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XXV. *Remarks on Mr. Potter's Experiment on Interference.*
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To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the last Number of your Journal is a paper by Mr. Potter upon certain phænomena of Interference, which he considers to be inexplicable on the theory of undulations. These phænomena are, in fact, a confirmation of the truth of that theory, and might have been predicted from the theory. I should not therefore have troubled you with these remarks, if I did not feel that the public is much interested in seeing a distinct and correct interpretation put upon experiments and calculations like those to which I allude; and if I were not convinced that Mr. Potter, whose labours as an experimentalist I value most highly, would feel any thing but pain at my pointing out the error into which (I conceive) he has fallen as a theorist.

In Mr. Potter's experiment two pencils of light originating from a common source are made to interfere by falling upon two plane mirrors inclined at a small angle: the two reflected pencils fall upon a prism whose edge is parallel to the line of junction of the mirrors, and the interference-fringes, after emergence, are examined by means of an eye-glass in the usual manner. The light being supposed homogeneous, Mr. Potter finds (correctly) from the theory of undulations that the points, at which the paths of the pencils from the two images are equal, are almost exactly at the centre of the mixture

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of the lights: he infers that the centre of the mixture of lights ought to be the centre of the interference-fringes; instead of which it appears in experiment that on removing the eye and eye-glass further and further from the prism, the interference-fringes deviate from the centre of the mixed light more and more towards the thick side of the prism, and finally disappear. And this discordance is the point now to be explained.

Suppose now that the light is *not* homogeneous (the reasons for believing that this supposition is true will be given presently); and suppose only that the proportion of the differences of the lengths of the waves, or if you please, of the lengths of the fits, for all colours, from the length corresponding to some particular colour, is the same as the proportion of the differences of the deviation for those colours (produced by the prism of the experiment) from the deviation corresponding to that particular colour. This assumption is very nearly true if the light comprehends rays from no more than one half or one third of the spectrum. Let us now inquire what will be the position of the fringes.

Any one of the kinds of homogeneous light composing the incident heterogeneous light will produce a series of bright and dark bars, unlimited in number as far as the mixture of light from the two pencils extends, and undistinguishable in quality. The consideration, therefore, of homogeneous light will never enable us to determine which is the point that the eye immediately turns to as the centre of the fringes. What then is the physical circumstance that determines the centre of the fringes?

The answer is very easy. For different colours the bars have different breadths. If then the bars of all colours coincide at one part of the mixture of light, they will not coincide at any other part; but at equal distances on both sides from that place of coincidence they will be equally far from the state of coincidence. If then we can find where the bars of all colours coincide, that point is the centre of the fringes.

It appears then that the centre of the fringes is *not* necessarily the point where the two pencils of light have described equal paths, but is determined by considerations of a perfectly different kind. And this is the radical error into which Mr. Potter has fallen. The distinction is important in this and in other experiments.

To find the centre of the fringes in Mr. Potter's experiment, we must proceed thus:—First, we premise that the prism produces greater deviation for yellow rays than for red rays, and that the interference-bars are narrower for yellow rays than for red rays. Next, we allow that the centre of the mixture *for each colour* is a point which the interfering pencils

of that colour have reached by equal (or rather equivalent) paths, and therefore is the place of a bright bar *for that colour*. Lastly, as the interfering pencils for different colours are made to deviate unequally by the prism, the centres of the mixture; for different colours, will not occupy the same place.

From the last consideration it follows at once that the centre of the mixture will not be the place of the centre of the fringes, inasmuch as similar bars of the different colours are not united there. To find the place where they are united we must consider that the centre of the red mixture, being the least deviated, has made the smallest progress towards the thick side of the prism, but that its bars are the broadest: the centre of the yellow mixture has made the greatest progress, but its bars are the narrowest. A number n of bars can therefore be found such that the linear deviation of the red centre $+n \times$ breadth of a red bar = linear deviation of yellow centre $+n \times$ breadth of a yellow bar: (if the equality is not exact, n may be chosen so that the second side is the greater, but so that on putting $n+1$ for n the first side will be the greater). The n th red bar and the n th yellow bar, thus determined, will coincide; and their place will be the true centre of the fringes. It is plain that when the linear dispersion produced by the prism is small, that is, when the eye-glass is very near the prism, the centre of the fringes will not sensibly differ from the centre of the mixture for any of the colours; but that when the linear dispersion is great, or the eye-glass far from the prism, the centre of the fringes will be far from the centre of any of the mixtures.

Algebraically we may express it thus:—If d be the linear deviation of the centre of the mixture for one standard colour, $d+\delta d$ that for any other colour, b the breadth of a bar for the standard colour, $b+\delta b$ that for any other colour, then the distances of the n th bars from the origin of d will be $d+nb$ and $d+\delta d+n(b+\delta b)$ respectively; and these will be equal

if $\delta d+n\delta b=0$, whence $n=-\frac{\delta d}{\delta b}$; which makes the di-

stance of the place of coincidence $d-\frac{b\delta d}{\delta b}$. As δd and δb

have different signs, this is greater than d ; or the place of coincidence is removed from the origin of d in the direction of deviation, that is, towards the thick side of the prism. And

as, upon receding from the prism, δd increases, while $\frac{b}{\delta b}$ is not altered, it follows that the place of coincidence or centre of the fringes advances further and further towards the thick side

of the prism. If δd and δb have the same proportion for rays of different colours (which we have mentioned above to be very nearly accurate), then in a given position of the eye-glass the union of colours will be perfect at the place which we have found for the centre of the fringes; and the same place would be found for the centre of the fringes if a part of the colours were abstracted, or if the light were made more nearly homogeneous.

It appears then that, according to the theory of undulations, we ought to have precisely the phænomenon which Mr. Potter has observed, supposing the light heterogeneous. It appears also that a diminution of the heterogeneity will not alter the place of the centre of the fringes; its only effect being to make a greater number of bars visible on both sides of the centre; except, indeed, the light were strictly homogeneous, when the whole place of mixture of lights would be covered with bars, of which no one could be called the centre of fringes more than another.

But this will not apply to Mr. Potter's experiment unless we can show that the light used by him was heterogeneous. I believe that any person who has tried experiments of this kind sufficiently to know what brilliancy is necessary for them, and how faint is light of any reasonable degree of homogeneity, will at once allow it to be *probable*. But I will go further, and will assert that, on the face of the experiment itself, it is *certain* that the light was heterogeneous. And the reason is simple and unanswerable, that if the light had been homogeneous there would have been no distinguishable centre of fringes. And however small might be the heterogeneity, the centre of fringes would occupy the same place as if the light were (within certain limits) ever so heterogeneous. The phænomenon observed by Mr. Potter is therefore, as far as general explanation goes, completely in accordance with the theory of undulations.

The principle upon which I have determined the place of the centre of the fringes, though it has been used by Sir John Herschel in one experiment, has never (so far as I know) been distinctly stated. It applies, however, in a great number of instances, as well in the explanation of the phænomena of polarization by interference, as in the interference of common light. The following instructive experiment (which has frequently been alluded to) shows very clearly to the eye the application of the principle; and I cite it the more readily, because I infer from a passage in Mr. Potter's paper that he may not be unwilling to try it. I shall describe it in the form in which I have found it easiest in practice.

Make a small wooden frame (like a window-frame, or rather like the frame of a schoolboy's slate, but smaller); with a saw cut through the centres of two opposite sides and unite them by hinges; keeping the parts in one plane, fix in them a clear piece of glass with putty; and when it is well bedded, cut it with a diamond along the line of the hinges. The frame will now turn well, carrying two pieces of glass of the same thickness almost exactly in contact at the turning line. Fix one side of the frame so that the glass in it receives, perpendicularly, the light which is to fall upon one of the mirrors, and so that the line of the hinges divides the pencils which fall upon the two mirrors. One pencil will then pass through the fixed glass and one through the moveable glass. Now if the glasses are in the same plane, the interference-fringes are not altered; but if, while one is perpendicular to its pencil, the other is inclined, the fringes immediately shift towards the pencil which has passed through the inclined glass. Thus far the experiment has often been described; but the part which I wish particularly to point out is the following:—If, after looking with the glasses in the same plane, the observer should leave his eye-glass and change the position of one glass and then return to his eye-glass, he would find the centre of the fringes shifted, and might perhaps infer that the fringes had shifted bodily, and that the bar which was the central bar before is the central bar now. Nothing could be less true; as he would see if by a little additional apparatus (a string, and a weight to act in opposition to the string are quite sufficient,) he inclined the glass without taking his eye from the eye-glass. The fringes shift, but at the same time they change their character in such a manner that till they have been observed a few times the eye is completely bewildered. Supposing (for clearness of ideas) that the fringes shift to the right; the central white bar as it travels becomes blue at its right edge and red at its left edge, and when it has shifted about *four* bars, the bright white bar is the *fifth* from the original place of the bright white bar (I do not pretend to great accuracy in these numbers). This observation, which is easily made without moving the eye, shows clearly the difference between the *shifting of the bars* and the *shifting of the centre of the fringes*. In Mr. Potter's experiment, while (upon withdrawing the eye and eye-glass from the prism) the *centre of the fringes* shifts, the *bars* themselves (according to theory) remain nearly stationary; but it would not be easy to preserve that steadiness of eye which is necessary for these observations.

The other fact which Mr. Potter mentions,—namely, that

the fringes become narrower as the prism is turned so as to increase much the angle of emergence,—is a direct consequence of the theory of undulations. In fact, the light after emergence comes from two virtual images more widely separated than the first images; and the breadth of the fringes is, *cæteris paribus*, inversely as the distance of the radiant images.

I have now made the remarks which I proposed to make upon Mr. Potter's experiment. I cannot, however, conclude without noticing an incidental expression, "the unfortunate half undulation which has continually to be asked for by those who adopt the undulatory theory of light." And this I do, partly because I have heard an objection something like Mr. Potter's, and partly because from Mr. Potter's way of stating it, I conclude that he must have derived it from some very imperfect or erroneous statement. I know of *no* case in which "half an undulation has to be asked for."

It happens sometimes unfortunately for a theory, that the words of its original proposer, which were necessary when the theory was new, are retained when they are not only unnecessary, but even mischievous. The propositions which in the early stages of a theory are necessary to point out the distinctions of different cases, add in no small degree to its obscurity when it is so far advanced that every case can be included in one general process. This has happened in regard to the propositions to which, I suppose, Mr. Potter alludes here.

The change of half an undulation is, in fact, a change of sign of the vibrations of which the undulation consists. The only thing to be explained then is a change of sign; and the only cases in which it occurs (so far as I know) are, certain cases of the interference of polarized light; and the interference of light forming Newton's rings. Perhaps I can best explain the apparent difficulty by referring to simple geometrical cases.

In describing to a student the relation between the versed sine and cosine, we might say, "the versed sine is formed by subtracting the cosine from the radius when the arc is less than a quadrant, and by adding it when it is greater than a quadrant." These are to him two distinct cases; but the accomplished mathematician considers them as one, connected by the theory of the change of sign. Thus it is with the interference of polarized light: in certain cases two resolved vibrations are added; on approaching a certain limit one of them disappears; on passing that limit it reappears, but in such a way that it must be subtracted from the other. But all these changes take place with as great regularity as the

changes in the rule for forming the versed sine from the cosine, and in fact follow exactly the same law.

In explaining to a young student in mechanics the motion of elastic balls when one has impinged on another which was at rest, we might perhaps make two separate cases distinguished by the circumstances of the impinging ball being the greater, or the impinging ball being the smaller; and we should point out, that in the former case the motion of the impinging ball after impact was in the same direction as before; while in the latter case the motion after impact was in the direction opposite to its first motion. A more advanced student would perceive that both were included in the general

formula $\frac{A-B}{A+B}v$. Thus it is with the reflexion of light from

the inner or outer surface of glass: the mechanical conditions appear to be precisely similar to those which I have mentioned, and the theoretical result is similar; namely, that whereas in one case we are bound to suppose the remaining motion (which produces the reflected ray) to retain the same direction as before, in the other case we are equally bound to suppose that the remaining motion has a direction opposite to that which it had before. I lay smaller stress upon this part of the theory than upon the other, because I consider the mechanical part of the theory of undulations generally as less complete than the geometrical part: but what I have stated shows clearly that there is nothing arbitrary in this change of sign; but that it is absolutely required by theory as far as theory goes. I may add, that in making a complete mathematical investigation in any part of the theory of undulations,—for the explanation, for instance, of the most complicated phenomena of polarization,—the “demand of half an undulation,” which has made so strong an impression on Mr. Potter, *never occurs*.

I rejoice that Mr. Potter has seriously undertaken to compare his experiments with the mathematical results derived from the theory of undulations. I hope (for the sake of the science) that he will continue his experiments; and I hope (more particularly for his own conviction) that he will continue the corresponding mathematical investigations. If the comparison between them be continued in the same philosophical spirit as that which marks his last paper, I can with confidence predict one result:—Mr. Potter will very soon become an *undulationist*.

I am, Gentlemen, your obedient Servant,
Observatory, Cambridge, Feb. 2, 1833. G. B. AIRY.