The Bakerian Lecture: Experiments and Calculations Relative to Physical Optics
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PHILOSOPHICAL

TRANSACTIONS.

I. *The* Bakerian *Lecture. Experiments and Calculations relative to physical Optics. By* Thomas Young, *M. D. F. R. S.*

Read November 24, 1803.

I. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

In making some experiments on the fringes of colours accompanying shadows, I have found so simple and so demonstrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to establish, that I think it right to lay before the Royal Society, a short statement of the facts which appear to me so decisive. The proposition on which I mean to insist at present, is simply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the most prejudiced, that the assertion is proved by the experiments I am about to relate, which may be repeated with great ease, whenever the sun shines, and without any other apparatus than is at hand to every one.

Exper. 1. I made a small hole in a window-shutter, and covered it with a piece of thick paper, which I perforated with a fine needle. For greater convenience of observation, I placed a small looking glass without the window-shutter, in such a position as to reflect the sun's light, in a direction nearly horizontal, upon the opposite wall, and to cause the cone of diverging light to pass over a table, on which were several little screens of card-paper. I brought into the sunbeam a slip of card, about one-thirtieth of an inch in breadth, and observed its shadow, either on the wall, or on other cards held at different distances. Besides the fringes of colours on each side of the shadow, the shadow itself was divided by similar parallel fringes, of smaller dimensions, differing in number, according to the distance at which the shadow was observed, but leaving the middle of the shadow always white. Now these fringes were the joint effects of the portions of light passing on each side of the slip of card, and inflected, or rather diffracted, into the shadow. For, a little screen being placed a few inches from the card, so as to receive either edge of the shadow on its margin, all the fringes which had before been observed in the shadow on the wall immediately disappeared, although the light inflected on the other side was allowed to retain its course, and although this light must have undergone any modification that the proximity of the other edge of the slip of card might have been capable of occasioning. When the interposed screen was more remote from the narrow card, it was necessary to plunge it more deeply into the shadow, in order to extinguish the parallel lines; for here the light, diffracted from the edge of the object, had entered further into the shadow, in its way towards the fringes. Nor was it for want of a sufficient intensity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intensity was reduced to one-tenth or one-twentieth.

Exper. 2. The crested fringes described by the ingenious and accurate Grimaldi, afford an elegant variation of the preceding experiment, and an interesting example of a calculation grounded on it. When a shadow is formed by an object which has a rectangular termination, besides the usual external fringes, there are two or three alternations of colours, beginning from the line which bisects the angle, disposed on each side of it, in curves, which are convex towards the bisecting line, and which converge in some degree towards it, as they become more remote from the angular point. These fringes are also the joint effect of the light which is inflected directly towards the shadow, from each of the two outlines of the object. For, if a screen be placed within a few inches of the object, so as to receive only one of the edges of the shadow, the whole of the fringes disappear. If, on the contrary, the rectangular point of the screen be opposed to the point of the shadow, so as barely to receive the angle of the shadow on its extremity, the fringes will remain undisturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

If we now proceed to examine the dimensions of the fringes, under different circumstances, we may calculate the differences of the lengths of the paths described by the portions of light, which have thus been proved to be concerned in producing those fringes; and we shall find, that where the lengths are equal, the light always remains white; but that, where either the brightest light, or the light of any given colour, disappears and reappears, a first, a second, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progression, as nearly as we can expect experiments of this kind to agree with each other. I shall compare, in this point of view, the measures deduced from several experiments of NEWTON, and from some of my own.

In the eighth and ninth observations of the third book of NEWTON'S Optics, some experiments are related, which, together with the third observation, will furnish us with the data necessary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the sun's light, admitted through a small aperture; and the point of concourse of the two first dark lines bordering the shadows of the respective knives, was observed at various distances. The results of six observations are expressed in the first three lines of the first Table. On the supposition that the dark line is produced by the first interference of the light reflected from the edges of the knives, with the light passing in a straight line between them, we may assign, by calculating the difference of the two paths, the interval for the first disappearance of the brightest light, as it is expressed in the fourth line. The second Table contains the results of a similar calculation, from NEWTON'S observations on the shadow of a hair; and the third, from some experiments of my own, of the same nature: the second bright line being supposed to correspond to a double interval, the second dark line to a triple interval, and the succeeding lines to depend on a continuation of the progression. The unit of all the Tables is an inch.

Table I. *Obs.* 9. N.

Distance of the knives from the aperture - - -						101.
Distances of the paper from the knives -	$1\frac{1}{2}$,	$3\frac{1}{3}$,	$8\frac{3}{5}$,	32,	96,	131.
Distances between the edges of the knives, opposite to the point of concourse -	.012,	.020,	.034,	.057,	.081,	.087.
Interval of disappearance	.0000122,	.0000155,	.0000182,	.0000167,	.0000166,	.0000166.

Table II. *Obs.* 3. N.

Breadth of the hair - - - - -		$\frac{1}{280}$.
Distance of the hair from the aperture - - -		144.
Distances of the scale from the aperture - -	150,	252.
(Breadths of the shadow - - - -	$\frac{1}{54}$,	$\frac{1}{9}$.)
Breadth between the second pair of bright lines -	$\frac{2}{47}$,	$\frac{4}{17}$.
Interval of disappearance, or half the difference of the paths	.0000151,	.0000173.
Breadth between the third pair of bright lines - -	$\frac{4}{73}$,	$\frac{3}{10}$.
Interval of disappearance, $\frac{1}{4}$ of the difference - -	.0000130,	.0000143.

Table III. *Exper.* 3.

Breadth of the object - - - - -	.434.
Distance of the object from the aperture - - - -	125.
Distance of the wall from the aperture - - - -	250.
Distance of the second pair of dark lines from each other - -	1.167.
Interval of disappearance, $\frac{1}{3}$ of the difference - - -	.0000149.

Exper. 4.

Breadth of the wire - - - - - -	.083.
Distance of the wire from the aperture - - - -	32.
Distance of the wall from the aperture - - - -	250.
(Breadth of the shadow, by three measurements	.815, .826, or .827; mean, .823.)
Distance of the first pair of dark lines -	1.165, 1.170, or 1.160; mean, 1.165.
Interval of disappearance - - - - -	.0000194.
Distance of the second pair of dark lines -	1.402, 1.395, or 1.400; mean, 1.399.
Interval of disappearance - - - - -	.0000137.
Distance of the third pair of dark lines -	1.594, 1.580, or 1.585; mean, 1.586.
Interval of disappearance - - - - -	.0000128.

It appears, from five of the six observations of the first Table, in which the distance of the shadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increased in the ratio of 7 to 1, that the difference of the routes constituting the interval of disappearance, varied but one-eleventh at most; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{160}$ part. Hence we are warranted in inferring, that the interval appropriate to the extinction of the brightest light, is either accurately or very nearly constant.

But it may be inferred, from a comparison of all the other observations, that when the obliquity of the reflection is very great, some circumstance takes place, which causes the interval thus calculated to be somewhat greater: thus, in the eleventh line of the third Table, it comes out one-sixth greater than the mean of the five already mentioned. On the other hand, the mean of two of NEWTON'S experiments and one of mine, is a result about one-fourth less than the former. With respect to the nature of this circumstance, I cannot at present form a decided opinion; but I conjecture that it is a deviation of some of the light concerned, from the rectilinear direction assigned to it, arising either from its natural diffraction, by which the magnitude of the shadow is also enlarged, or from some other unknown cause. If we imagined the shadow of the wire, and the fringes nearest it, to be so contracted that the motion of the light bounding the shadow might be rectilinear, we should thus make a sufficient compensation for this deviation; but it is difficult to point out what precise track of the light would cause it to require this correction.

The mean of the three experiments which appear to have been least affected by this unknown deviation, gives .0000127 for the interval appropriate to the disappearance of the brightest light; and it may be inferred, that if they had been wholly exempted from its effects, the measure would have been somewhat smaller. Now the analogous interval, deduced from the experiments of NEWTON on thin plates, is .0000112, which is about one-eighth less than the former result; and this appears to be a coincidence fully sufficient to authorise us to attribute these two classes of phenomena to the same cause. It is very easily shown, with respect to the colours of thin plates, that each kind of light disappears and reappears, where the differences of the routes of two of its portions are in arithmetical progression; and we have seen, that the same law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The distribution of the colours is also so similar in both cases, as to point immediately to a similarity in the causes. In the thirteenth observation of the second part of the first book, NEWTON relates, that the interval of the glasses where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh observation of the third book, that the distances between the fringes, under the same circumstances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the squares, in order to find the relation of the difference of the routes, we have the proportion of 14 to $9\frac{1}{4}$, which scarcely differs from the proportion observed in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crested fringes of GRIMALDI, already described; for it will appear that, under the circumstances of the experiment related, the points in which the differences of the lengths of the paths described by the two portions of light are equal to a constant quantity, and in which, therefore, the same kinds of light ought to appear or disappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the shadow, and the asymptotes nearly with the diagonal line. Such, therefore, must be the direction of the fringes; and this conclusion agrees perfectly with the observation. But it must be remarked, that the parts near the outlines of the shadow, are so much shaded off, as to render the character of the curve somewhat less decidedly marked where it approaches to its axis. These fringes have a slight resemblance to the hyperbolic fringes observed by Newton; but the analogy is only distant.

III. APPLICATION TO THE SUPERNUMERARY RAINBOWS.

The repetitions of colours sometimes observed within the common rainbow, and described in the Philosophical Transactions, by Dr. Langwith and Mr. Daval, admit also a very easy and complete explanation from the same principles. Dr. Pemberton has attempted to point out an analogy between these colours and those of thin plates; but the irregular reflection from the posterior surface of the drop, to which alone he attributes the appearance, must be far too weak to produce any visible effects. In order to understand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the posterior surface of the drop, and crossing each other in various directions, till, at the angle of the greatest deviation, they coincide with each other, so as to produce, by the greater intensity of this redoubled light, the common rainbow of 41 degrees. Other parts of these two portions will quit the drop in directions parallel to each other; and these would exhibit a continued diffusion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other similar cases, divides the light into concentric rings; the magnitude of these rings depending on that of the drop, according to the difference of time occupied in the passage of the two portions, which thus proceed in parallel directions to the spectator's eye, after having been differently refracted and reflected within the drop. This difference varies at first, nearly as the square of the angular distance from the primitive rainbow: and, if the first additional red be at the distance of 2° from the red of the rainbow, so as to interfere a little with the primitive violet, the fourth additional red will be at a distance of nearly 2° more; and the intermediate colours will occupy a space nearly equal to the original rainbow. In order to produce this effect, the drops must be about $\frac{1}{76}$ of an inch, or .013, in diameter: it would be sufficient if they were between $\frac{1}{70}$ and $\frac{1}{80}$. The reason that such supernumerary colours are not often seen, must be, that it does not often happen that drops so nearly equal are found together: but, that this may sometimes happen, is not in itself at all improbable: we measure even medicines by dropping them from a phial, and it may easily be conceived that the drops formed by natural operations may sometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the observation, may best be determined from Dr. LANGWITH's own words.

"August the 21st, 1722, about half an hour past five in the "evening, weather temperate, wind at north-east, the appearance "was as follows. The colours of the primary rainbow were as "usual, only the purple very much inclining to red, and well

" defined: under this was an arch of green, the upper part of " which inclined to a bright yellow, the lower to a more dusky " green: under this were alternately two arches of reddish " purple, and two of green: under all, a faint appearance of " another arch of purple, which vanished and returned several " times so quick, that we could not readily fix our eyes upon it. " Thus the order of the colours was, I. Red, orange-colour, yel- " low, green, light blue, deep blue, purple. II. Light green, dark " green, purple. III. Green, purple. IV. Green, faint vanishing " purple. You see we had here four orders of colours, and per- " haps the beginning of a fifth: for I make no question but that " what I call the purple, is a mixture of the purple of each of " the upper series with the red of the next below it, and the " green a mixture of the intermediate colours. I send you not " this account barely upon the credit of my own eyes; for there " was a clergyman and four other gentlemen in company, " whom I desired to view the colours attentively, who all " agreed, that they appeared in the manner that I have now de- " scribed. There are two things which well deserve to be taken " notice of, as they may perhaps direct us, in some measure, to " the solution of this curious phenomenon. The first is, that the " breadth of the first series so far exceeded that of any of the " rest, that, as near as I could judge, it was equal to them all " taken together. The second is, that I have never observed " these inner orders of colours in the lower parts of the rainbow, " though they have often been incomparably more vivid than " the upper parts, under which the colours have appeared. I " have taken notice of this so very often, that I can hardly look " upon it to be accidental; and, if it should prove true in general, " it will bring the disquisition into a narrow compass; for it will

" show that this effect depends upon some property which the " drops retain, whilst they are in the upper part of the air, but " lose as they come lower, and are more mixed with one ano- " ther." Phil. Trans. Vol. XXXII. p. 243.

From a consideration of the nature of the secondary rainbow, of 54°, it may be inferred, that if any such supernumerary colours were seen attending this rainbow, they would necessarily be external to it, instead of internal. The circles sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

The experiment of GRIMALDI, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by NEWTON. Those who are attached to the NEWTONIAN theory of light, or to the hypotheses of modern opticians, founded on views still less enlarged, would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at least from idle declamation against a system which is founded on the accuracy of its application to all these facts, and to a thousand others of a similar nature.

From the experiments and calculations which have been premised, we may be allowed to infer, that homogeneous light, at certain equal distances in the direction of its motion, is possessed of opposite qualities, capable of neutralising or destroying each other, and of extinguishing the light, where they happen to be united; that these qualities succeed each other alternately in successive concentric superficies, at distances which are constant for the same light, passing through the same medium. From the agreement of the measures, and from the similarity of the phenomena, we may conclude, that these intervals are the same as are concerned in the production of the colours of thin plates; but these are shown, by the experiments of NEWTON, to be the smaller, the denser the medium; and, since it may be presumed that their number must necessarily remain unaltered in a given quantity of light, it follows of course, that light moves more slowly in a denser, than in a rarer medium: and this being granted, it must be allowed, that refraction is not the effect of an attractive force directed to a denser medium. The advocates for the projectile hypothesis of light, must consider which link in this chain of reasoning they may judge to be the most feeble; for, hitherto, I have advanced in this Paper no general hypothesis whatever. But, since we know that sound diverges in concentric superficies, and that musical sounds consist of opposite qualities, capable of neutralising each other, and succeeding at certain equal intervals, which are different according to the difference of the note, we are fully authorised to conclude, that there must be some strong resemblance between the nature of sound and that of light.

I have not, in the course of these investigations, found any reason to suppose the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them; and, upon considering the phenomena of the aberration of the stars, I am disposed to believe, that the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees.

The observations on the effects of diffraction and interference, may perhaps sometimes be applied to a practical purpose, in making us cautious in our conclusions respecting the appearances of minute bodies viewed in a microscope. The shadow of a fibre, however opaque, placed in a pencil of light admitted through a small aperture, is always somewhat less dark in the middle of its breadth than in the parts on each side. A similar effect may also take place, in some degree, with respect to the image on the retina, and impress the sense with an idea of a transparency which has no real existence: and, if a small portion of light be really transmitted through the substance, this may again be destroyed by its interference with the diffracted light, and produce an appearance of partial opacity, instead of uniform semitransparency. Thus, a central dark spot, and a light spot surrounded by a darker circle, may respectively be produced in the images of a semitransparent and an opaque corpuscle; and impress us with an idea of a complication of structure which does not exist. In order to detect the fallacy, we may make two or three fibres cross each other, and view a number of globules contiguous to each other; or we may obtain a still more effectual remedy by changing the magnifying power; and then, if the appearance remain constant in kind and in degree, we may be assured that it truly represents the nature of the substance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. HEWSON in the Phil. Trans. Vol. LXIII. for 1773, might not in some measure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch focus, I have found them nearly such as Mr. Hewson has described them.

V. REMARKS ON THE COLOURS OF NATURAL BODIES.

Exper. 5. I have already adduced, in illustration of Newton's comparison of the colours of natural bodies with those of thin plates, Dr. Wollaston's observations on the blue light of the lower part of a candle, which appears, when viewed through a prism, to be divided into five portions. I have lately observed a similar instance, still more strongly marked, in the light transmitted by the blue glass sold by the opticians. This light is separated by the prism into seven distinct portions, nearly equal in magnitude, but somewhat broader, and less accurately defined, towards the violet end of the spectrum. The first two are red, the third is yellowish green, the fourth green, the fifth blue, the sixth bluish violet, and the seventh violet. This division agrees very nearly with that of the light reflected by a plate of air $\frac{1}{6840}$ of an inch in thickness, corresponding to the 11th series of red and the 18th of violet. A similar plate of a metallic oxide, would perhaps be about $\frac{1}{15000}$ of an inch in thickness. But it must be confessed, that there are strong reasons for believing the colouring particles of natural bodies in general to be incomparably smaller than this; and it is probable that the analogy, suggested by Newton, is somewhat less close than he imagined. The light reflected by a plate of air, at any thickness nearly corresponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumstances, the 11th red and the neighbouring colours may still be distinguished. The light of some kinds of coloured glass is pure red; that of others, red with a little green: some intercept all the light, except the extreme red and the blue. In the blue light of a candle, expanded by the prism, the portions of each colour appear to be narrower, and the intervening dark spaces wider, than in the analogous spectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye sold in the shops, which is a preparation of the carthamus, affords a good specimen of a yellow green light regularly reflected, and a crimson probably produced by transmission.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Exper. 6. The existence of solar rays accompanying light, more refrangible than the violet rays, and cognisable by their chemical effects, was first ascertained by Mr. RITTER: but Dr. WOLLASTON made the same experiments a very short time afterwards, without having been informed of what had been done on the Continent. These rays appear to extend beyond the violet rays of the prismatic spectrum, through a space nearly equal to that which is occupied by the violet. In order to complete the comparison of their properties with those of visible light, I was desirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpose, I formed an image of the rings, by means of the solar microscope, with the apparatus which I have described in the Journals of the Royal Institution, and I threw this image on paper dipped in a solution of nitrate of silver, placed at the distance of about nine inches from the microscope. In the course of an hour, portions of three dark rings were very distinctly visible, much smaller than the brightest rings of the coloured image, and coinciding very nearly, in their dimensions, with the rings of violet light that appeared upon the interposition of violet glass. I thought the dark rings were a little smaller than the violet rings, but the difference was not sufficiently great to be accurately ascertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum, so rapidly as at the red end. For performing this experiment with very great accuracy, a heliostate would be necessary, since the motion of the sun causes a slight change in the place of the image; and leather, impregnated with the muriate of silver, would indicate the effect with greater delicacy. The experiment, however, in its present state, is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law which is the principal subject of this Paper. If we had thermometers sufficiently delicate, it is probable that we might acquire, by similar means, information still more interesting, with respect to the rays of invisible heat discovered by Dr. HERSCHEL; but at present there is great reason to doubt of the practicability of such an experiment.