; 'Mémoire sur les phénomènes qui accompagnent la reflexion et la refraction de lumière', ibid., 112-20.

41. F. Arago, 'Mémoire sur une modification remarquable qu'éprouvent les rayons lumineux dans leur passage à travers certain corps . . .', ibid. (1811), pte. 1, 92-134.

42. Ibid., 133.

43. J. B. Biot, 'Mémoire sur de nouveaux rapports qui existent entre la reflexion et la polarisation de la lumière par les corps cristallisés', ibid., 135-280; 'Mémoire sur un nouveau genre d'oscillations que les molécules de la lumière éprouvent en traversant certains cristaux', ibid. (1812), pte. 1, 1-371. Reprinted in J.B. Biot, *Recherches expérimentales et mathématiques sur les mouvements des molécules de la lumière autour de leur centre de gravité* (Paris, 1814), 8-152, 153-523.

44. This was his assumption in the first of the two memoirs cited above, presented to the Institut on 1 June 1812.

45. Newton, op. cit. note 35, 233.

46. See the second memoir cited in note 43, presented to the Institut in November and December of 1812 and January 1813.

47. Biot, *Recherches expérimentales,* op. cit. note 43, lxii-lxiii.

48. Ibid., lxii.

49. See as examples E. Verdet, Introduction to *Oeuvres complètes d'Augustin Fresnel* (Paris, 1866), Vol. I, xlvi-xlvii; W. Whewell, *History of the Inductive Sciences* (London, 1837), Vol. II, 116-17.

50. Newton, op. cit. note 35, 278.

51. Biot, *Recherches expérimentales,* op. cit. note 43, 230-31.

52. Newton, op. cit. note 35, 280-81.

53. Biot, op. cit. note 32, Vol. IV, 91.

54. Ibid., 245-46.

55. Ibid., 246.

56. Arago, op. cit. note 41.

57. J. B. Biot, 'Expériences sur les plaques de cristal de roche tailiés perpendiculairement à l'axe de cristallisation', *Mém. Inst.* (1812), pte. 1, 218-314. Also, Biot, *Recherches expérimentales,* op. cit. note 43, 370-466.

58. Biot, *Recherches expérimentales,* op. cit. note 43, 408.

59. J. B. Biot, 'Mémoire sur les rotations que certaines substances impriment aux axes de polarisation des rayons lumineux', *Mémoires de l'Académie des Sciences,* Vol. II (1818), 41-136.

60. Ibid., 49.

61. Newton, op. cit. note 35, 154-58.

62. In fact I have omitted a very important debate between Biot and David Brewster over the nature of double refraction. See D. Brewster, 'On the Laws of Polarization and Double Refraction in Regularly Crystallized Bodies', *Phil. Trans.* (1818), 199-273, and J.B. Biot, 'Mémoire sur les lois générales de la double refraction et de la polarisation dans les corps regulièrement cristallisés', *Memoires de l'Académie des Sciences,* Vol. III (1818), 177-384.

63. R.J. Haüy, *Traité élémentaire de physique,* Vol. II (Paris, 2nd ed., 1806), 138.

64. J.B. Biot, *Traité de physique mathématique et expérimentale,* Vol. III (Paris, 1816), 149.

65. Fox, op. cit. note 12, 107-08.

66. Ibid.

67. T. Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts,* 2 Vols (London, 1807).

68. See Peacock (ed.), op.cit. note 36, Vol. I, for a convenient collection of these memoirs.

69. H. Brougham, 'The Bakerian Lecture on the Theory of Light and Colours. By Thomas Young', *Edinburgh Review,* Vol. I (1802-03), 450-56; 'Review of "An Account of Some Cases of the Production of Colours not hitherto described" by Thomas Young', ibid., 457-60; 'The Bakerian Lecture. Experiments and Calculations relative to Physical Optics. By Thomas Young', ibid., Vol.V (1804-05), 97-103.

70. See note 36.

71. (T. Young), 'Review of Malus, Biot, Seebeck and Brewster on Light', *Quarterly Review,* Vol. XI (1814), 42-56.

72. MS letter, J.B. Biot to D. Brewster, 20 February 1816, Bibliotheque de l'Institut de France, Paris, cote 4895 #70.

73. Private communication to the author from Kurt M. Pedersen, University of Aarhus, Denmark.

74. E.L. Malus, 'Mémoire sur les phénomènes qui accompagnent la reflexion et la refraction de la lumière', *Mém. Inst.*, Vol. X (pte. 2, 1810), 120.

75. Peacock (ed.), op. cit., note 36, Vol. I, 248.

76. E.L. Malus, 'Mémoire sur l'influence des formes des molécules de la lumière dans diverses phénomènes d'optique', *Mémoires de la Société des Sciences de Strasbourg*, Vol. I (1811), 281-300.

77. F. Arago, 'Malus', *Biographies of Distinguished Scientific Men*, transl. by W.H. Smyth, B. Powell and R. Grant (Boston, 1859), Vol. II, 159.

78. F. Arago, 'Mémoire sur les couleurs des lames minces, lue à l'Institut le 18 fev. 1811', *Oeuvres complètes de Francois Arago* (Paris, 1858), Vol. X, 35.

79. F. Arago, 'Mémoire sur plusieurs nouveaux phénomènes d'optique', ibid., 97.

80. F. Arago, 'De la scintillation', ibid., Vol. VII, 97-99.

81. See Crosland, op. cit. note 3, 333, and *Procès-verbaux*, Vol. V (1811-1814), 41, 43.

82. See Crosland, op. cit. note 3, 334, for an account of this dispute. A copy of the *Mém. Inst.* (1811) at the University of Aarhus contains this note by Biot and Arago's reply, both of which have been removed from other copies.

83. For the dispute with Malus over polarization by refraction (11 March 1811) see Crosland, op. cit. note 3, 333, and the references he cites. The two memoirs in question can be found in the *Moniteur Universel*, 12 and 13 March 1811. The dispute with Brewster took place only in private correspondence, the most relevant being an MS letter D. Brewster to J.B. Biot, 9 October 1815, Bibliotheque de l'Institut de France, Paris, cote 4895 #64, and J.B. Biot, 'Extrait d'une lettre à M. Brewster, 18 Oct. 1815', MS, ibid., #65.

84. Letter, 'Augustin Fresnel à son frère Léonor', 15 mai 1814, *Oeuvres de Fresnel*, Vol. II, 819.

85. Letter, 'Augustin Fresnel à son frère Léonor, Nyons, le 5 juillet 1814', ibid., 820-22.

86. Silliman, op. cit. note 2, 41.

87. A. Fresnel, 'Première mémoire sur la diffraction', *Oeuvres*, Vol. I, 9-33.

88. F. Arago and A.M.T. Petit, 'Sur les puissances refractives et dispersives de certains liquides et des vapeurs qu'ils forment', *Annales de chimie et de physique*, Vol. I (1816), 1-9. Read to the Institut on 11 December 1815.

89. Letter, 'F. Arago à A. Fresnel, Paris le 8 novembre 1815', *Oeuvres de Fresnel*, Vol. I, 38.

90. F. Arago, 'Rapport fait a la Premiere Classe de l'Institut (le 25 mars 1816) sur un mémoire relatif aux phénomènes de la diffraction de la lumière par M. Fresnel', ibid, 86-87.

91. A. Fresnel, 'Deuxième mémoire sur la diffraction de la lumière', *Annales de chimie et de physique*, Vol. I (1816), 239 ff.; *Oeuvres*, Vol. I, 89-128.

92. Crosland, op. cit. note 3, 409.

93. *Procès-verbaux*, Vol. VI (1816-1819), passim.

94. Biot, op. cit., note 64, Vol. IV, 743-75.

95. Ibid., Pl. VII, Fig. 83; also J.B. Biot, *Précis élémentaire de physique expérimentale* (Paris, 1817), Vol. II, Pl. VII, Fig. 26. This erroneous figure was eliminated from later editions of the *Précis* and replaced by a more accurate one showing hyperbolic fringes. See *Précis* (Paris, 3rd edn., 1824), Vol. II, Pl. IX, Fig. 5.

96. *Procès-verbaux*, Vol. VI (1816-1819), 164-65.

97. Letter, 'Léonor Merimée à son neveu A. Fresnel, Paris le 16 mars 1817', *Oeuvres de Fresnel*, Vol. II, 841.

98. A. Fresnel, 'Mémoire sur la diffraction de la lumière couronné par l'Académie des Sciences', *Oeuvres*, Vol. I, 245-382.

99. *Procès-verbaux*, Vol. VI, 345, 427, 432.

100. The archives of the *Académie des Sciences* have no manuscripts relating to this contest whatsoever.

101. Haüy, *Traité de physique* (Paris, 3rd edn.,1821), Vol. II, 380-85; Biot, *Précis de physique* (Paris, 3rd edn., 1824), Vol. II, 463-73. The second edition of the *Précis* (1821) also contains material on the wave theory of diffraction.

102. Biot, ibid., 472-73.

103. Young, op. cit. note 36, 233.

104. Peacock (ed.), op. cit. note 36, Vol. I, 360.

105. Ibid., 266-73. See also op. cit. note 71.

106. A. Fresnel, 'Mémoire sur l'influence de la polarisation dans l'action que les rayons lumineux exercent les uns sur les autres', *Oeuvres*, Vol. I, 387. (First draft of this memoir, 'signée et datée du 30 aout 1816', 385, note.)

107. Ibid., 388.

108. Ibid., subsequent draft, 'presentée à l'Institut le 7 octobre 1816', 410-39.

109. Fresnel, op. cit. note 106, 437.

110. Letter, 'A. Fresnel à son frère Léonor, Paris, le 5 septembre 1818', *Oeuvres*, Vol. II, 849.

111. Biot, 'Mémoire sur les rotations', *Mém. Inst.*, Vol. VI (1818), 126-36.

112. See Biot, op. cit. note 64, Vol. IV, 419, 426 for attempts.

113. F. Arago, 'Rapport fait à l'Académie des Sciences le lundi 4 juin 1821 sur un mémoire de M. Fresnel. . . .', *Oeuvres de Fresnel*, Vol. I, 553-69.

114. Ibid., 553-54.

115. Ibid., 556.

116. A. Fresnel, 'Notes sur la polarisation mobile', ibid., 542-44.

117. *Procès-verbaux*, Vol. VII, 201.

118. J. B. Biot, 'Remarques sur un rapport de M. Arago lu à l'Académie des Sciences le 4 juin 1821', *Oeuvres de Fresnel*, Vol. I, 569-90.

119. Ibid, 590.

120. F. Arago, 'Examen des remarques de M. Biot', ibid., 595-96.

121. Biot, op. cit. note 118, 583.

122. Ibid., 587.

123. Ibid., 580.

124. Ibid., 588.

125. J.B. Biot, 'Mémoire sur un nouveau genre d'oscillations que les molécules de la lumière éprouvent en traversant certains cristaux', *Mém. Inst.* (1812), pte.1, 1-371.

126. J.B. Biot, 'Sur une loi remarquable qui s'observe dans les oscillations des particules lumineuses', *Mém. Arcueil*, Vol. III (1817), 132-47.

127. Op. cit. note 120, 591-601.

128. A. Fresnel, 'Note sur les remarques de M. Biot', *Oeuvres*, Vol. I, 601-08; 'Note sur le calcul des teintes que la polarisation developpe dans les lames cristallisées', ibid., 609-29.

129. Arago's report, *Annales de chimie et de physique*, Vol. XVII (cahier

de mai, 1821), 80 ff.; Biot's 'remarks', ibid., Vol. XVII (cahier de juillet 1821), 225 ff.; Arago, 'Examen', ibid., 258 ff.; Fresnel, 'Note sur les remarques de Biot', ibid., Vol. XVII (cahier d'aout 1821), 393 ff.

130. A. Fresnel, 'Considerations mécaniques sur la polarisation de la lumière', *Oeuvres,* Vol. I, 629-49; *Annales de chimie et de physique,* Vol. XVII (cahier de juin 1821), 167 ff.

131. Silliman, op. cit. note 2, 221.

132. This work is contained in a series of memoirs, abstracts and supplements 'sur la double refraction', reprinted in *Oeuvres de Fresnel,* Vol. II, 261-596, and presented to the Académie des Sciences 19 and 26 November 1821, 22 January 1822, and 22 April 1822.

133. Verdet, 'Introduction', *Oeuvres de Fresnel,* Vol. I, lxxxvi-lxxxvii.

134. A. Fresnel, 'De la lumière', *Oeuvres,* Vol. II, 3-142.

135. Fresnel, 'Considerations mécaniques', op. cit. note 130, 629-39.

136. Verdet, op. cit. note 133, lv, note 2.

137. In a note to the article 'Polarization' in the supplement to the *Encyclopedia Britannica* (Edinburgh, 1827), 279. Cited in E. Mach, *The Principles of Physical Optics* (New York: Dover edn. 1956), 201.

138. See *Oeuvres de Fresnel,* Vol. II, 459, note (b). Poisson did not sign the report on this memoir.

139. A. Fresnel, 'Note sur les accès de facile reflexion et de facile transmission des molécules lumineuses dans le système de l'émission, lue a la société Philomathique', *Oeuvres,* Vol. II, 147-65.

140. S. D. Poisson, 'Extrait d'un mémoire sur la propagation du mouvement dans les fluides élastiques', *Annales de chimie et de physique,* Vol. XXII, 250 ff. Reprinted in *Oeuvres de Fresnel,* Vol. II, 192-206. 'Extrait d'une lettre de M. Poisson à M.A. Fresnel', *Annales,* Vol. XXII, 270 ff.; *Oeuvres,* Vol. II, 206-13.

141. Poisson, 'Extrait d'un mémoire . . .', *Oeuvres de Fresnel,* Vol. II, 195.

142. A. Fresnel, 'Response à la lettre de M. Poisson', *Oeuvres,* Vol. II, 235-36.

143. F. Lefort, 'Un savant chrétien, J.B. Biot', *Le Correspondant, Nouvelle séries,* Vol. XXXVI (1867), 978-79.

144. See op. cit. note 12.

145. See R. Fox, 'The Background to the Discovery of Dulong and Petit's Law', *British Journal for the History of Science,* Vol. IV, Pt. 1, No. 13 (June 1968), 1-23.

146. See op. cit. note 13, 330-47. Also see L.P. Williams, 'Ampère's Electrodynamic Molecular Model', *Contemporary Physics,* Vol. IV (1962-1963), 113-23, especially the quotation from Ampère on page 119. For Ampère's memoirs see *Collection de Mémoires Relatifs a la Physique,* op. cit. note 5, Vols. II and III, *Mémoires sur l'Électrodynamique.*

147. J.B. Biot, 'Sur l'aimantation imprimée aux metaux par l'electricité en mouvement', *Journal des savans* (1821), 221-35. This memoir is discussed in op. cit. note 13, 341-42.

148. *Procès-verbaux,* Vol. VII (1820-1823), 394.

149. Arago, 'History of My Youth', *Biographies,* op. cit. note 77, Vol. I, 89.

150. G. Libri, 'Les sciences en France', *Revue des deux mondes* (1840), 797.

151. Whewell wrote of the debate between Biot and Arago, ' . . . this may be considered as the last struggle in favour of the theory of emission among mathematicians of eminence'. See W. Whewell, *History of the Inductive Sciences*

(London, 1837), Vol. II, 116-17.

152. C.S.M. Pouillet, *Elements de physique expérimentale et de météorologie* (Paris, 1830), Vol. II, pte.2, 403.

153. M. Grosselin, *Lecons de physique de la Faculté des Sciences* (Paris, 1828), II^{e} partie, Pouillet.

154. M. L. Am. Sedillot, *Les professeurs de mathématique et de physique générale au Collège de France* (Rome, 1869), 200.

155. *Programme de l'enseignement de l'École Royale Polytechnique, pour l'année scolaire 1821-1822* (Paris, n.d.), 39.

156. Verdet, op. cit. note 133, lxxxix, note 2.

157. See *Oeuvres de Fresnel,* Vol. II, 850-51; note by Léonor Fresnel on a conversation with Biot, 9 March 1846.

158. See J.B. Biot, *Mélanges scientifiques et littéraires,* Vol. I (Paris, 1858), 155.

159. E.G. Fischer, *Physique mécanique,* trans. J.B. Biot (Paris, 3rd edn.,1830), 373.

160. Kuhn, op. cit. note 1., 82.

161. Ibid., 79.

162. Fox, op. cit. note 12, 130.