*c*. In a similar manner, the variations in length of one of the circuits act upon the *intensity* of the current which emanates from a constant source. Now it is possible that two currents of unequal intensity may be incapable of interfering in conditions in which they would be mutually destroyed if their intensities had been equal.

73. It must be remarked, that the two first objections do not apply to the method of induced currents. But, in order to deprive them of all value, I have imagined a third, which consists of employing only a single thermo-electric pair, the current of which passes at the same time in part into the wire of the rheometer, and in part into a wire of derivation. This wire was the copper one No. 4, and the pair that of the platinum wire No. 2 and the copper one No. 3.

### C. *Method of Derivations.*

74. When the circuit of a pair is closed with the rheometer, three derivations may be effected:—from the platinum wire to the copper one, by thus compelling a part of the current not to traverse the measuring apparatus; from the platinum wire to itself, by establishing at certain points of its length junctions with the extremities of the wire of deviation, and causing a variation both in the distance of these points and in the length of this wire; lastly, from the copper wire to itself, by proceeding in an analogous manner.

75. The wire of derivation was $0^{m}\cdot003$ in diameter, and consequently could not be wound round the rheostat. To ensure a constant contact on its surface (an extremely important condition), and to cause its length to vary so slowly that this was necessary, I employed a tube of pure copper, $0^{m}\cdot05$ long, with a bore of $0^{m}\cdot0034$, and fixed in one of the holes of a Poggendorff's clamp; its two extremities were split like a porte-crayon, and the lips might be partially closed at will by compressing rings.

76. *None of the three derivations occasioned phænomena of interference.*

77. The second derivation proved the remarkable action of the conductibility on the intensity of the current, for the copper wire No. 4 being a much better conductor than the platinum wire of the pair, increased the deviation of the needle sensibly. In one of the experiments, by making the junctions on the one hand on the platinum wire immediately after its exit from the test-tubes, and on the other with the rheometer, the index was propelled 15° from its first position. This increase in the deviation became weaker, but without any intermittence, when the first point of contact was brought near the second.



78. The third derivation produced no sensible action on the needle, as was easy to foresee, since the wire of derivation and that of copper of the pair were of the same diameter, and as each one, taken separately, had a conductibility of its own and dimensions sufficient to transmit the whole thermo-electric current.

79. The method of derivations appears to me to be free from the third objection (72, *c*), that of the intensities. In fact, the difference in the intensity of the currents which re-united after issuing from one and the same source, and having followed two entirely similar ways, was capable of being rendered as feeble as possible without any result of interferences. Now analogy being here our sole guide, it is necessary to remember that in wholly similar circumstances, the vibrations of the æther which constitute light, and those of the elastic fluids which engender sound, have presented very evident phænomena of mutual destruction*.

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## LXXVIII. *On* Fresnel's *Theory of Double Refraction.* *By* R. Moon, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society*†.

HAVING on several previous occasions treated of the theory of unpolarized light, and having, as I trust, effectually exposed the futility of the celebrated hypothesis devised by Fresnel for the elucidation of many of the principal phænomena in that department of optics, I now come to the consideration of the subject of polarized light; upon his treatment of which Fresnel's great fame now principally rests, and to whose views in regard to which many of his adherents, who have felt themselves compelled to give up his theory of diffraction, still cling with unshaken fidelity. What my own faith on this subject may be, it is unnecessary at present to disclose further than this, that whether the original idea of transversal

* In the fundamental experiment of Fresnel, the bundles of light do not necessarily reach the two mirrors under the same incidence, and have not the same intensity when they interfere after reflexion. After M. W. Weber had shown that the surfaces according to which sound disappears around a vibrating diapason are hyperbolically curved, Dr. Kane succeeded, following out an idea of Sir John Herschel, in constructing united tubes, the lengths of which are in the relation of 2 to 3, or of 6 to 7, and which destroy by interference one, in a determined number, of the sounds which is made to pass through them. (Philosophical Magazine, vol. vii. p. 301; Poggendorff's *Annalen der Physik*, vol. xxxvii. p. 485.)

† Communicated by the Author.

vibrations is due to Young only or not, I pledge myself to prove that the researches of Fresnel have not advanced us one step beyond it.

Fresnel's first step in the mathematical part of his theory, is to prove the existence of the axes of elasticity: "a proposition," says Mr. Smith (Cambridge Mathematical Journal, vol. i. p. 1), "on which the whole theory of double refraction depends, and which Fresnel has proved by a method which has the advantage of geometrical distinctness, but which is long and rather difficult to follow out on that account." On the same account I shall prefer to give Mr. Smith's elegant analysis in preference to the cumbrous processes of Fresnel, trusting my readers will take my word for it, that whatever it may want in "geometrical distinctness," it gains in logical clearness.

"The proposition is thus stated:—In any system of particles acting on each other with forces which are functions of their mutual distances, there are three directions at right angles to each other, along which if a particle be displaced, the forces of restitution will act in the same direction.

"Let $x\,y\,z$ be the co-ordinates of the attracted point $x_1\,y_1\,z_1$; $x_2\,y_2\,z_2$ ... be the co-ordinates of the attracting points; $r_1\,r_2\,r_3$ ... the distances between the attracted and attracting points; $\phi_1(r_1)\;\phi_2(r_2)\;\phi_3(r_3)$ ... the attractions; X Y Z the total resolved forces along the axes, then we shall have

$$X = \frac{x_1 - x}{r_1}\,\phi_1(r_1) + \frac{x_2 - x}{r_2}\,\phi_2(r_2) + \&c.;$$

and similarly for Y and Z. Now let

$$R = -\Sigma \int \phi(r)\,d\,r,$$

then

$$\left.\begin{aligned} X &= \frac{d\,R}{d\,x} = 0, \\ Y &= \frac{d\,R}{d\,y} = 0, \\ Z &= \frac{d\,R}{d\,z} = 0, \end{aligned}\right\} \text{when the particle is in equilibrio.}$$

"Let the particle receive a small displacement, the projections of which on the co-ordinate axes are $\delta x,\ \delta y,\ \delta z$. Then supposing the displacement to be very small, the force of restitution may be taken as proportional to it, so that we have



$$\left.\begin{aligned} X &= \frac{d^2 R}{d\,x^2}\,\delta x + \frac{d^2 R}{d\,x\,d\,y}\,\delta y + \frac{d^2 R}{d\,x\,d\,z}\,\delta z, \\ Y &= \frac{d^2 R}{dx\,dy}\,\delta x + \frac{d^2 R}{dy^2}\,\delta y + \frac{d^2 R}{d\,x\,d\,z}\,d\,z, \\ Z &= \frac{d^2 R}{dx\,dz}\,\delta x + \frac{d^2 R}{dy\,dz}\,\delta y + \frac{d^2 R}{d\,z^2}\,\delta z. \end{aligned}\right\} \quad . \quad . \quad (A.)$$

"Now the force of restitution will be in the direction of the displacement, if X Y Z be proportional to $\delta x,\ \delta y,\ \delta z$. Let then

$$s = \frac{X}{\delta x} = \frac{Y}{\delta y} = \frac{Z}{\delta z};$$

then putting

$$\frac{d^2 R}{d\,x^2} = A, \quad \frac{d^2 R}{d\,y^2} = B, \quad \frac{d^2 R}{d\,z^2} = C,$$

$$\frac{d^2 R}{d\,z\,d\,y} = A', \quad \frac{d^2 R}{d\,z\,d\,x} = B' \quad \frac{d^2 R}{d\,x\,d\,y} = C',$$

and substituting in the former equations, they become

$$\begin{aligned} &(A - s)\,\delta x + C'\delta y + B'\delta z = 0, \\ &C'\delta x + (B - s)\delta y + A'\delta z = 0, \\ &B'\delta x + A'\delta y + (C' - s)\,d\,z = 0," \end{aligned}$$

from which it is easy to prove, supposing the above process correct, "that there are three directions at right angles to each other, along which, if a particle be displaced, the force of restitution acts in the same direction."

But the fact is, the above process is entirely fallacious if it is meant to apply to the case of the motion of a particle of the æthereal medium by which light is produced. What is meant by the mysterious principle, that "supposing the displacement to be very small, the force of restitution may be taken as proportional to it," I profess myself unable to comprehend; but this I do understand, that when the co-ordinates of the particle, whose motion is being considered, vary from $x, y, z$ to $x + \delta x,\ y + \delta y,\ z + \delta z$, the co-ordinates of the other particles of the medium will vary from $x_1\,y_1\,z_1,\ x_2\,y_2\,z_2$, &c. to $x_1 + \delta x_1,\ y_1 + \delta y_1,\ z_1 + \delta z_1,\ x_2 + \delta x_2,\ y_2 + \delta y_2,\ z_2 + \delta z_2$, &c.; and that therefore the above values (A.) for the resolved parts of the force on the particle whose motion is being considered, are entirely fallacious.

The true value of X in this case is,

$$\frac{d^2 R}{d\,x^2}\,\delta x + \frac{d^2 R}{dx\,dy}\,\delta y + \frac{d^2 R}{dx\,dz}\,\delta z$$

$$+\frac{d^2 R}{dx\,dx_1}\delta x_1 + \frac{d^2 R}{dx\,dy_1}\delta y_1 + \frac{d^2 R}{dx\,dz_1}\delta z_1$$
$$+\frac{d^2 R}{dx\,dx_2}\delta x_2 + \frac{d^2 R}{dx\,dy_2}\delta y_2 + \frac{d^2 R}{dx\,dz_2}\delta z_2$$
$$+ \&c.,$$

and similarly for Y and Z; from which it is evident, that to talk of the existence of axes of elasticity in every system of particles acting on each other is mere absurdity. And hence it appears, that the "proposition on which the whole theory of double refraction depends" is altogether untrue.

Will it be urged, however, that although the general proposition does not hold, there still may be particular systems of particles for which it does hold? I do not hesitate to state my belief, that the existence of such a system is *impossible*; and at any rate would challenge any analyst whatever to suggest any such.

The case then stands thus:—A writer states a proposition as the basis of a theory; he offers a proof of the proposition, which turns out to be fallacious; and not only is the proof itself erroneous, but during the investigation there appears a degree of evidence approaching to certainty, that the proposition itself, after modifying it in every conceivable way consistent with the case to which it is meant to apply, is untrue; and there is moreover a *perfect certainty* that it is incapable of proof. Thus we have a fundamental proposition of which a false proof is given, a certainty that if true, it must always remain a mere assumption incapable of independent proof; and this in the face of the fact that there is every reason to suppose it untrue. Such a combination of circumstances would have decided the fate of any other theory; why is this to be made an exception to the rule? But to return.

Assuming the existence of the axes of elasticity, we are next introduced to the surface of elasticity. Referring the co-ordinates to the axes of elasticity, we have

$$\left.\begin{array}{l} X = a\,\delta x = a\,r\cos\alpha \\ Y = b\,\delta y = b\,r\cos\beta \\ Z = c\,\delta z = c\,r\cos\gamma \end{array}\right\}\text{where } a, b, c \text{ are constants.}$$

The rest I shall give in the words of Sir John Herschel (vide Encyclopædia Metropol., art. Light, 1004): "M. Fresnel next conceives a surface, which he terms the 'surface of elasticity,' constructed according to the following law:—On each of the axes of elasticity, and on every radius $r$ drawn in all directions, take a length proportional to the square root of the elasticity exerted on the displaced molecule by the medium in the direction of the radius, or to $\sqrt{F}$; then if we call R this length, or the radius vector of the surface of elasticity, we shall have



$$R^2 = \{a\,r\,.\,\overline{\cos\alpha}|^2 + b\,r\,.\,\overline{\cos\beta}|^2 + c\,r\,.\,\overline{\cos\gamma}|^2\} \times \text{const.};$$

the values of R parallel to the axes are then had by the equation

$$R^2 = \text{const.} \times a\,r \quad R^2 = \text{const.} \times b\,r \quad R^2 = \text{const.} \times c\,r,$$

which we shall express simply as $a^2, b^2, c^2$, so that the equation of the surface of elasticity will be of the form

$$R^2 = a^2\cos^2 X + b^2\cos^2 Y + c^2\cos^2 Z;$$

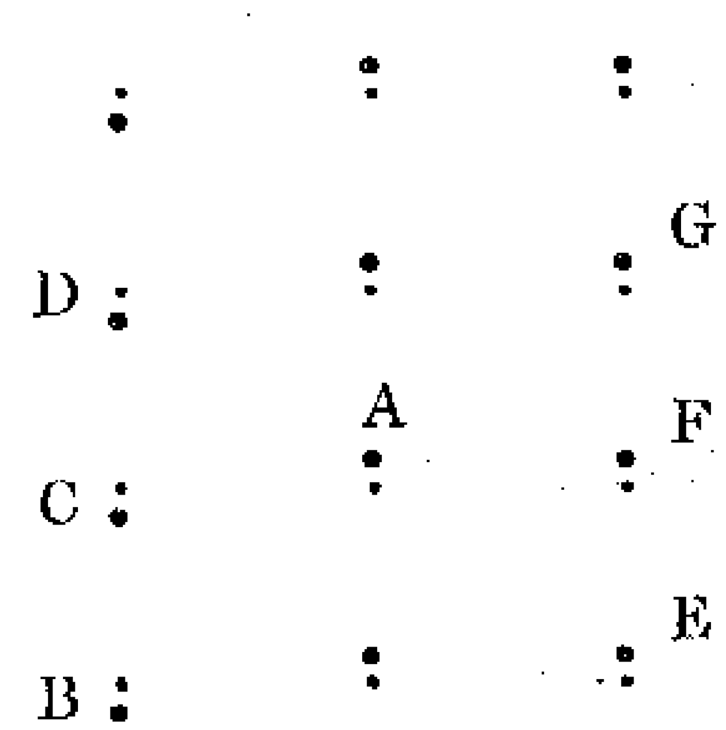
where X Y Z now stand for $\alpha\,\beta\,\gamma$, the angles made by R with the axes of co-ordinates.

"Let us now imagine a molecule displaced and allowed to vibrate in the direction of the radius R, and retained in that line, or at least let us neglect all that portion of its motion which takes place at right angles to the radius vector. Then the force of elasticity by which its vibrations are governed will be proportional to $R^2$, and the velocity of the luminous wave propagated by means of them, in a direction transverse to them (or at right angles to R), will be proportional to R."

Of this extraordinary proposition the accomplished author does not offer one syllable of proof or explanation. Whether Fresnel's writings are equally deficient, I am not aware; but another eminent mathematical writer, the present Astronomer Royal, after bestowing, as we may reasonably suppose, some degree of diligence on the study of Fresnel's papers, appears to have found nothing better in the way of a demonstration than the following (vide Airy's Tracts, 2nd edition, p. 341):—

"To explain on mechanical principles the transmission of a wave in which the vibrations are transverse to the direction of its motion.

"In figure adjoined let the faint dots represent the original situations of the particles of a medium, arranged regularly in square order, each line being at the distance $h$ from the next. Suppose all the particles in each vertical line disturbed vertically by the same quantity, the disturbances of different vertical lines being different. Let $x$ be the horizontal abscissa of the second row, $x - h$ that of the first, and $x + h$ that of the third; let $u\,u_1$ and $u'$ be the corresponding disturb-



ances. The motions will depend upon the extent to which we suppose the forces are sensible. Suppose the only particles whose forces on A are sensible, to be B, C, D, E, F, G (omitting those in the same line, as their attractions are equal and in opposite directions); and suppose them to be attractive, and as the inverse square of the distance; and the absolute force of each $= m$. The whole force to pull A downwards is

$$\frac{m(h+u-u_1)}{\{h^2+(h+u-u_1)^2\}^{\frac{3}{2}}}+\frac{m(u-u_1)}{\{h^2+(u-u_1)^2\}^{\frac{3}{2}}}$$
$$-\frac{m(h-u+u_1)}{\{h^2+(h-u+u_1)^2\}^{\frac{3}{2}}}+\frac{m(h+u-u')}{\{h^2+(h+u-u')^2\}}$$
$$+\frac{m(u-u')}{\{h^2+(u-u')^2\}^{\frac{3}{2}}}-\frac{m(h-u+u')}{\{h^2+(h-u+u')^2\}^{\frac{3}{2}}}.$$

"Expanding these fractions and neglecting powers of $u-u_1$ and $u-u'$ above the first, the force tending to diminish $u$ is

$$\left(1-\frac{1}{2^{\frac{3}{2}}}\right).\frac{m}{h^3}(2u-u_1-u').$$

Putting for $u_1$,

$$u-\frac{du}{dx}h+\frac{d^2u}{dx^2}\frac{h^2}{2},$$

and for $u'$,

$$u+\frac{du}{dx}h+\frac{d^2u}{dx^2}\frac{h^2}{2},$$

we find

$$\frac{d^2u}{dt^2}=\left(1-\frac{1}{2^{\frac{3}{2}}}\right)\frac{m}{h}\frac{d^2u}{dx^2},$$

an equation of exactly the same form as that for the transmission of sound. The solution therefore has the same form; and therefore the transversal motion of particles supposed here follows the same law, that is, it follows the law of undulation." And moreover, if the above were correct, the velocity of the luminous wave would be proportional to the square root of the force of elasticity in a direction transverse to the direction of the course of the wave.

Whether the above illustration—for at best it would be nothing more—is due to Fresnel or Mr. Airy himself, I am not aware: but the whole is erroneous from beginning to end. The mathematics not only fail to meet the case under consideration, but there is a palpable mathematical error in the process, which, even admitting the data, completely vitiates the result. I need but advert to the circumstance, that in the



approximate values substituted for $u_1$ and $u'$, it is assumed that $h$ is small with respect to $u$, or that the distances between the particles are small compared with their actual motions, a supposition entirely at variance with the assumed data of the problem. Hence it is plain that this supposed illustration is for every purpose entirely worthless.

Thus as we were compelled to assume the existence of the axes of elasticity, not only in the default, but in the face of evidence, so we are compelled to assume this rule as to the mode of calculating the velocity on a bare analogy to a case presenting the most striking difference from that under consideration, namely that of the direct transmission of an undulation when the vibrations are in the direction of transmission. We are not only compelled to assume the existence of *undulations* consisting of vibrations executed in directions perpendicular to the course of the wave,—respecting which it is not too much to say, that it is impossible for the mind to conceive the possibility of their existence,—but we are to suppose ourselves acquainted with an exact law to which they are subject*. Of the worth of such a theory I leave my readers to judge. The discussion of the remaining portion of it I must defer to another opportunity.

Liverpool, November 8, 1845.

* It is easy to conceive of transversal as the consequence of direct vibration, but I confess myself unable to conceive the possibility of there being a *surface* of transversal vibrations in the same phase—that of a sphere for example. The case of a stretched cord affords no analogy to guide us, for there the *wave* is in the direction of the motion. At all events, if the hypothesis of transversal vibration is to hold its ground, it must have much more thought bestowed upon it than it has yet received. The most painful circumstance connected with the later history of the undulatory theory, is the manner in which ideas, in themselves perhaps valuable as hints, have been dressed up into a settled theory. A truly philosophical mind, to which the idea of transversal vibrations had once suggested itself, would have set itself to work to discover, if possible, some method by which such motion could be conceived, and would not have rested satisfied so long as a doubt existed as to the perfect feasibility of the scheme. Thus it is that we may account for Young not having attempted to carry his first notion any further. He saw, no doubt, the difficulties by which the idea of transversal vibrations was beset, and was well-aware that till these were got over, it was hopeless to attempt to enter with any chance of success into the discussion of their nature and consequences. Fresnel, on the contrary, was satisfied with a series of possibilities, upon which he has built a theory, not only of no value in itself, as having nothing solid to rest upon, but from its crudity and manifold errors discreditable to himself and to the age by which it has been received.