Note on the remarks of Mr. Biot relating to colors of thin plates

by Augustin Fresnel
(published Aug. 1821)

with notes and analytical table of contents
by the editors of Fresnel's *Oeuvres complètes*
1866–70

Translated by Gavin R. Putland *
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English translation of A. Fresnel, "Note sur les remarques de M. Biot, publiées dans le cahier précédent", *Annales de Chimie et de Physique*, Ser. 2, vol. 17, pp. 393–403 (August 1821), as reprinted in *Oeuvres complètes d'Augustin Fresnel*, vol. 1 (1866), pp. 601–608, with the corresponding extract from the "Table Analytique" in *Oeuvres complètes...*, vol. 3 (1870), at pp. 580–81.

In a chromatic-polarization experiment with the principal section of the calcite analyzer in the initial "plane of polarization", and the axis of the "crystalline plate" at an angle thereto, the observed color of the extraordinary image as a function of path difference should, according to Fresnel's theory, match that of Newton's rings in reflected light. Biot agrees with the match, but contends, using Newton's methodology, that the colors predicted by Fresnel change too smoothly and are insufficiently saturated. Fresnel dismantles this argument, noting that:

- Contrary to Biot, Newton's "empirical formula" [using his color circle to find the resultant of a combination of spectral colors] is not, and does not purport to be, exact.
- The colors named in "Newton's Table" are not unambiguous, and moreover the mismatch alleged by Biot usually does not extend to the names, but involves only the white content (loss of saturation), for which Newton's "empirical formula" seems particularly unreliable, and which Biot has not measured anyway.
- (§2) Newton's diagram showing what prismatic colors are present for a given thickness of air [treating each color as simply present or absent] is not, and does not purport to be, exact.
- The approximation embodied in this diagram—that the rings in monochromatic light alternate *sharply* between bright and dark bands, corresponding to equal ranges of thickness—is easily debunked by inspecting the rings with a loupe under a sufficiently bright light.
- (§3) Similarly, in the analogous chromatic-polarization configuration, the extraordinary image is not fully dark at the extremes of the path differences corresponding to the dark rings, but just as bright as the ordinary image, as Fresnel's formulae predict.

(§2, cont.) The confirmation of Fresnel's interference formulae through the location of diffraction minima is more thorough than it may seem, because each minimum is not simply a cancellation of two wavetrains, but the convergence of an infinitude of secondary wavetrains with all possible degrees of reinforcement and cancellation.

Fresnel acknowledges his debt to Biot's experimental work (and Young's priority in discovering that the colors of crystalline plates depend on the path difference between the ordinary and extraordinary waves). He acknowledges the relation between Biot's formulae and his own in the case of a single plate. But he notes that his own formulae are not deducible from Biot's, and that his own theory—unlike Biot's—extends to more complicated cases without additional assumptions.

— *Translator*.

* Melbourne, Australia. Gmail address: grputland. This paper—one of a series in which Fresnel reconstructed physical optics on the transverse-wave hypothesis between May 1821 and January 1823—has been translated in time for its bicentenary, without institutional sponsorship. My opportunity to translate the whole series depends on sponsorship.

Translator's preface

This paper was first published in Arago's *Annales* under a title that may be translated "Note on the remarks of Mr. Biot, published in the preceding issue". The last phrase was understandably omitted from the title of the reprint in Fresnel's *Oeuvres complètes* [10]. In the bibliographies of Buchwald [4, p.462] and Darrigol [7, p. 300], this phrase is replaced with a longer one that may be translated "relative to the phenomena of colors produced by thin plates", for which no source is given. The title that I have used for the present edition is a compromise between the *Oeuvres complètes* and Buchwald/Darrigol.

Context: When plane-polarized white light is normally incident on a birefringent "crystalline plate" whose "axis" (fast or slow) makes an angle i with the initial "plane of polarization" (in modern terms, the plane *normal* to the electric displacement), each incident wavetrain is resolved into perpendicularly polarized components ("ordinary" and "extraordinary"), which propagate at different speeds through the plate. Hence the polarization of the emergent light is generally elliptical and depends on i and the thickness of the plate, and on the wavelength ("chromatic polarization"), causing colors to appear when the emergent light is viewed through an analyzer ("colors of crystalline plates"). If the chosen analyzer is a "rhomb" of calcite whose principal section makes an angle (azimuth) s with the initial plane of polarization, the observed image of the plate is split into an *ordinary* image whose plane of polarization is at azimuth s, and an *extraordinary* image whose plane of polarization is at azimuth $s + 90°$. Thus, if $s = 0$, the extraordinary image corresponds to what we now call "crossed polarizers".

PARIS, 1821: Jean-Baptiste Biot, the recognized authority on chromatic polarization and the most active proponent of the established corpuscular/emission theory of light, has found empirically, as long ago as 1812, that the colors of the ordinary and extraordinary images are well described by the formulae

$$I_o = U \cos^2 s + A \cos^2(2i - s)\,, \tag{1}$$

$$I_e = U \sin^2 s + A \sin^2(2i - s)\,, \tag{2}$$

where A (for "affected") is the color of Newton's reflection ring for an air thickness proportional to (but much greater than) the plate thickness, and U (for "unaffected") is the complementary color, and both may be understood, for present purposes, as spectral densities.[1] (Some symbols have been changed for clarity or continuity.) Biot interprets these formulae in terms of his theory of "mobile polarization". In the simplest version of this theory, the rays of each spectral color oscillate between (if I may adapt Newton's terms) *fits of polarization* at the initial azimuth 0, and fits of polarization at the alternative azimuth $2i$, the lengths of these fits being proportional to those of Newton's fits of easy refraction and reflection—so that, as the plate thickness is increased, the composite color whose rays are at azimuth $2i$ (seemingly *affected* by the plate) passes through the same sequence as Newton's rings in reflected light [8, pp. 148–50], while the composite color whose rays are at azimuth 0 (seemingly *unaffected* by the plate) passes through the complementary sequence. In the analyzing crystal with its principal section at azimuth s, the two colors are weighted according to Malus's law; and *provided* that they do not interfere with each other (!), their intensities at any s are simply additive—in accordance with the above formulae.

Fresnel, the recent discoverer of some laws of interference of polarized light [1], has derived rival formulae from a wave-based theory—not only the mature version [9] of 1821, which Biot has only recently seen [3, p.589], but also earlier versions dating back to 1816 and summarized in Arago's report of 4 June 1821 [10, vol. 1, pp. 553–68]. Biot in turn has noticed [10, vol. 1, pp. 537,575–6] that Fresnel's formulae are obtainable from his own through trigonometric identities and the substitutions

$$U = \cos^2\left(\frac{\pi d}{\lambda}\right), \quad A = \sin^2\left(\frac{\pi d}{\lambda}\right), \tag{3}$$

where λ is the wavelength, and d (my abbreviation) is the optical-path-length difference between the ordinary and extraordinary waves in the plate and is consequently proportional to the thickness of the

[1] I follow Buchwald [4, pp. 92–7] and Darrigol [7, pp. 194–5] in using U for *unaffected* and A for *affected*. Biot confusingly uses O for *ordinary* and E for *extraordinary*, respectively, because he initially thought that only certain spectral colors suffered the extraordinary refraction [8, p. 148]; but further observations forced him to abandon his first impression.

plate. The substitutions are not *explained by* Biot's theory; but they yield Fresnel's formulae therefrom, provided that I_o and I_e then become *relative* intensities as functions of wavelength (or fit-length). In the interesting special case for which $s = 0$ and $i \neq 0$, these formulae reduce to

$$I_o = U + A\cos^2 2i\,, \qquad I_e = A\sin^2 2i \propto A = \sin^2\left(\frac{\pi d}{\lambda}\right), \tag{4}$$

so that the intensity I_e of the extraordinary image is proportional to that of Newton's reflection ring (at normal incidence) for air thickness d, as given by Fresnel's interference formula for sinusoidal waves.

Biot rejects not only Fresnel's underlying theory, but also (preferably) the formulae derived from it, and therefore needs the substitutions for U and A to be *wrong* (although he discovered them himself). In particular he claims [3, p. 578*n*] that the function $\sin^2(\pi d/\lambda)$ varies too smoothly and consequently predicts too little saturation in the colors of the reflection rings in "Newton's Table" [12, p. 233], as obtained by using Newton's construction [12, p. 227] to determine the prismatic colors present for the given air thickness, then finding their resultant composite color from his "empirical formula" involving his color circle [12, pp. 154–8]. In short, Biot has tried to defend his theory of "mobile polarization" by firing an extremely late and extremely optimistic shot at Fresnel's explanation of Newton's rings.

The present note is Fresnel's reply—which, in combination with his mature paper on chromatic polarization [9] and Arago's report on Fresnel's earlier work, reduced Biot to silence on the subject. This episode, though less well known than the earlier showdown over diffraction, was far more consequential for the fortunes of the wave theory of light, and heralded a changing of the guard at the French Academy of Sciences.[2]

Conventions: Footnotes to the text of this translation (but not footnotes to footnotes) are numbered sequentially. Immediately after their sequential numbers, footnotes by Fresnel are further identified by their *original numbers* in their original parentheses, and footnotes by the editors of the *Oeuvres complètes* are further identified by their *original letters* in their original parentheses. Section numbers, in the text and in the analytical table of contents, are from the *Oeuvres*. Footnotes identified by sequential numbers alone, together with all items in *square brackets* (in the analytical table or the main text or the footnotes, and including citations such as "[12, p. 233]"), are mine. Some of my notes are signed *Translator* for added clarity, although their origin is already implied. The final edition of Newton's *Opticks* was known to Fresnel through the 1787 French translation [13] by Jean-Paul Marat (yes, him). Where this work is quoted, I have substituted the original English for Marat's French.

— *Translator*.

Analytical Table of Contents

[2] Further reading: Buchwald [4], pp. 88–103, 214–18, 226–51, 368–9, 384–6, 395–7, 431–2, 440, 442–4; Buchwald [5]; Darrigol [7], pp. 193–8, 206–10; Frankel [8], pp. 147–51, 163–8.

1. For the purpose of judging the accuracy of the intensity formulae that I have deduced from the principle of interference, Mr. Biot [3] has applied them to various cases in Newton's Table [12, p. 233], which relates to the tints[3] of the reflected rings ["Newton's rings"]. But this verification itself rests on two hypotheses: the perfect exactitude of Newton's Table and that of the empirical formula which he has given for calculating the tint resulting from any mixture of colored rays [12, pp. 154–8]. Now, in the first place I do not know that we have done the series of numerous and methodical experiments that would be needed to demonstrate the rigorous correctness of this formula, and in particular to show that it well represents the proportions of white light; to me this seems improbable. Certain colors, such as those of various flowers, in which one finds with the prism a notable quantity of heterogeneous rays, often appear to us as vivid and as pure as the most simplified [monochromatic] rays of the solar spectrum. There are some composite colors, such as rose and purple, which produce in the eye sensations of which we cannot find equivalents in the simple rays of the spectrum. Yet Newton's empirical construction assumes this equivalence.[4] We should therefore regard it as only a rough representation of the varied sensations by which we perceive the diverse combination of heterogeneous rays; and when it indicates a high proportion of white light, we need not always conclude that the composed color is pale and lifeless.[5] So to me it does not seem safe to use this construction for a final judgment on the correctness of a formula for the intensities of simple light, especially when relying on a table whose perfect accuracy has not yet been demonstrated, and whose expressions can be differently interpreted by different observers according to their manner of perceiving and naming the colors.[6]

It is possible that Newton's Table is not very accurate for the first ring, and especially near the [central] black spot, where the slightest flexure of the glass may lead to error in the thickness of the layer of air as judged by distance from the center. So the part of the layer of air that Newton has called the beginning of black, for which he has supposed a thickness of 2 millionths of an English inch according to the measure of the diameter, may have been somewhat thinner. Besides, nothing proves that what

[3] French *teintes*, which in this instance could almost be rendered as *hues* rather than *tints*, because Newton's table does not recognize degrees of saturation of the colors except for the last, which he calls "Ruddy White".

[4] In modern terms, Newton's chromaticity diagram [12, p. 155, Fig. 11] collapses the line of purples to a single point, and therefore treats all colors on this line as being of the same hue, although we perceive them as having a wide range of hues (all with full saturation).

[5, (1)] Newton himself says, on p. 153 of the first volume of the French translation of his treatise on optics, that *the compounded violet is more bright and more fiery than the uncompounded* [Newton's words [12, p. 156], relegated to a footnote in Marat's translation]; and yet, according to the construction, the one containing a little white light should present, on the contrary, a less lively tint than the other.

Newton continues on the same page: "Also if only two of the primary Colours which in the circle are opposite to one another be mixed in an equal proportion, the point Z shall fall upon the center O, and yet the Colour compounded of those two shall not be perfectly white, but some faint anonymous Colour. For I could never yet by mixing only two primary Colours produce a *perfect white*" [Fresnel's emphasis]. Now this white should be perfect if Newton's rule were rigorous; so he presents these facts as exceptions to his rule, which he did not believe to be exact, since he says on p. 155 [12, p. 158], "This Rule I conceive accurate enough for practice, though not mathematically accurate".

Mr. Biot expresses himself differently on the same subject, at the end of p. 454 of vol. 3 of his *Traité de physique* [2]: "One must therefore beware", he says, "of confusing this law given by Newton with an empirical hypothesis; it must be considered a *true law* drawn from experiment" [Fresnel's emphasis]. It is quite remarkable that Mr. Biot should have a higher opinion of the accuracy of Newton's rule than Newton himself had. Mr. Biot is more severe with respect to my formula, and believes it to be false although I have presented it as rigorous; but I am persuaded that, when he has given himself the time to consider it further, he will realize that he has judged it too fast and too unfavorably.

[6, (2)] I have often had occasion to observe that a very skillful painter, who certainly knows colors well and knows how to distinguish their most delicate nuances,[(a)] in many cases does not give them quite the same names as Mr. Biot. From this I am far from concluding that Mr. Biot is wrong; I can only point out that two persons may give different names to the same tints and the same names to different tints, and that therefore it is not by names that we can be sure of their identity, but only by direct comparison of tints placed side-by-side; and even then we judge only the identity of sensation and not that of composition. [This footnote, by Fresnel, is numbered (1) in the original printing and (2) in the *Oeuvres complètes*, due to different pagination. Again the names of the tints [*teintes*] would tend to refer to hue rather than saturation.]

[(a)] Léonor Mérimée, Fresnel's uncle. (See his treatise *Of Painting in Oil* [11], ch. 7.)

Newton calls *the beginning of the black spot*[7] reflects a light much fainter than a third of that of the white of the first order; for he further distinguishes *black* and *very black*.

I have redone the calculation of Mr. Biot for this case only, and found that the sum of the diverse rays, taken in their color proportions given by Newton's empirical formula,[8] was a little more than a third of the same sum calculated for the thickness that reflects the white of the first order;[9] but in comparing the green, yellow, and orange rays, which are much brighter than the others and have much more influence as illuminating rays, I found that their sum in the first case was less than a third of their sum in the second. Now this difference in intensity is already considerable. It has often been noticeable, by looking at the letters of a book through a rhomb of calcite, how the mere halving of the light on a point of an illuminated space renders the point dark by comparison with surrounding parts.[10]

I shall not pause to discuss the other cases in which Mr. Biot has compared my formula with Newton's Table. It seems to me that they do even less than the first case to disprove my formula; for the colors that he deduces from it are the same, at least in name, as those of Newton's Table, since Mr. Biot finds *red* when it says *red*, and *violet* when it says *violet*; and the discrepancies that he thinks he perceives involve only the proportions of white light, which he has not measured. Thus, even if we consider both Newton's Table and his empirical rule for mixing colored rays as rigorously exact, we find nothing that really proves my formula to be at fault, at least in the particular cases chosen by Mr. Biot. This learned gentleman compares the results of my formula with those of the construction indicated by Newton for determining the simple rays that enter into the tints of the reflected rings;[11] and because my formula does not give the same proportion of white light, he concludes that it is false. By this way of reasoning it was unnecessary to do all the calculations, and it was enough to say: Mr. Fresnel's formula does not coincide with Newton's construction, so it is false.

2. It is all the more permissible not to accept this argument, since the construction of Newton [12, p. 227], which that great geometer did not suppose to be rigorous [12, p. 228], being founded (as Mr. Biot himself observes) on the hypothesis that the rings completely dark under homogeneous light have the same width [far from the center] as those that partly reflect it,[12] is in manifest contradiction with the facts. To convince ourselves of this, it suffices to use a brilliant light and, having simplified [monochromatized] it by means of a prism or a red glass, to let it fall on a prism in contact with a slightly convex lens, of which we have blackened the lower surface to suppress the second reflection; the two upper faces of the prism must make an angle the more obtuse as we want to observe the rings the closer to perpendicular incidence. By virtue of this angle, the eye receives only the rays reflected at the second surface of the prism and at the first surface of the convex lens—that is, only those that concur in the formation of the rings. Now, by observing them with a loupe, one recognizes that the parts of the dark rings that present a near-total absence of light, appearing of a uniform black, are much narrower than the illuminated parts, even in the rings of the first, second, and third orders, where the imperfect homogeneity of the light has very little effect. In this experiment it is helpful to use the light of white clouds strongly illuminated by the sun, or solar rays introduced into a dark room. The latter process would need to be adopted if we wanted to compare accurately the intensities of a sensibly homogeneous light at the various points of the

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[7] Apparently not a verbatim quote.

[8] I take this to mean that wavelengths representing the seven principal colors are initially weighted in proportion to their sectors in Newton's color circle [12, p. 155], this combination giving white light according to Newton's "empirical formula", and then further weighted by Fresnel's interference factors for Newton's stated thickness. —*Translator*.

[9] As Biot claims [3, p. 578*n*]. He adds that in this case, Fresnel's interference formula together with Newton's color circle gives a bluish white, whereas experiment and Newton's table give, if anything, an extreme violet. So, in this case, Biot implies that Fresnel makes an error in hue.

[10] If a dark letter is seen double against a light background, each image of the letter is "fogged" by an image of the background and is therefore at least half as bright as the background—but still looks dark.

[11] Newton's "Fig. 6" [12, p. 227], which tries to show what spectral colors are present in the reflected light for a given thickness of air [12, pp. 229–30], but fails to capture the continuous variation in amplitude with thickness—as Newton himself admits [p. 228].

[12] Because Newton's fits of easy refraction are supposed to be of the same length as his fits of easy reflection for the same spectral color.

dark and bright rings. I am confident that we would then find results consistent with my formula, at least for the rings of the first two orders.

This confidence is based on the numerous and varied tests to which I have subjected the same interference calculations in my experiments on diffraction; for, in determining the positions of the dark and bright fringes, I have not only verified the formula for the extreme cases of complete agreement and disagreement—as if, for example, I had only calculated the *maxima* and *minima* of the fringes produced by two mirrors, where there are only two interfering wavetrains—because in the phenomena of diffraction properly so called, the *minima* are produced by the reunion of an infinitude of elementary wavetrains that find themselves in all possible degrees of agreement and disagreement; and if the interference calculation that gives the intensity of their total resultant were not correct for all these degrees, I should have sometimes found notable differences between theory and observation concerning the positions of the *minima*. It is true that I was thereby verifying formulae deduced from both the calculation of interference, which suffices for the colored rings, and Huygens' principle, which is needed to explain the phenomena of diffraction; and it will perhaps be alleged that the falsity of this principle combined with the falsity of my interference calculations could have led me, by a happy accident, to consistently accurate results! That is why I propose to verify the interference formulae separately on the reflected rings, as soon as my occupations permit, and then to compare the intensities of the different points of the diffraction fringes, to complete the experimental demonstration of Huygens' principle.[13]

3. In the mean time, I point out that the intensity formulae deduced from the principle of interference have been directly verified not only in the extreme cases of *maximum* and *minimum*, but also in the intermediate cases where the two wavetrains differ by a quarter-cycle, or in general by an odd whole number of quarter-cycles; for we then find by experiment on the crystalline plates, in turning their principal section into the azimuth of 45°, that the two images are always of equal intensity in accordance with the calculation. By this alone, the exactness of my formulae would already be as likely as that of Malus's law, which so far has been rigorously verified only for the extreme angles 0° and 90°, and for the intermediate angles of 45°.

Also like Malus's law, they satisfy the condition that the sum of the intensities of the two images remains constant. As they agree with experiment on all these points, it is implausible that they could be as false as Mr. Biot supposes.

I further note that the experimental result that I have just mentioned is entirely opposed to the idea, proposed by this learned physicist, concerning the relative intensities of the light on the various points of the reflected rings. For, if the thickness corresponding to the limit of one perfectly dark ring in homogeneous light were the mean of those corresponding to the middle of the dark ring and the middle of the bright ring, it would follow, according to the analogy that Mr. Biot himself established between these phenomena and those of crystalline plates, that the thickness of plate midway between that which produces complete polarization in azimuth $2i$ [where $i = 45°$ as above], and that which presents complete polarization in the initial plane, should no longer give any noticeable light in the extraordinary image; but it is precisely in this case that the two images are of equal intensity [9, §5].

4. Mr. Biot recalls a conversation in which he explained to me how the formulae that had led him into error on the tints produced by two plates of equal thickness crossed at 45° were not a necessary consequence of the theory of mobile polarization. I confess that I did not very well understand what he did me the honor of telling me on this subject, and that I still do not divine how this learned physicist can *deduce from his theory* the general formulae for the case where the axes make between them an arbitrary angle. But I have never cited the error into which he had been led by his first formulae, and *of which I was warned by mine*, as a decisive proof of the inaccuracy of his theory;[14] I only wanted to show by this example that I had chosen a better guide than his: and it seems to me that he did not deny this in the

[13,(a)] It seems that Fresnel was never able to follow up on this project.

[14] In his definitive paper on chromatic polarization [9], written shortly before the present note, Fresnel explains Biot's error and attributes it to Biot's *formulae* [9, §9]. But he introduces the exposition by noting that Biot's *theory* yields these formulae only in particular cases, implicitly leaving open the question whether his theory yields the offending formulae in the case under discussion. Thus the example shows that Biot's *theory* fails to predict the facts, but not necessarily that it makes counterfactual predictions; the distinction is perhaps subtle, but critical. —*Translator*.

conversation that he cites, for he told me that the theory that I had adopted "took the phenomena from higher, and carried them further".[15]

5. In concluding this note, I again acknowledge the assistance that I found in the works of Mr. Biot as I busied myself with the coloration of crystalline plates. His formulae helped me to recognize easily, without recourse to experiment, in what cases the tints became white, and in what cases they reached their *maximum* saturation [*intensité*], and indicated to me the image [ordinary or extraordinary] for which we must add a half-cycle to the path difference of the two wavetrains—a rule that I could also deduce from my experiment on the two rhombs.[16] But this is all that I have borrowed from Mr. Biot;[17] and one will easily sense that, despite the relation that he notices between my formulae and his in the case of a single plate, mine differ too much in their foundation to have been deduced from his, since mine give the intensity of each species of ray [wavelength], while his simply refer to Newton's Table, as he himself remarks. But it is chiefly when the superposition of several plates complicates the phenomenon that we find a large difference in usefulness between the two theories. In the one that I have adopted, the laws of the most complicated phenomena are *necessary* consequences of the same principles that have served to calculate the tints of a single plate, whereas Mr. Biot is obliged to make new assumptions to *reconnect the oscillations of the luminous molecules* from one plate to the next: it is here especially that the complication and the multiplicity of his hypotheses make his theory so improbable. If we add to the fits of the luminous molecules their axes of polarization, the oscillations of their axes, and all the physical properties that they must acquire inside the crystals and carry with them to restart their oscillations in a second crystal, sometimes at one depth and sometimes at another, it will be hard to conceive how so many different modifications can be combined in the same molecule.

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[15] French: *prenait les phénomènes de plus haut, et les conduisait plus loin*. The earlier parts of this paragraph have also been translated by Buchwald [4, p. 444, note 11].

[16] This experiment, described with cryptic brevity in a note by Fresnel [10, vol. 1, pp. 538–41, at p. 540] supplementary to Arago's report [10, vol. 1, pp. 553–68], seems to be the "loupe" version of the experiment described in more detail in §17 of that report; *cf.* [4, pp. 241–2]. The "crystalline plate" of the standard chromatic-polarization experiment is replaced by two stacked calcite rhombs of equal thickness, with their principal sections perpendicular to each other. The incident light is taken from a point-source, and the emergent light is viewed through a loupe. In the focal plane of the loupe, one sees an aerial two-source interference pattern in which the color at each point matches that of the standard chromatic-polarization configuration with the same angles and path difference. If the central stripe is dark, one of the interfering wavetrains is inverted: that is, "we must add a half-cycle to the path difference" when calculating the interference. — *Translator*.

[17,(1)] I should add that it was by the precise measurements of this celebrated physicist that I [Fresnel] assured myself that the tints of crystalline plates held to the path difference of the ordinary and extraordinary rays that traversed them. This idea came to me immediately as I began to occupy myself with these phenomena, without then knowing of the note published by Dr. Young on this subject several years earlier [*Quarterly Review*, London, April 1814, reprinted in [14] at pp. 266–72]. Mr. Arago had not yet spoken of it to me, when I communicated to him the result of my calculation for the particular case of perpendicular incidence. I say this not to claim any part of the honor for the discovery—which belongs entirely to Dr. Young—but to give a sense of how easy it was, with the wave theory, to discover this intimate relation between the colored rings and the tints of crystalline plates [through path difference], which had escaped the sagacity of Mr. Biot guided by the emission theory.

References

[1] D.F.J. Arago & A. Fresnel, "Mémoire sur l'action que les rayons de lumière polarisés exercent les uns sur les autres", *Annales de Chimie et de Physique*, Ser. 2, vol. 10, pp. 288–305, March 1819; reprinted in [10], vol. 1, pp. 509–22; translated as "On the action of rays of polarized light upon each other" in Crew [6], pp. 145–55. (The title in [10] changes "polarisés" to "polarisée", which is followed in [6].)

[2] J.-B. Biot, *Traité de physique expérimentale et mathématique*, Tome III, Paris: Deterville, 1816; books.google.com/books?id=k5hW9C4g3UYC.

[3] J.-B. Biot, "Remarques de M. Biot sur un rapport lu, le 4 juin 1821, à l'Académie des Sciences par MM. Arago et Ampère", *Annales de Chimie et de Physique*, Ser. 2, vol. 17, pp. 225–58, July 1821; reprinted in [10], vol. 1, pp. 569–90. Cited page numbers are from the reprint.

[4] J. Z. Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century*, University of Chicago Press, 1989.

[5] J. Z. Buchwald, "The battle between Arago and Biot over Fresnel", *Journal of Optics*, vol. 20, no. 3 (May 1989), pp. 109–17.

[6] H. Crew (ed.), *The Wave Theory of Light: Memoirs by Huygens, Young and Fresnel* [Vol. X in J.S. Ames (ed.), Scientific Memoirs], American Book Co., 1900; archive.org/details/wavetheoryoflighOOcrewrich.

[7] O. Darrigol, *A History of Optics: From Greek Antiquity to the Nineteenth Century*, Oxford, 2012.

[8] E. Frankel, "Corpuscular optics and the wave theory of light: The science and politics of a revolution in physics", *Social Studies of Science*, vol. 6, no. 2 (May 1976), pp. 141–84; jstor.org/stable/284930.

[9] A. Fresnel, "Note sur le calcul des teintes que la polarisation développe dans les lames cristallisées" et seq., *Annales de Chimie et de Physique*, Ser. 2, vol. 17, pp. 102–11 (May 1821), 167–96 (June 1821), 312–15 ("Postscript", July 1821); reprinted (adding section nos.) in [10], vol. 1, pp. 609–48; translated by G.R. Putland as "On the calculation of the tints that polarization develops in crystalline plates, & postscript", doi.org/10.5281/zenodo.4058004 (2021).

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