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XXXI. On Mr. Earnshaw's Reply to the defence of the newtonian law of molecular action

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it to a new analysis. The result was so different from Mr. Keating's, that it became evident that the position which I had assigned it was a wrong one, and that in reality it was a quadruple salt consisting of silica united to the four bases, lime, alumina, iron, and magnesia.

The colour of Jeffersonite is dark olive green, passing into brown. It is foliated, and according to Keating, may be cleaved in various directions. The specimen in my possession is an imperfect four-sided prism; but the faces are not smooth enough to admit of measurement.

The lustre is resinous and almost semimetallic; the streak is gray, and the powder light green. It is rather harder than fluorspar, though softer than apatite. The specific gravity is 3·51. Before the blowpipe it fuses readily into a dark coloured globule: its constituents are

Silica	44·50
Lime	22·15
Alumina	14·55
Protoxide of iron	12·30
Magnesia	4·00
Moisture	1·85
	99·35

The constitution may be represented by the formula

$$4(\mathrm{Cal}\ \mathrm{S}) + 4(\mathrm{Al}\ \mathrm{S}) + 2(f\mathrm{S}^2) + \mathrm{Mg}\ \mathrm{S}^2;$$

so that it differs essentially in its composition from both pyroxene and amphibole.

XXXI. *On* Mr. Earnshaw's *Reply to the Defence of the Newtonian Law of Molecular Action. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., &c., Professor of Mathematics in the University of Edinburgh**.

On receiving the Philosophical Magazine for December, 1842, in which Mr. Earnshaw terminates his Reply to me by requesting an answer to four questions, I thought it right not to delay complying with his request. But I am not, of course, hindered thereby from discussing the rest of his paper. In point of fact, I am in arrear two answers to Mr. Earnshaw, viz. to part of his paper in the November Number, and to that before me. I do not, however, see that the former of these requires any rejoinder. I admit the first and second remarks in it, understood of relative, not of absolute displacements, but do not see that they bear on the question before us. To the third remark I have already replied.

Let me then confine myself to the last paper, and examine

* Communicated by the Author.

the different portions of it in order. 1. I am told that an argument I have used "is considered as a strong indication of my having allowed other motives than a desire of truth to influence me in bringing it forward." It is quite true I had other motives,—motives of delicacy. I did not wish to make a conclusion so evidently at variance with the premises stand forth prominently in a friendly controversy. In the first sketch of my paper it bore a more conspicuous place than I afterwards permitted it to do. Mr. Earnshaw remarks that it stands in his Memoir as a purely casual observation. I am glad to learn he intends it as no more, and an incorrect one of course. It appeared to me to be the summing up of the argument which I was replying to; for this is the way in which it is introduced; "and *consequently* whether the particles are arranged in cubical forms, or in any other manner, there will always exist a direction of instability." (Art. 5). If I am to understand that Mr. Earnshaw withdraws this, then have I attained the main object of my reply to the objection from instability, for he then withdraws the arguments on which it is founded. I trust to the discernment of my readers to decide whether in what I said I stepped "out of the line of legitimate argument."

Mr. Earnshaw goes on to say, "unfortunately for the Professor, in this instance he reaps no advantage by stepping out of the line of legitimate argument, as his objection is founded on the misconception that I have supposed the particles to be in equilibrium." I reply that I certainly did consider that the portion of Mr. Earnshaw's Memoir which relates to *instability* admitted that the particles are (or at least *may be*) supposed in their position of rest. Had I conceived Mr. Earnshaw would not allow this, I certainly should not have thought his objections worth answering, and I apologize for having troubled my readers with a reply. But I must not on that account refuse Mr. Earnshaw's request (top of p. 438), "to point out the link of his argument against Newton's law which violates that supposition" (viz. that the particles are not in their position of rest). It is to be found in Art. 11 of his Memoir (Trans. Camb. Ph. Soc. v. 7), the enunciation of which is as follows:—"To find the force of restitution when a particle is slightly disturbed from its *position of equilibrium*." In this article it is assumed that $\frac{dV}{df} = 0$, or the *force* on the particle parallel to any axis is zero; the particle being in its position of rest, and the other particles in their positions *not* of rest. This assumption is manifestly incorrect. It amounts to the following: *By moving all the particles of a system but one in* *any manner whatever, no force is put in play on the one which is not moved.* But Mr. Earnshaw refers to Art. 4 for proof. There is nothing about $\frac{dV}{df}$ in that article, so I suppose this is a misprint for Art. 3, where $\frac{dV}{df}$ is said to be equal to 0 when the position of the point is one of *neutral attraction*, i. e. when the force $\left(\frac{dV}{df}\right)$ *is* 0. But why it is also 0 when the point is in a position of *equilibrium* (a very different thing from neutral attraction when the other particles are not in their positions of rest), does not appear. If any one doubts whether $\frac{dV}{df}$, or the force parallel to an axis be really 0 or not, in such circumstances, I refer him to M. Cauchy's demonstration, that it is not in the *Nouveaux Exercices*, p. 190, or the *Exercices d'Analyse*, p. 304.

But that I am justified in my misconception (in supposing that Mr. Earnshaw's Memoir has reference to equilibrium), will, I hope, be admitted by any one who reads the hypothesis on which it is based. "It is assumed that the other consists of detached particles; each of which *is in a position of equilibrium*, and when slightly disturbed is capable of vibrating in any direction." Further, a point of neutral attraction and a position of equilibrium are used as synonymous, Arts. 3, 4, 6, 11, &c. And moreover, if equilibrium is a *failing case* in his objection (Art. 15), that "the equilibrium can only be stable in one plane," I am at a loss to know what the objection itself amounts to.

2. I turn now to the second portion of Mr. Earnshaw's Reply (p. 438). It is quite unconnected with the former. Relative to the first four paragraphs, I beg to direct attention to the previous objection of my opponent and to my Reply. The objection is this (Phil. Mag. for July, 1842, p. 47): the equation $2\pi^2\left(\frac{v}{\lambda}\right)^2 = \Sigma\left(A_r \sin^2\frac{rh}{2}\right)$ is such "that its right-hand member involves λ implicitly, in a manner which depends upon the arrangement of the molecules of æther, &c. Hence if there be dispersion in a medium on the finite interval theory, there must be dispersion *in vacuo* also." To this I replied by stating that this expression *in a medium* "must contain a term due to the action of the particles of matter." (Phil. Mag. for Oct. 1842, p. 264.) Mr. Earnshaw's argument assumes that it does not. Now in the Reply before me, it is attempted to be shown that the action of the particles of matter is included.

Clearly therefore the *form* of the function in this case, and where there are no particles of matter, is *different*, and Mr. Earnshaw withdraws his objection that they must be the same. I have only to add in answer to the suggestion, "Perhaps the Professor will point out what step of my investigation implies the existence of the absent particles:" *none whatever.* The symbol Σ may take in everything. And this puts the matter in the most simple light as regards Mr. Earnshaw's *inference.* If Σ in a medium is discontinuous, and *in vacuo* continuous, then have we the clearest reason why the expression *does* depend on h in the one, and *does not* in the other. The next paragraph has reference to another subject,—numerical verification. Of course no one considers an *error* of calculation as strengthening a theory. And I have already explained why the *processes* employed do so (p. 267).

3. The paragraph at the foot of p. 440, is a reiteration of Mr. Earnshaw's assertion that he has proved $v = 0$ or $n = 0$, &c. As this is of very great importance, the consequences being broadly hinted at by Mr. Earnshaw, I deem it requisite to state that I find three proofs of it. The first (Phil. Mag. for Nov., p. 341) depends on the assumption that v, v', and v'' are equal, which they are not. The second (Memoir, Art. 8), on the assumption that $\frac{d^2 \alpha}{d t^2} = \frac{d^2 \mathrm{V}}{d f^2} \alpha$, which it is not, as I have shown above. The third (Phil. Mag. for Jan. 1843, p. 24), on the assumption that an exponential function is inconsistent with the reductions effected by means of a circular one. To this I replied in my last*. The objection that $v = 0$ is so important that it ought not to be lightly passed over. If it is admitted, then a considerable portion of the writings of Cauchy and myself must be incorrect. No one I am sure will attach any weight to the arguments which I have mentioned, but lest any one should conceive the *possibility* of proving the function to be zero, I write it down,

$$n^2 = 2\,\Sigma \left(\frac{1}{r^3} - \frac{3\,\delta\,x^2}{r^5}\right) \sin^2 \frac{\pi\,\delta\,x}{\lambda}$$

taken throughout the whole medium. This expression can be summed so as to depend on a single definite integral with respect to r, viz.

$$n^2 = \Sigma\left(- \frac{\sin k\,r}{k\,r^4} + \frac{3 \sin k\,r}{k^3\,r^6} - \frac{3 \cos k\,r}{k^2\,r^5}\right)$$

where $k = \frac{2\,\pi}{\lambda}$.

* I may add that it *assumes* the existence of transverse and normal vibrations at the same time.

The sum of this function will depend on the distance between two consecutive particles: when that distance is exceedingly small (ε) it is $\frac{\pi k^2}{45\varepsilon}$ (abstracting from sign), as I shall have to prove in the prosecution of my arguments in reply to the two remaining objections to Newton's law of molecular action.

4. Mr. Earnshaw *explains* his equations which he asserts I have misunderstood. "I fear it will give to my letter an air *of great sameness* if I again accuse the Professor of misunderstanding what he attempts to criticise." The equations in their first form (Phil. Mag. for May, p. 373) are the same as Cauchy's well-known ones. But the coefficients, it appears, are very different. M. Cauchy's depend on the direction of transmission, Mr. Earnshaw's do not. This was the ground of my objection to the latter. Let us see the reply. "I ask how does the Professor know that these coefficients are not equal?" "Does it depend upon the direction of transmission? This question and a similar one for each of the other coefficients M. Cauchy has not answered, but I have answered it for myself in the negative, on experimental grounds."

It is quite true that Cauchy does not (in the Memoir alluded to) answer the question, for a most obvious reason. He could never have conceived it to admit of doubt. What is the problem they are solving? It is this: *To find the vibrations which are capable of being transmitted, when the position of the plane of the wave is given.* Had it turned out that the coefficients are independent of the position of the plane of the wave, one of two consequences must have followed; either,—1, that any vibrations whatever may be transmitted along a given direction or with a given wave; or 2, that only certain vibrations can be transmitted, whatever be the plane of the wave; both of which are contrary to experiment. I say then that Cauchy could not conceive it possible that his coefficients should be independent of the plane of the wave. But I add, that although he did not *give* their values, he left only one step to be supplied for their determination. The coefficients D, E, and F, are expressed at p. 38 (equations 70), viz.

$$D = 2\,R\,b\,c\,k^2,\ E = 2\,R\,a\,c\,k^2,\ F = 2\,R\,a\,b\,k^2,$$

$$\therefore D : E : F :: \frac{1}{a} : \frac{1}{b} : \frac{1}{c} :$$

that is, these coefficients are reciprocally proportional to the cosines of the angles which the perpendicular to the plane of the wave makes with the coordinate axes. It is evident therefore that they depend on the position of the plane of the wave. Since, then, Mr. Earnshaw's do *not*, are we to conclude with him, "that M. Cauchy's are at variance with experiment?" I believe very few persons will be found to join in this opinion. M. Cauchy's name, in the first place, is a sufficient guarantee for the accuracy of results which he has repeatedly obtained at different remote intervals. But, in the next place, the same problem has been solved, in different forms, by Mr. Airy (Tracts, Art. 110), by M. Naumann, by myself, by Mr. Green (Trans. Camb. Phil. Soc., 129), and lastly by Mr. O'Brien (Phil. Mag., March 1842, p. 210), all arriving at like results, viz. that the equation $\frac{d^2 \xi}{d t^2} = - n^2 \xi$, &c. correspond generally to three directions determined *relative to the front of the wave*, not to the axes of symmetry in the medium *only*, or in a medium of symmetry *at all*. [See Mr. O'Brien's paper, Phil. Mag., March 1842, p. 211.] But, lastly, Mr. Earnshaw says he effects his reductions on *experimental* grounds. On what kind of experiments? let me ask. In the next page (442) we find again, "The forms of the new equations of motion

$$\left(\frac{d^2 \xi}{d t^2} = -k_1^2 \xi, \ \frac{d^2 y}{d t^2} = -k_2^2 \eta, \ \frac{d^2 \zeta}{d t^2} = -k_2^2 \zeta\right)$$

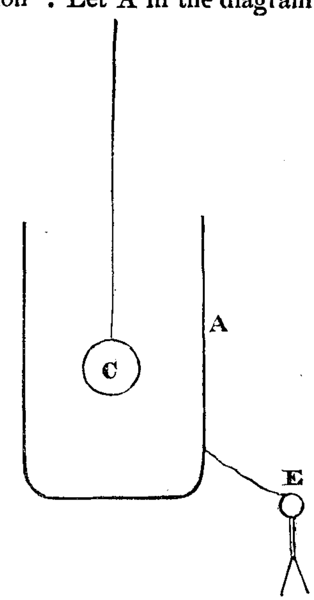
show that *these axes* are *axes of dynamical symmetry*,—those in fact which are better known as the axes of elasticity. Now from *experiment* we know that k_1^2, k_2^2, k^3 are constant quantities, i. e. independent of the wave's front." The *inference* which Mr. Earnshaw here draws from his equations is directly opposed to that drawn by *all* the authors quoted above. Other writers consider their symmetry to refer to *the front of the wave*. But, not to waste words on an error so obvious, let me ask Mr. Earnshaw a question. Are k_1^2, k_2^2, k_3^2 equal or unequal in uncrystallized media? If they are not, on what does their inequality depend? If they *are* equal; then can it be shown that $D = 0$, $E = 0$, $F = 0$, and $A = B = C$, so that the transformation is no transformation at all. If Mr. Earnshaw will carefully examine this remark, he will be convinced, I am sure, that the problem he conceives himself to be engaged in is the following:—"To find those directions within any medium, in which if a particle be disturbed, the resultant of the forces acting on it will tend to move it back in the same line in which the displacement is produced." This problem has been solved by Fresnel (*Mém de l'Institut*, 1824), by Herschel (Light, Art. 1001), and by A. S. in the Cambridge Mathematical Journal, vol. i. p. 3. Now this is a totally different problem from that against which Mr. Earnshaw brings his conclusions to bear. In the latter we are not so much concerned with how a particle must be displaced relatively to the *medium*, as with how it must be displaced relatively to *the front of the wave*. And the confounding of these two is (as I said before) the cause of Mr. Earnshaw's difficulties and the explanation of the inapplicability of his objections.

XXXII. *On Static Electrical Inductive Action. By* Michael Faraday, *Esq., D.C.L., F.R.S.*

To R. Phillips, Esq., F.R.S.

Dear Phillips,

PERHAPS you may think the following experiments worth notice; their value consists in their power to give a very precise and decided idea to the mind respecting certain principles of inductive electrical action, which I find are by many accepted with a degree of doubt or obscurity that takes away much of their importance: they are the expression and proof of certain parts of my view of induction*. Let A in the diagram represent an insulated pewter ice-pail ten and a half inches high and seven inches diameter, connected by a wire with a delicate gold-leaf electrometer E, and let C be a round brass ball insulated by a dry thread of white silk, three or four feet in length, so as to remove the influence of the hand holding it from the ice-pail below. Let A be perfectly discharged, then let C be charged at a distance by a machine or Leyden jar, and introduced into A as in the figure. If C be positive, E also will diverge positively; if C be taken away, E will collapse perfectly, the apparatus being in good order. As C enters the vessel A the divergence of E will increase until C is about three inches below the edge of the vessel, and will remain quite steady and unchanged for any lower distance. This shows that at that distance the inductive ac-



* See Experimental Researches, Par. 1295, &c., 1667, &c., and Answer to Dr. Hare, Philosophical Magazine, 1840, S. 3. vol. xvii. p. 56. viii.