



LXVI. On spherical waves in an elastic fluid, in reply to Mr. Stokes

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To cite this article: Rev. J. Challis M.A. F.R.S. F.R.A.S. (1849) LXVI. On spherical waves in an elastic fluid, in reply to Mr. Stokes, Philosophical Magazine Series 3, 34:231, 449-450, DOI: [10.1080/14786444908646264](https://doi.org/10.1080/14786444908646264)

To link to this article: <http://dx.doi.org/10.1080/14786444908646264>



Published online: 30 Apr 2009.



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LXVI. *On Spherical Waves in an Elastic Fluid, in reply to Mr. Stokes. By the Rev. J. CHALLIS, M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*.*

THE question which has been discussed by Mr. Stokes and myself in several recent Numbers of this Magazine, is of the following nature. On supposing the waves in a compressible fluid to be spherical, I deduced from that supposition, by reasoning given at length in the Philosophical Magazine for last February, a result inconsistent with one of the fundamental principles of hydrodynamics, viz. that of constancy of mass. Hence I concluded that the supposition of spherical waves is inadmissible. Mr. Stokes undertook to dispute this inference. Now although by an acknowledged rule of logic, the inference could not be set aside except by showing some fallacy in the reasoning which conducted to the absurdity, Mr. Stokes, in three attempts to set it aside, has not once alluded to any step in the reasoning. In the first attempt he produced an argument which took for granted the very point in dispute; in the next he denied, without giving any reason, what was altogether undeniable; in the third attempt (*Phil. Mag.* for May) he admits what he before denied, and denies, again without assigning a reason, what in the second attempt he admitted. The denial in this instance refers to the possibility of the propagation of a solitary wave of arbitrary condensation and constant type. I infer the possibility from the principle of discontinuity. Mr. Stokes calls this inference a gratuitous assumption, without making the slightest allusion to the principle on which it rests; and yet he has drawn a like inference from the same principle in the same way. (*Phil. Mag.*, vol. xxxiv. p. 54. l. 27-31.)

With respect to my having deduced only a *part* of the results to which the supposition of spherical waves leads, I am able to give a very good reason for not proceeding further. I obtained, as already stated, from that supposition, by reasoning of which Mr. Stokes has not shown, and I am unable to perceive, the fallacy, a result inconsistent with one of the fundamental principles of hydrodynamics. The supposition I consider to be thereby condemned. If I allowed myself to qualify this course of reasoning by another from the same supposition, I should proceed in direct opposition to an incontrovertible rule of logic, and neutralize a highly important and significant result.

Mr. Stokes appeals with great confidence to the results he

* Communicated by the Author.

Phil. Mag. S. 3. Vol. 34. No. 231. June 1849. 2 G

has obtained by two courses of reasoning in the *Phil. Mag.*, vol. xxxiv. pp. 54-57 and 57-59. I cannot concede that these results have any weight *against* my position, because the reasoning from which they are derived takes for granted the question in dispute; but I may adduce them in favour of it so far as they exhibit inconsistencies. The first argument, which professes to be a *general* consideration of a solitary wave of arbitrary condensation, conducts to the result that the sum of the condensations is exactly equal to the sum of the rarefactions. Now if the reasoning be restricted to the case in which the sum of the condensations is equal to the sum of the rarefactions by the original disturbance, it ceases to be general, and the result is a mere truism without meaning; and if it be not so restricted, it is impossible that the result can be true. Mr. Stokes's argument cannot escape from this dilemma. The second argument, which applies to a case in which condensation prevails over rarefaction, is included in the general argument above mentioned, if that be of any value, and its leading by a different process to a different result is only another phase of contradiction.

As I consider that this hydrodynamical question has now been so fully discussed that it is not likely to receive any additional elucidation, as far as I am concerned the discussion is closed.

Cambridge Observatory,
May 23, 1849.

LXVII. *On the Magnetic Relations of the Positive and Negative Optic Axes of Crystals.* By Professor PLÜCKER of Bonn, in a letter to, and communicated by, Dr. Faraday.

ALLOW me, Sir, to communicate to you several new facts which, I hope, will spread some light over the action of the magnet upon the optic and magnecrystallic axes.

I. The first and general law I deduced from my last experiments is the following one:—"There will be *either repulsion or attraction* of the optic axes by the poles of a magnet, according to the crystalline structure of the crystal. If the crystal is a *negative* one, there will be *repulsion*; if it is a *positive* one, there will be *attraction*."

The crystals most fitted to give the evidence of this law are *diopside* (a positive crystal), *cyanite*, *topaz* (both negative), and other ones, crystallizing in a similar way. In these crystals the line (A) bisecting the acute angles made by the two optic axes, is neither perpendicular nor parallel to the axis (B) of the prism. Such a crystal, suspended horizontally like a