



# IV. A reply to Professor Kelland's letter of November 1842

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2ndly. Professor Kelland now confesses that his fundamental equations are essentially erroneous. Mr. Earnshaw and myself have fully confuted his strange attempt to set up the plea of *misprint* and *mistranscription*.

3rdly. Professor Kelland does not defend his equations in page 159 of the Cambridge Transactions, vol. vi.

4thly. Professor Kelland most unquestionably denies the existence of a normal vibration in the Edinburgh Transactions, vol. xiv. page 396; and neglects taking the normal vibrations into account in that memoir, which plainly proves how little he *then* understood of the question of transversal and normal vibrations.

I now therefore am entitled confidently to assert the accuracy of all my assertions respecting Professor Kelland's investigations.

In conclusion, I beg to state that I have nowhere charged Professor Kelland with dishonesty towards M. Cauchy; and that what Professor Kelland calls "my attack upon him" is only my *defence* against his *unprovoked* attack upon me; and that when he took upon him to make a *public* attack upon me, he had no right to expect that I would have been content with a "*private explanation*."

Dec. 2, 1842.

IV. *A Reply to Professor Kelland's Letter of November 1842.*  
By S. EARNSHAW, M.A., Cambridge, and the Rev. M. O'BRIEN.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

PROFESSOR Kelland in your present Number [Dec. 1842.] seeks to avoid the consequences of our arguments by stating that we have been led astray by "a misprint, or rather a mistranscription," and that the quantities we have animadverted upon "are *not* equal," and that he has "supposed the axis of *y* to be that along which transmission takes place." We shall reply to these in a reverse order.

1. The Professor says he has supposed the axis of *y* to be that of transmission.

If the Professor will refer to p. 161 of his Memoir (Camb. Phil. Trans. vol. vi.), he will find that he there states the contrary to be the case in these words: "so that we might at once suppose the direction of transmission to be the axis of *y*, and put  $\delta y$  for  $\delta \rho$ ; this, however, *I shall not do*;" and as a proof that he did not, we find him near the bottom of the same

page writing  $\delta \rho = \delta x \cos \theta + \delta y \cos \phi + \delta z \cos \psi$ , which of course he would not have done had  $\delta \rho$  been equal to  $\delta y$ . At page 166 he gives  $\delta \rho$  the value  $\delta x$ , that is, he supposes *the axis of  $x$*  to be the axis of transmission; and a little below he gives  $\delta \rho$  the value  $\delta z$ , that is, he supposes *the axis of  $z$*  that of transmission; thus deducing the corresponding results from his previous investigation in a manner which can only be justified by the supposition that the axis of  $y$  was not the axis of transmission. At page 179 he for the *first* time really does suppose the axis of  $y$  to be that of transmission; and this hypothesis is introduced exactly as a person would introduce it who had all along supposed his previous investigations independent of the hypothesis; his words are, "suppose, then, to *fix ideas* that the wave is transmitted along the axis of  $y$ ." Now if this had been the hypothesis through the previous part of the paper, why was it here necessary to state it for the first time in order to fix ideas? The ideas would have been fixed upon it from the first. Is it not clear from this sentence, after what is said above, that the Professor did not suppose the axis of  $y$  to be the axis of transmission in the early part of his paper, to which only our remarks were directed?

2. The Professor says that the quantities we have quoted are not all equal to  $n^2$ , but that the second of them is equal to  $n_i^2$ .

It is at least very singular that no such quantity as  $n_i^2$  is mentioned or alluded to, or its existence implied in the whole memoir; which is almost inconceivable if the Professor's MS. had contained it. Now it so happens that the integrals of the three differential equations under discussion are written down in the middle of page 163, and those integrals are correct, only on the supposition that the coefficients *are all equal*; unless indeed we suppose that the transcriber and printer have made another mistake. In lines 2 and 13 the author twice informs us what is the value of  $n^2$ , the information being necessary in order to make his readers understand the meaning of the integrals, but he makes no allusion to an  $n_i^2$ , though, had there been such a quantity, information of its value was as necessary as in the case of  $n^2$ . Line 4 of the same page is irreconcilable with line 2, unless the author had supposed the coefficients all equal. And, had they been unequal, there would have been *two* velocities of transmission, a circumstance which it would have been absolutely necessary to notice to prevent confusion of ideas, when the author at pages 164, 165, 167, ... speaks of *the* velocity of transmission.

3. In the places we have quoted, and some others, the equality of the coefficients is implied in such a manner that

no change short of a total alteration of the whole memoir can correct it. For instance, the discussion of the particular case beginning at the bottom of page 158, which is made the foundation of the treatment of the general case, is inapplicable except on the supposition of equality of coefficients, for the author effects his reductions on that supposition, and that the integrals are

$$\alpha = a \cos (nt - \rho), \beta = b \cos (nt - \rho), \gamma = c \cos (nt - \rho);$$

and the transference of these, unaltered, into page 163 as the proper integrals for the general case, implies beyond the possibility of a doubt that the author believed the coefficients to be equal in the general case as in the particular case. It is hard to understand, after this statement, how we, or any other person reading the Professor's Memoir, could have conjectured, that in supposing the coefficients equal, we had been led astray by "a misprint or a mistranscription."

It is clear, the *arguments* against what we have advanced in our previous communications, are exhausted; we shall therefore consider this letter as concluding our correspondence on the subject.

We are, Gentlemen,

Your obedient Servants,

S. EARNSHAW.

M. O'BRIEN.

Cambridge, Dec. 2, 1842.

P.S. Dec. 3, 1842.—Perhaps we ought to have noticed the Professor's statement, that he has proved in his Memoir (p. 180) that the three quantities are *unequal*, on the *equality* of which our arguments, in your Magazine for November, entirely depend. The Professor certainly deceives himself in thinking that he has *proved* them unequal; for what he *has* proved is simply that  $v^2 + v'^2 + v''^2 = 0$ ; the only *legitimate* inference from which equation is, that  $v = 0$ ,  $v' = 0$ ,  $v'' = 0$ , which is precisely what we have proved from the Professor's equations of motion. But the Professor, instead of drawing this inference, has imagined from it that  $v'$  is impossible, an inference he was not *at liberty* to make, because it *violates the hypothesis* on which he had effected the reductions and transformations in the former part of his paper, upon the truth of which the correctness of the equation  $v^2 + v'^2 + v''^2 = 0$  depends. If  $v'$  is impossible the integral of his second equation of motion ought to have been an exponential; and then the case considered at pages 158, 159, is dis severed from the general case; and so the whole memoir would require to be remodelled.