



LXXXVI. On some passages in Mr. Ivory's remarks on a memoir by M. Poisson relating to the attraction of spheroid

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99. *Cynanthus Lucifer*.

Golden green; throat amethystine; the feathers elongated and narrow; tail short, the feathers pointed; bill curved.

Table land. Temiscaltepec.

This is an aberrant species; allied, by its curved bill, to *Cy. bifurcatus*.

G. LAMPORNIS. *Swains. in Zool. Journ.* No. 10.

100. *Lampornis amethystinus*. Sw.

Green; chin and upper part of the throat amethystine; ears black, margined above with white; tail black. Female?

Table land. Temiscaltepec. Real del Monte.

Total length, 5; bill, 1; wings, $2\frac{7}{10}$; tail, $1\frac{1}{2}$.

101. *Memotus Mexicanus*.

Head and neck cinnamomeous; back and wings green; ear feathers lengthened, black tipped with blue; beneath the eye a cerulean spot; under plumage greenish white.

Temiscaltepec.

Much smaller than the Brazilian species: on the throat are two small tufts of black feathers, longer than the others; a character which is not, however, peculiar to this species.

LXXXVI. *On some Passages in Mr. Ivory's Remarks on a Memoir by M. Poisson relating to the Attraction of Spheroids.*
By G. B. AIRY, Esq. A.M., Lucasian Professor of Mathematics in the University of Cambridge.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN a paper printed in the last Number of the Philosophical Magazine and Annals, Mr. Ivory has coupled my name with terms which have never before appeared in the pages of your Magazine, or (I will venture to say) in those of any other scientific Journal. After such an attack, I am entitled to ask that you will insert in your next Number my answer to the accusation which Mr. Ivory has brought against me in so undisguised a manner.

When I read this article, I was grieved to think that I had been the cause (I think I need not say the unintentional cause) of irritating Mr. Ivory's feelings to such a degree, as to occasion the use of the opprobrious epithets alluded to. Though conscious that I had used no language, except that of courtesy towards Mr. Ivory, I referred immediately to the note to my paper in the Philosophical Transactions, of which he complains so bitterly. In it I found nothing which could justify the torrent of spleen that Mr. Ivory has vented against me. And I profess that I have said nothing in that note
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which I would not willingly hear from any one, as far below me, in all respects, as I am below Mr. Ivory.

But that your readers may judge of the provocation that I have given, I will lay before them the article which is made the ground of animadversion. The note to my paper in the Philosophical Transactions, of which Mr. Ivory complains, is as follows:

“I have not considered the second condition of equilibrium given by Mr. Ivory in the Philosophical Transactions for 1824, as the reasoning upon which that gentleman has founded the necessity of such a condition, appears to me altogether defective.”

Upon which Mr. Ivory (Phil. Mag. and Annals, N.S. vol. i. p. 329, note) has made the following remarks:

“In the Phil. Trans. 1826, p. 557, there is a note of Mr. Airy, very injurious to me. He is treating of spheroids of variable density, and evidently misapprehends my conditions of equilibrium, which I have always limited to the case of homogeneity. The R. S. are not responsible for the accuracy of what they publish; but I apprehend few instances will be found so injurious to an individual, cast upon the public on the authority of mere assertion, and arising from mistaken notions. But I console myself because I know with the certainty of demonstration, that Mr. Airy’s problem, admitting that any practical utility could be attached to it, is not solved, and that it cannot possibly be solved except by my theory, and indirectly, with the help of that law with which he so flippantly finds fault. What a difference between the supercilious importance of the Cambridge Professor, and the candid expositions of M. Poisson!”

I will omit mention, for the moment, of those sentences in which Mr. Ivory says that I am mistaken on the mathematical points, and will allude at present only to those in which he attacks my character as a gentleman. I will therefore state, that in my paper in the Phil. Trans., it was my business not to *investigate* conditions of equilibrium, but to make use of those already known. The equations which are best known are the one (or rather the two) commonly used, and that which Mr. Ivory has suggested. For the latter I saw no foundation, and I contented myself with a simple statement to that effect: the object and the limits of my paper not allowing me to enter into details. But, I should not have made even this statement, did I not think that the character of Mr. Ivory demanded it. I could mention the name of another writer who has added one to the common equations, but whose character did not seem to require the same compliment

which I paid to Mr. Ivory's. This is merely to account for the introduction of the note. What foundation Mr. Ivory can find for the charges of "injury on the authority of mere assertion," "flippancy," and "supercilious importance," I cannot imagine. I have simply stated my difference of opinion from Mr. Ivory on a point which I was unable, from the nature of the paper, to explain at greater length. The note is now before your readers; and I appeal to them whether I have said any thing which can justify the use of such expressions. Upon the whole, I think that I have reason to complain of the terms in which Mr. Ivory has mentioned me, as most improper, and most unworthy of the respect which a gentleman ought to have for himself, as well as for any other who claims that title.

The only probable cause for Mr. Ivory's anger, independent of our difference of opinion, appears to be my omission of the reasons for that difference of opinion. The cause of that omission I have explained: but that Mr. Ivory may have for the future no ground of complaint, I shall state here my reasons for disagreeing with him. Mr. Ivory's opinion was first published in the Phil. Trans. for 1824, p. 101—108; but he has explained it in nearly the same terms in the Philosophical Magazine for July 1826. I shall request your readers, therefore, to refer to page 4 of that Number; and I shall begin my remarks at line 25. By the common theory it is known that if the forces which act on a fluid satisfy a certain equation, any level surface (*couche de niveau*) may, by the removal of a part of the fluid, become the external surface of the remaining fluid which is still in equilibrium. But this is true as a general proposition only when the forces are expressed by the same functions of the coordinates, whether the quantity of fluid be great or small. It appears then, from the common theory of fluids, that Mr. Ivory's proposition advanced in the sentence beginning in line 25, is certainly true, if there be no mutual attraction of the particles; but is not certainly true, if there be such attraction. It may happen, and in the particular case of which he treats it does happen, to be true, when the mutual attraction is taken into account, but this is quite accidental. The two following sentences are elucidations of the preceding: the latter of them is of course to be taken with the same restrictions as that of which I have treated; namely, it is to be supposed that the particles have no mutual attraction. With this supposition the reasoning of the next sentence, which depends entirely on the existence of attraction, falls to the ground. And after much consideration I am quite unable to see any force in the reasoning upon which I have commented.

Whatever

Whatever may be the meaning of the expression "similar forces," I am quite unable to discover in the sentence beginning at line 22, any grounds for the inference in line 25. Perhaps I may place the question in a clearer point of view, in the following manner. If a fluid mass in equilibrium, acted on by any external forces, and by the mutual attraction of its particles, were inclosed in a thin shell of the same shape, there would be no pressure on the shell. Or, if a pressure were communicated to the fluid (by slightly contracting the shell, suppose, or by a force acting on a small piston), the pressure on a unit of surface would be the same in every part of the shell. Now suppose some more of the fluid to be spread on the shell, and (from the action of the external forces, the attraction of the inclosed fluid, and the mutual attraction of its own particles) to receive the form of equilibrium. I do not see the slightest reason to believe that the pressure on the shell, produced by this superincumbent matter, would be every where equal. Though the whole force which acted on every particle in the original external surface must have been perpendicular to that surface, and consequently the whole force arising from the external forces and the attraction of the original fluid acts in a direction perpendicular to the shell upon the exterior particles in contact with the shell: yet there is another force not considered; namely, the attraction of the new stratum on its own lowest particles; and if this can be resolved into a perpendicular and a tangential force, the pressure on different parts of the shell *must* be unequal (from the property of equal transmission of pressure in all directions). Yet the whole fluid would still be in equilibrium, without owing its equilibrium to the existence of the shell, if the variations of the internal pressure on the shell, produced by the attraction of the external fluid on the internal, corresponded exactly to the variations of the external pressure.

Now I need not point out to Mr. Ivory that this is the case when the equation of integrability is satisfied; which holds with all the forces with which we have to do. The fluid therefore may be in equilibrium, and yet the surface which was the external surface may, for all that we can discover, be a surface of unequal pressure; and if this be admitted, the question is ended. I may remark, that even if Mr. Ivory had proved every thing which he has stated as far as line 41, the inference in the next sentence would have been unjust. "If the action of the exterior stratum does not disturb the equilibrium of the interior fluid body, this can happen only because the resultant of the attractions of the exterior matter upon any particle within the stratum is evanescent." It will be enough
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to remind Mr. Ivory, that the equilibrium would not be disturbed if the resultant of these attractions were a force expressed by a function of the coordinates of the attracted point, similar to the function expressing the previously-acting forces (including the attraction), and that there does not therefore appear to be any reason for saying that it must be evanescent.

These are my reasons for not admitting Mr. Ivory's new equation. I have stated them plainly, but I hope not uncivilly: if I am wrong, I shall be glad to have my errors pointed out in the same manner. I trust that I shall not be exposed to the charge of presumption for holding the opinion of Laplace and Poisson, in opposition to that of which Mr. Ivory is (I believe) the sole advocate.

But Mr. Ivory says that I misapprehend his conditions, which he has always limited to the case of homogeneity. When I wrote the note in question, I was perfectly aware that the algebraical investigations which Mr. Ivory had founded on his equation were confined to homogeneous fluids; but I did not so clearly know that the reasoning was equally restricted. I have since examined the reasoning with some attention; and I declare, that I cannot discover any part of it which is not as applicable to heterogeneous fluids as to homogeneous fluids. Judging from my own feelings, I think that the scientific world would be much obliged to Mr. Ivory, if he would point out the parts of his reasoning which are not applicable to heterogeneous fluids.

Mr. Ivory "consoles himself because he knows with the certainty of demonstration, that my problem is not solved, and cannot possibly be solved except by his theory." I console myself by thinking that Mr. Ivory has not reasoned with his usual accuracy upon a point which is somewhat abstruse, and by believing that my problem is solved (as far as such a problem can be solved) without the assistance of Mr. Ivory's equation.

I had intended to confine my remarks to the offensive note in which Mr. Ivory has treated me so unhandsomely. But as Mr. Ivory has in the preceding page mentioned another point on which we are at variance, I will endeavour to lay before your readers a more complete statement of the argument than he has given. I think it proper to say, that I have no reason whatever to complain of the terms in which he has there mentioned my name.

The first part of my paper (as Mr. Ivory has correctly stated) is employed in attempting to prove that Laplace's fundamental equation (*Méc. Céleste*, liv. iii. No. 10) is exactly demonstrated,

monstrated, and for all kinds of spheroids differing little from a sphere. The only limitation of its generality is, that the sine or tangent of the angle made by the spherical and spheroidal surfaces at their intersection, must be expressed by a finite multiple of α ; which condition is satisfied when y is expressed by any function, rational or irrational, that never makes $\sqrt{1-\mu^2} \cdot \frac{dy}{d\mu}$ or $\frac{1}{\sqrt{1-\mu^2}} \cdot \frac{dy}{d\omega}$ infinite. I have only to add, that this part of the paper is little more than a filling-up of the sketch given by Laplace in one of the last books of the *Mécanique Céleste*.

I cannot at present enter on the discussion of a very nice and abstruse point: I shall merely remark, that the difficulties which Mr. Ivory has found (see his paper, Phil. Trans. 1812, p. 16), appear to arise from the separation of $y' - y$ into two parts. For the rest I must beg leave to refer the reader to my paper in the Cambridge Transactions, vol. ii. "Now," says Mr. Ivory, "admitting that the equation in question is accurately and numerically proved, it seems impossible to deny that the series of terms deduced from it is numerically equal to the distance between the surfaces of the sphere and spheroid." With this I perfectly agree: but Mr. Ivory afterwards says, "Mr. Professor Airy, by supporting the fundamental equation without restricting it, and at the same time denying the unavoidable consequence, has only introduced new inconsistencies," &c. I can only infer from this that Mr. Ivory has not read the whole of my paper. However little the trouble of reading it might be repaid, it is not right to make such remarks on the connection of the first and the last parts, without examining or alluding to the subject which occupies the body of the paper. In the beginning I have endeavoured to show that the equation $-\alpha \frac{dV}{dr} = \frac{2\pi\alpha^2}{3} + \frac{V}{2}$ is generally true. From this the equation $4\alpha\pi\alpha^2y = \frac{U^{(0)}}{a} + \frac{3 \cdot U^{(1)}}{a^2} + \frac{5 \cdot U^{(2)}}{a^3} + \&c.$ is derived by an unobjectionable process. But this equation as it stands is useless, unless we can resolve $4\alpha\pi\alpha^2y$ into a series of terms, distinguished by the same peculiarities which separate those on the other side of the equation. If it is not possible to resolve $4\alpha\pi\alpha^2y$ into more than one such series, the corresponding terms must be equal: if it is possible to do it in more than one way, nothing can be inferred from the equation, but the equality of the whole quantity on one side to the whole quantity on the other side. It is therefore necessary to prove that this resolution can be effected

fected in only one way. Now the most important part of my paper is occupied in endeavouring to show that the proof offered by Laplace is insufficient, and in giving a demonstration not liable to the same objections. Laplace's proof professes to be general; mine applies only to the cases in which y can be expressed (at any rate approximately) by a rational function of the coordinates. Where then is the unavoidable consequence of which Mr. Ivory speaks? I have endeavoured to show that the fundamental equation of Laplace is general, but that its application to the theory of the attractions of solids, is restricted by the limited nature of the proof of one of the subsequent steps. In this I can discover no inconsistency, nor do I perceive that I have embroiled the subject with new difficulties. I have only done with regard to one point, what Mr. Ivory has done respecting another: I have endeavoured to show that a demonstration professing to be general, is unsatisfactory, and have substituted one which appears, though more restricted, to be better founded.

I am sorry that I should have come in contact with Mr. Ivory, for the first time, on an occasion so disagreeable. I am not desirous of appearing in a public controversy of this nature; and under any common censure I should have remained quiet. But the manner in which I have been mentioned is so gross, and the name of the person who has mentioned me stands so high, that I have no other resource than to lay my defence before all who have read the accusation. I am aware, that the Editors of a *Philosophical Journal* can take little pleasure in inserting the squabbles of quarrelsome writers; and therefore, whatever further provocation may be offered, I shall not trouble you again with my complaints.

I am, Gentlemen, yours, &c.

Trinity College, Cambridge, May 9, 1827.

G. B. AIRY.

LXXXVII. *On a new Mineral Substance, proposed to be called Murchisonite.* By A. LEVY, Esq. M.A. F.G.S.

IN looking over some specimens of the conglomerate of the new red sandstone, which Mr. Murchison had brought from the neighbourhood of Dawlish, and which he was so good as to show me, I observed, in many of them, a felspar-like laminated substance, with a peculiar nacreous cleavage, which induced me to believe, it might differ from common felspar. Upon further examination I found that it had cleav-

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