From the PHILOSOPHICAL MAGAZINE for July 1852.

REMARKS ON LORD BROUGHAM'S "EXPERIMENTS AND OBSERVATIONS ON THE PROPERTIES OF LIGHT, &c."

INSERTED IN THE PHIL. TRANS. 1850, PART I.

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THE publication of Lord Brougham's optical researches, in which a number of experimental facts connected with the phænomena now usually called "diffraction," are viewed according to a peculiar theory of certain new properties of light, and in some respects held to be irreconcileable with the principle of interference, seems to render desirable some examination into the actual bearing of the results on the theory of undulations, by which not only all the phænomena of diffraction, hitherto known, have been so perfectly explained, but which has also been applied so extensively to other large classes of facts, as to render it unphilosophical to resort to theories assumed on independent grounds to meet apparent exceptions in limited classes of phænomena.

These researches having been briefly alluded to by the Astronomer Royal in his opening address to the British Association at Ipswich*, and having also myself made a few observations on the subject at the same meeting†, my object in the present communication is to follow up the question in somewhat more detail; and without pretending to enter on any controversy as to the author's *theory*, to examine merely the *experimental evidence* adduced, and inquire how far it seems accordant or not with the undulatory theory.

During the summer of last year I took the opportunity of

* See Athenæum, No. 1236. † See Ibid. No. 1237.

(12)

repeating the experiments with the utmost care, for all the most material cases considered; since which time various causes have delayed the publication of my results.

The whole of the author's investigations are expressed with reference to his peculiar hypothesis of certain forces of "deflexion" and "inflexion" supposed to be exerted upon the rays of light by the action of the edge of an opake body near which they pass: nor is it always an easy matter to disentangle the actual facts from the language of this theory, so as to see to what the experimental evidence really amounts.

Of those of the author's propositions which refer solely to the exposition of his theoretical views, I do not propose to enter on any discussion. There are also other portions of the investigations, which, though of a more experimental character, will not call for much observation, as they either tend to establish phænomena in exact conformity with well-known results, or are of a nature not having much bearing on theory either way.

Of this class are the preliminary experiments (Prop. I. Exp. 1, 2, 3); though with respect to the last it ought to be remarked, that Newton by no means limits the number of fringes to *three*, and in one modification of the experiment expressly mentions that *four or five* were rendered visible*. When (as in Exp. 4) the origin of light is not the *single point absolutely requisite in all accurate investigations*, but an extended object, such as a flame, the moon, &c., it may be questioned how far the fringes may be properly termed images of it. In Prop. II. Exp. 2, that the nature or form of the edge makes no difference in the result, accords exactly with the long-known experiments of Biot, Haldat, and others. Indeed, as is equally well understood, the fringes may be produced without any opake edge at all, as at the junction of two faces cut on a glass, slightly inclined to each other. Again, the hyperbolic fringes of an acute angle (in Prop. V. Exp. 3), as well as the measures of the fringes at successive distances from the edge determining the locus of any given fringe (in Prop. X., and additional remarks, (2) p. 252), appear to agree with previous observations; though, according to the author's theory, each fringe seems to be regarded as an individual ray, while in the interference theory it is the locus of the intersections of a series of rays.

At another part of his discussion the author assumes (Prop. XI.) an aggressive position, and endeavours to *refute* the application of the interference theory. In reply I think it will suffice to remark, under the several heads,—(1) the theory of interference explains perfectly *both* the internal and external fringes of a shadow; (2) the *breadth* of the fringes has no dependence on

* See Opticks, book 3. part 2. obs. 2.

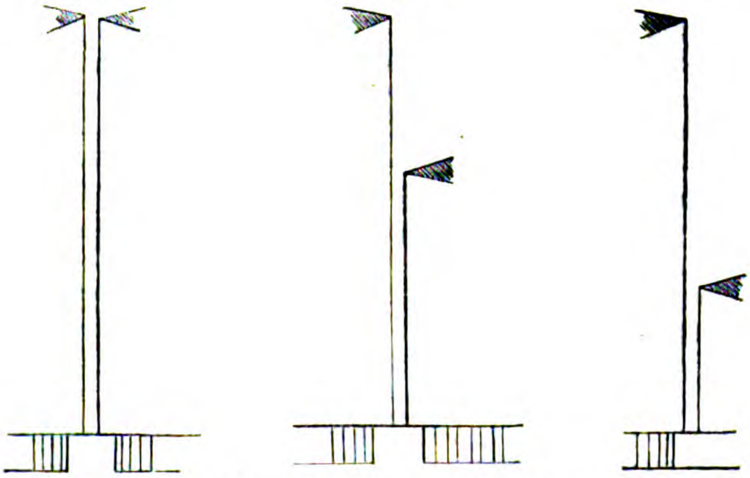
the *length of route* of the rays, but it has on the *angle* at which they intersect; so that (3) in the case represented in the author's fig. (20), supposing abstractedly two pairs of interfering rays (such as BC, AC, and BD, AD), it is evident that the fringes at D *ought to be* broader than those at C, not owing to any difference in the *routes*, but because the *angle* BDA is less than BCA; while (4) interference perfectly explains fringes, even when the action is wholly on *one side* of the ray or edge.

But passing from these points of confessedly less importance, we will proceed to the most material and fundamental experiment (Prop. II. Exp. 1), in which, when fringes are formed by the edge of an opake body, if a second edge be placed at a greater distance along the ray from the origin on the same side as the first edge, it produces no change in the fringes, but on the opposite side it does, the fringes being shifted in position towards the first side; or in other words, in the one case it has no power of producing further diffraction, in the other it has: and this is viewed by the author as supporting his theory of a peculiar action exerted by the edge upon the ray passing near it, by which it is disposed or indisposed for further flexure according to the conditions above expressed.

The experimental fact in general is easily verified. There is, however, one material condition necessary to be attended to for reproducing the result exactly as described by the author.

When two edges are at the same distance from the origin and from a narrow aperture, they give, as is well known, fringes on each side extending into the shadow, with a white centre (fig. 1). As one edge is removed successively further from the origin and nearer to the screen, the fringes on that side dilate (fig. 2),

Fig. 1. Fig. 2. Fig. 3.



become faint, and at length disappear (fig. 3); so that beyond a certain distance there remain only the fringes on the other side, or on that of the edge nearest the origin, which diverge further into the shadow on that side as the breadth of the effective aperture is diminished.

In this way, then, the second edge, if beyond the limits of distance mentioned, will cause an appearance of fringes on the side towards the first edge diverging into the shadow.

With regard to the bearing of this experiment on theory, it is in the first instance necessary to bear in mind, that, *according to the undulatory theory, neither the formation of fringes, nor any shifting of those fringes, implies a* FLEXURE *in the rays; in this theory no such idea is introduced or needed.*

In the particular case in question, when the two edges are at the same distance from the origin forming a narrow aperture, the nature of the fringes is perfectly explained and reduced to quantitative results by Fresnel's theory.

When the second edge is placed as in Lord Brougham's experiments, at a greater distance along the ray, this would be equivalent to a wide aperture placed obliquely to the direction of the ray, so as to be effectively as narrow as before. Now this case is one *which has not yet been reduced to calculation.*

The formulas of Fresnel, even in the simplest cases, are considerably complicated, and involve integrations which cannot be generally exhibited in a finite form. In the cases of a single edge, or that of an aperture when it is a long narrow parallelogram, an equilateral triangle, or a circle, the integration has been performed in a way sufficient for calculation*.

In the case of the *oblique* aperture, at my request, a friend eminently versed in the analysis of the subject, undertook to work out the formulas; and he pursued the inquiry far enough to be able to say that they became immensely complicated; still it could not be certain that they might not be made to yield to proper treatment, should anyone think it worth while to follow up the attempt.

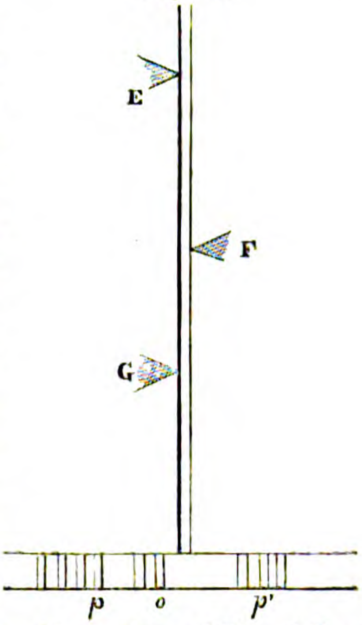
But further, this particular case *has been considered,* though only in a general way, by Fresnel†. Upon the obvious geometrical construction he points out the general conditions for determining the position of a fringe, and shows that the fringes will in this case undergo a modification, and *will not be symmetrical,* but more expanded on one side than the other, which exactly agrees with observation.

* See Airy's Tracts, Undulatory Theory, art. 73 *et seq.* Journal of Science and Phil. Mag. vol. xv. Dec. 1839; and vol. xviii. Jan. 1841.

† *Mém. sur la Diffraction. Mém. de l'Institut,* vol. v. note, p. 452, for 1821, published in 1826.

The simple facts affirmed in Prop. III. Exp. 1 and 2, when divested of all theoretical language, appear to be, that if three edges, E, F, G, be placed at successive distances from the origin in the order of the letters, E and G being on one side of the ray and F on the other; then if E and F alone give fringes as at *o* (fig. 4), and G be then made to act upon them, or if F and G alone give fringes and E be made to act upon them, in either case the fringes will be shifted to *p* towards the side on which E and G lie, and become broader; and the conclusion which the author chiefly insists upon is that all three edges act in producing the ultimate result: the same thing being further confirmed by exp. 3, in which a curved form given to the edge E, is still exhibited in the form of the fringes after the action of F and G.

Fig. 4.



That all three edges should be in some degree effective in producing the ultimate character of the fringes, would, on a general view, be obviously consistent with the wave theory; since, on that theory, a new set of waves originates at each edge, all of which conspire to produce the ultimate result; though antecedent to exact calculation, it would be impossible to say what would be the precise action of each.

On repeating the experiment, however, in regard to the *particular appearances* described by the author, I have found considerable difficulty: consistently with the conditions before remarked, if the edges E and F form a narrow aperture so as to give a white centre, and within such limits of distance along the ray as to produce fringes on *each* side (as in fig. 2), then if G be also within the same limits, and be advanced so near to the ray laterally as to make a still narrower aperture, the fringes on *each side* will expand further into the shadow. If the edges be *beyond* those limits (which seems to be implied in the author's description, since he speaks of only *one* set of fringes), then E and F will give a white centre with fringes *on the side towards* E, as at *o*; and when G is introduced it will narrow the aperture and give new fringes *on the side towards* F, at *p'*, that is, just *the opposite way* to that which the author describes. In repeating the experiment a great number of times at very different distances, and under varied conditions, I have never been able to obtain any other result: indeed it would clearly be inconsistent with the former experiments that it should be otherwise.

Prop. VI. appears precisely to express Fresnel's conclusion (above referred to), that with two edges at unequal distances from the origin, the fringes will be broader on the side towards the edge most remote from the origin, which is again more precisely exhibited in Prop. VIII., when the aperture is sufficiently narrow to give a white centred image; the same regard being had to the limits in distance as before.

In Prop. VII. the meaning is by no means obvious; but it seems to amount experimentally to this,—that with one edge only, the fringe nearest that edge is the broadest; and that when a second edge acts opposite to it at some distance along the ray, but so as to give the fringes of an aperture, then among the fringes of each set, those towards the middle of the aperture are the broadest: the first being obviously the case of the external fringes; the second easily verified, and agreeing with the ordinary case of an aperture with edges at the same distance; while as to the application of the undulatory theory, we can only make the same remarks as before.

In the Additional Observations, (3), p. 254, the truth of the general assertion, that when fringes are formed by two edges, a third can affect them only when parallel and not when at right angles to them, is indeed obvious; but the precise conditions of the experiment mentioned are difficult to understand. It would seem to consist in first forming the fringes of a narrow aperture with a broad white centre in the ordinary way; and then in that white centre producing new fringes by a third edge nearer to the screen: these, however, the author affirms, will be formed only when the third edge is parallel to the aperture and not when at right angles to it; they are also described as brighter and narrower than the ordinary fringes. The author cautions us against confounding them with the ordinary external fringes, and proceeds to argue that they are of a different nature, for several reasons, but chiefly because (Exp. 1) they do not increase in breadth when the aperture is narrowed, and (Exp. 2) because their breadth increases as the distance of the third edge from the aperture is diminished, the third edge remaining at the same distance from the screen.

The last results (which I have fully verified) do not appear to me to evince any *peculiarity*: relatively to the third edge, the aperture may be regarded as a new origin of light, in which light the third edge gives its external fringes.

But with respect to the first part of the proposition, viz. that these fringes are only formed parallel to the aperture, on repeatedly trying the experiment, I have uniformly found them formed equally, whether the edge be parallel or perpendicular to the aperture; though in the latter case they may for obvious reasons be less distinct and conspicuous.

It might indeed be fully admitted that the rays forming the white centre may be in some respects under different conditions from the ordinary rays, and that thus the fringes formed in them might possibly be different: I can only say that I have never been able to detect any such difference.

If, indeed, the author's meaning be that these fringes extended in any degree into the lateral fringes, it is obvious that they would be mutually affected in a way conformable to previous experiments.

One other remark of the author deserves especial attention*; that, but for what he considers the incapacity for further flexure in the same direction, induced in a ray after one inflexion, that ray might be continually bent round an opake body; and thus a luminous object might be seen, though the whole of the body intervened, or in other words, that we might *see round a corner*.

Now if such inflexion took place it would clearly be always accompanied by a considerable diffusion of the light, so that after a few successive inflexions it might be so much weakened as to become imperceptible.

It is however a remarkable fact, that such an apparent inflexion *does take place* to a very great extent, as I have pointed out in a paper "On Luminous Rings round Shadows" (Memoirs of the Royal Astronomical Society, vol. xvi. p. 306), and which (as I have there mentioned) I believe to be a modification of the same phænomenon, described rather obscurely by Newton† and more distinctly by Hooke‡, and apparently accordant with the theory of undulations (Ibid. p. 310).

* Additional Observations, 4.

† Opticks, book 3. part 1. obs. 5.

‡ As this curious point seems to have been much overlooked, I shall perhaps be excused in annexing a brief notice of Dr. Hooke's experiment, from a fragment on Light, appended to the Essay on Comets and Gravity, in his posthumous works: London, 1705, p. 186.

Light being admitted into a dark room through a very small hole and received on a screen at some distance, on holding an opake body in the cone of light, besides a "zone or fascia of light much brighter than the rest of the surface," along and outside the edge of the shadow (which was probably the first diffraction fringe), he observed *a faint light extending from the edge into the shadow*; and when the opake body was held so as to cover nearly the whole of the luminous circle, "*rays were seen darting downwards perpendicular to the edge of the shadow, like the tail of a comet, striking downwards more than* 10 *times, probably* 100 *times their breadth, or very near to a quadrant," and growing fainter at greater distances.* The "rays" were obviously occasioned by irregularities in the edge; the rays were per-

I have thus, I trust with perfect impartiality, gone through all the main experimental points of the author's investigations, and upon the whole I can perceive *nothing substantiated* which is positively *irreconcileable with the principle of interference,* while the new modifications of the phænomena here presented, so far as *general* considerations can be relied on, seem sufficiently conformable to the undulatory theory: but as to their more *exact,* or *quantitative* explanation, no definitive opinion can be pronounced, until certain analytical investigations of almost impracticable length and complexity, shall have been gone through, by which alone that theory can be brought into exact and satisfactory comparison with experiment.

pendicular to the edge; if circular, tending to the centre; if angular, bisecting it; if concave, spreading out, &c. A representation of the appearance is given in Plate ii. fig. 8 (p. 155). At p. 190, the Editor adds a memorandum found among Dr. Hooke's papers, stating, that on March 18, 167$\frac{4}{5}$, he "read a discourse" on several new properties of light; which he sums up as follows:—

"That there is a deflexion of light differing both from reflexion and refraction, and seeming to depend on the unequal density of the constituent parts of the ray, whereby light is dispersed from the place of condensation and rarefied or gradually diverged into a quadrant;" 2ndly, that this takes place "perpendicularly to the edge;" and 3rdly, that "the parts deflected by the greatest angle are the faintest."

I have fully referred to and commented upon Newton's description of the same phænomenon, conveyed in terms so singularly coincident, in my paper before referred to.