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do not surprise us, for they result simply from the fact of the equality of the atomic volumes with respect to the isomorphous compounds, we meet with a number of most striking anomalies. For the chlorides of calcium, strontium and magnesium, the atomic volume is equal to 11 multiplied by 6, *i. e.* the number of equivalents of water of crystallization of those salts, but for the alums it is 11×25, while there are only 24 equiv. water. For the sulphate and borate of soda with 10 equiv. water the volume $= 11 \times 10$, but for the pyrophosphate with 10 equiv. water it is 11×11; and for the carbonate likewise, with 10 equiv. water, it is $9{\cdot}8 \times 10$; for the anhydrous carbonate of soda the factor 11 is taken, and for the hydrated carbonate 9·8; on the contrary, for the anhydrous sulphate of soda the authors prefer 9·8, and for the hydrated sulphate 11. The bromide of potassium $= 4 \times 11$, the bromide of sodium $= 5 \times 11$, the chloride of potassium $= 4 \times 9{\cdot}8$, the chloride of sodium $= 3 \times 9{\cdot}8$.

These instances we think will suffice to show that the hypothesis of Messrs. Playfair and Joule is not confirmed by an analogy of formulæ such as ought to be expected, and that the coincidence which does exist between the calculated and the observed densities merely result from the easy way in which the authors select at will the factor 9·8 or the factor 11, or even of combining them for one and the same body, as they have done in a large number of cases.

LXXXI. *Remarks on* Dr. Faraday's *Paper on Ray-vibrations. By* G. B. Airy, *Esq., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE communication which accompanies this was sketched before my attention was called to Dr. Faraday's leading paper in your Number for the present month. I need not to say that I read that paper with great interest and great pleasure. Yet I will ask your indulgence, and I am sure that I shall receive the forgiveness of Dr. Faraday, while I comment on the principal points of that paper somewhat critically. I am desirous of examining, or of suggesting grounds for examination by others, as to how far the fundamental suppositions of Dr. Faraday are necessarily limited by recognised phænomena, and as to how far the subject is metaphysical or physical.

The paper, as I understand, treats of two subjects:—

1. The possibility of explaining phænomena of radiation,

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more especially of light, by supposing that when there is no body obviously occupying the path of the light, &c., the vibrations which are assumed as the foundation of the undulations producing the phænomena are transmitted on the *lines of force* by what (for want of a received term) may be called *lateral shakes.*

2. The possibility of removing the idea of *substance* and substituting for it that of *centres of force.*

I shall treat of these in the order in which I have written them above.

1. With regard to the transmission of light through the planetary spaces.

Dr. Faraday and myself agree in receiving the undulatory theory of light with transversal vibrations, as applicable to those phænomena which present themselves in ordinary optical experiments. Without any wish therefore to dogmatize on this matter, I shall assume the undulatory theory in all the following remarks.

It is admitted that vibrations forming progressive undulations are required for the explanation of certain crystalline and other phænomena. But I must claim somewhat more. Progressive undulations (leaving the nature of their vibrations undetermined) are required to explain the phænomena of *diffraction*; and these progressive undulations must not be of the nature of radial shakes, where each shake derives its virtue or existence from the momentary influence of the distant origin, but they must be true waves, of which the mechanical characteristic is that the motion of a succeeding set of particles is determined by the relative motion of the preceding set of particles; the order of "preceding" and "succeeding" not being confined to a radial line or to any lines whatever, but being such that the motion of particles may be origin of motion to other particles extending round them through a very large solid angle. I defy any one to put together a theory of radial lines subject to lateral shakes which shall explain diffraction; and I say that it will be found absolutely necessary to admit, in the theory explanatory of diffraction, that each disturbance of particles produces a swell (to use language derived from the motion of water), which swell is propagated in all directions through at least a very large solid angle. Now the consequences of this are very important. Diffraction takes place in air; therefore the vibrating medium exists in air, and the undulations are transmitted by *it*, and not by radial shakes. As far as we can perceive air in its utmost degree of tenuity, it produces *refraction*; refraction inexorably requires for its explanation a change of velocity of the undulations, and a power at the same time of changing the direction of progress in a degree exactly corresponding to the change of velocity: these changes are in the simplest and most natural manner possible explained by the theory of true waves in which the swell produced by every particle is propagated in all directions through a very large angle, while (as I apprehend) it will be found somewhat difficult to modify a theory of radial shakes so as to explain them; therefore I conceive it demonstrated that the propagation of true waves takes place through the air to its utmost borders. Beyond the existence of sensible air we can make no experiments; and I am free to concede that if we supposed the air and its accompanying æther [if different] to terminate at a distinct frontier, and if we supposed the transversal shakes to be propagated radially through the planetary spaces to that frontier, and then supposed each shake, as it presented itself, to be the origin of a spreading swell through the æther, the phænomena of light would be explained. But here a remarkable circumstance forces itself on our minds. A moment's consideration will show that at this frontier the course of the light will be subject to refraction, in just the same way as if the incident light had consisted of waves, and following the same law as depending on the velocity of propagation. Now it is abundantly established that at the boundary of our air there is no sensible refraction, that is, that the velocity of the propagation is not sensibly altered. Now is it not a very curious circumstance that there should be a system of radial shakes outside and a system of true waves inside which propagate the undulations with *exactly* the same velocity? Is there any philosopher who would be inclined to receive as true this suggestion of two independent causes of velocity, and this exact adjustment of independent velocities, when the adjustment will *necessarily* exist if the same vibrating medium or æther occupies all space? Not I, certainly. However well-disposed I might be to admit any such *saltus* of nature at the surface of glass or crystal where the phænomena of light are totally changed, I cannot bring myself to believe in it as existing either *through the air* where the change of phænomena is gradual, or *at the limits of the air* where there is no change at all. In a word, I must have the same theory of light for the planetary spaces as for the air in which our experiments on diffraction are made; and that theory must be the theory of true waves.

I do not insist on the novelty of the conception, that lateral influences take place in a travelling succession along a radial line, in a manner different from anything whatever that we know. I am perfectly aware that the theory is merely sketched by Dr. Faraday as the result of hasty thought, and that it might be in some measure modified in its details on further consideration by its author. But while the distinctive features of the theory are retained, it will be, for the reasons which I have given, inadmissible to me. The theory is however, in my opinion, a fair subject for the consideration of the natural philosopher.

2. With regard to the substitution of centres of force for matter.

This speculation, in its general character, differs little from the celebrated inquiry regarding Substance and Accidents. In the latter the question is, whether, when we have found a lump of matter to possess certain form, colour, weight, and other properties, we can satisfy ourselves by saying that this lump of matter is a combination of such a form, such a colour, such a weight, &c.? And the answer has usually been that the mind is not satisfied unless we describe the lump of matter as *something* possessing the properties of such a form, such a colour, such a weight, &c. In the speculation before us, the question is, whether instead of matter which exerts certain actions upon other matter, we may assume that there is nothing but a number of centres of force producing these actions? I think that most persons would say that the mind is not satisfied with this assumption, and that it requires the idea of a *something* as foundation for these centres of force. But this question, in my opinion, is purely a metaphysical question, entirely removed from the province of the natural philosopher.

To a great extent I am willing to admit that the supposition of centres of force is satisfactory. Mechanical attraction or repulsion (including weight under the former term), colour, radiation of every kind where the existence of something intermediate between the radiating body and the body receiving the radiation is not apparently demonstrated; all these may, I think, be received without scruple as the results of mere centres of force. But there is one property, to which by chance Dr. Faraday has not alluded in his paper, that appears to me irreconcileable with the notion of centres of force; I mean the property of *inertia*. And I believe that the general notion of *substance* is really founded upon the perception of *inertia*. Construct for any one a mass of matter possessing invariable form, colour, and other attributes, even attraction; if he finds that this mass yields to muscular or other force without perceptible resistance (it matters not whether it continually retain the same velocity or not), he will scarcely scruple to admit that there is no *substance*. While the resistance to force remains, it seems scarcely possible to get rid of the idea of *substance*.

Perhaps it may be said that even inertia may be represented by centres of force, only supposing the development of the force to be dependent in some way upon time. Such, however, is not the character of forces that we know best; and the introduction of this idea appears to give greater complexity to the force-centre-theory than is given by the idea of substance in the material theory.

Now I say that, in the wave-theory of light, and in all theories of waves where the amplitude of the vibrations does not diminish transcendentally with relation to the distance passed over by the wave, the supposition of inertia (or something equivalent) is absolutely necessary. This will be evident to any mathematician who compares the results obtained from the different suppositions of inertia or no inertia. For instance; in the theory of the transmission of heat by conduction, no inertia is supposed; the equation then has the form $\frac{dh}{dt} = A \cdot \frac{d^2 h}{dx^2}$, of which the solution (supposed to be periodic) is, $h = B \cdot \varepsilon^{-\alpha x} \cdot \cos(nt - \beta x)$. But in the theory of the transmission of sound, where the vibrating particles are supposed to possess inertia, the equation is $\frac{d^2 X}{dt^2} = A \cdot \frac{d^2 X}{dx^2}$, of which the solution (similarly restricted) is $X = B . \cos(nt - \beta x)$. The former result certainly does not represent anything like the law of diminution of light; the latter does represent its general constancy of intensity (the distance of the source being very great). I infer therefore that the supposition of inertia is absolutely necessary.

Combining this inference with that obtained above regarding the universality of undulations in space, I am led to the conclusion that all space with which we are acquainted contains something which exhibits the property that we call *inertia*. The reasons which have led me to this conclusion appear to me decisive, but I admit them to be fair subjects for doubt and discussion by natural philosophers. Whether we are to infer from this that there is *matter* through all space, is, in my opinion, a metaphysical question.

But the remarks that I have just made will enable me to answer one paragraph of Dr. Faraday's paper. "Perhaps I am in error in thinking the idea generally formed of the æther is that its nuclei are almost infinitely small, and that such force as it has, namely its elasticity, is almost infinitely intense. But if such be the received notion, what then is left in the æther but force or centres of force?" To this I reply, that *almost infinitely* has no meaning but *finitely*, and therefore that the supposed æther, under this description, is precisely in the same category as all other fluids. But I add, in regard to the latter sentence, that the mathematical considerations which I have detailed above, show that there is something in the æther besides force or centres of force, namely *inertia*. And I repeat the expression of my own opinion, that it is easier to conceive this as indicating *substance* (however obscure the idea may be), than to frame a system of laws applying to centres of force which shall represent its effects equally well.

I am, Gentlemen,
Your obedient Servant,
G. B. Airy.

Royal Observatory, Greenwich,
May 12, 1846.

LXXXII. *Explanation of the Vorticose Movement, assumed to accompany Earthquakes. By* Robert Mallet, *C.E., M.R.I.A., Ph.D., &c., Secretary of the Geological Society of Dublin**.

IN our progress to the ascertainment of physical knowledge, the removal of error is next in importance to the discovery of that which is true, inasmuch as by the former the road is cleared, by which the difficult journey towards truth is to be accomplished. The substitution, therefore, of a true for a false explication of phænomena, however in themselves unimportant, is never to be neglected; and with this view it was that I some time since addressed myself to the discovery of what I believe to be the true explanation of a somewhat singular and heretofore puzzling circumstance attendant upon the effects of earthquakes upon buildings, which has been frequently observed, and has been hitherto explained, so far as it has been attempted to be explained at all, by the assumption of a vorticose or gyratory movement having been in some inexplicable way given to the ground. The phænomenon alluded to, is the displacement of the separate stones of pedestals or pinnacles, or of portions of masonry of buildings by the motion of earthquakes, in such a manner that the part moved presents evidence of having been *twisted* in its bed *round a vertical axis*.

The first notice I find recorded of such a peculiar motion, is in the Philosophical Transactions, in an account of the

* Communicated by the Author.