the terms *cirro-stratus* and *strato-cirrus*. Finally the terms *cumulo-stratus* and *cirro-stratus* are sorely needed for varieties of clouds intermediate between the class I have described and the *cumulus* and *cirrus* types, if any part of Howard's terminology is to be left to us at all.

It would be a pity that that terminology, lucid and expressive, should perish, merely because, to a few minds, the originator of a system needs appear infallible, and his classification perfect as Minerva when issuing from the head of Jupiter. I think that Luke Howard would have been the last to put forward such a claim.   W. CLEMENT LEY

June 8

The Motion of Fluids

PROF. REYNOLDS, in the course of his review (NATURE, vol. xxi. p. 342) of my book on the above subject, cites two instances in which I have been guilty of what he considers loose and vague reasoning. I would ask space for a few remarks on the points in question.

To take the more important matter first, Prof. Reynolds says, *apropos* of a certain proof of the velocity-potential theorem given in Art. 23 :—

"Mr. Lamb has offered a proof of this now historic theorem, which, if judged by the space it occupies, should be much simpler than the acknowledged proofs of Cauchy and Stokes. As no authority is cited, it would appear that this proof is here given for the first time. If so, the author has done himself great injustice in not examining or explaining his reasoning more closely. For, as it stands, it suggests the idea that he has ignored the fact that dx, dy, dz, on the left of his equation, are integrals through a finite time, and hence, inasmuch as he has given no reason to the contrary, may be of a different order of magnitude from their initial values, da, db, dc, which appear on the right of his equation. If this is not so it is a peculiarity of the motion of continuous fluid, and needs establishing ; otherwise we might infer that two people who had once shaken hands could never after be so much as a mile apart."

Prof. Reynolds, who himself strongly recommends the careful study of "work from the master's hand," will hardly take it amiss if I ask him to turn to the proofs which he justly cites as classical, and to notice that they contain, one of them (Cauchy's) in exactly the same form, the other in a form which is mathematically equivalent, the very assumption which he here calls in question. The assumption is in fact nothing more than a tacit limitation, which is made at the very outset of the subject, as to the class of motions which are proposed for study. In the "Eulerian" method it is implied that the first derivatives of the component velocities u, v, w with respect to the co-ordinates x, y, z are to be everywhere and always finite throughout the motion considered ; in the "Lagrangian" method the corresponding, and equivalent, assumption is that the derivatives $\dfrac{dx}{da}, \dfrac{dx}{db}, \dfrac{dx}{dc}$, &c , and also $\dfrac{d^2 x}{da\,dt}, \dfrac{d^2 x}{db\,dt}, \dfrac{d^2 x}{dc\,dt}$, &c., are to be finite. We do not assert that these are universal characteristics of fluid motion, for it is easy to imagine cases in which they are violated ; we merely exclude such cases *ab initio* from the scope of our investigations. But, in one form or another, these fundamental limitations are, from the point of view of analytical hydrodynamics, unavoidable ; they are made implicitly every time we write down the equations of motion, and it is therefore not surprising that they should be found to be essentially involved, not only in the proof which Prof. Reynolds on this account criticises, but in every other proof of the velocity-potential theorem which has yet been propounded.

I have only to add that the proof in question is, and professes to be, merely a very obvious corollary to H. Weber's transformation of the Lagrangian equations.

The other passage of Prof. Reynolds's review which I wish to notice is as follows :—

"There is a considerable amount of vagueness attending the author's use of the term *particle*. Having rightly defined fluids as being such 'that the properties of the smallest portions into which we can conceive them divided are the same as those of the substance in bulk,' he proceeds to reason about a particle as though it were a discrete quantity, the position of which is defined by some point, thus ignoring the fact that, according to his definition, the same particle of fluid may at one time be a sphere, at another a filament of indefinite length, or a sheet of

indefinite breadth. This vagueness appears to have led him into error in Art. 11."

A good deal of this criticism is, I think, met by the remarks already made. In a fluid moving subject to the conditions I have stated, only finite changes of shape can be produced in a moving element within a finite time.

Prof. Reynolds does not indicate the precise nature of the "error" which he finds in Art. 11. After a careful reconsideration, the argument of that article appears to me to be sound ; but I am free to confess that it is not stated with all the clearness desirable, and that the article is further disfigured by an unfortunate clerical error in the foot-note, where "$u = \pm \sqrt{x}$" should be read for "$u = \pm x$."   HORACE LAMB

Adelaide, March 30

On the Physical Aspects of the Vortex-Atom Theory

WILL any charitable person explain a difficulty which I (and other non-mathematical people) have encountered when seeking to understand and be satisfied with this theory ?

The only proof of those properties of vortex rings which match the physical properties of atoms that I have met with is that in Bésant's "Hydromechanics"; and is based on the initial-co-ordinate method.

Now it seems to me that this method assumes what is equivalent to the permanence of the vortex filament ; so that in proving the latter by use of this system of co-ordinates we may be merely arguing in a circle.

For it assumes that if initially we have any infinitesimal tetrahedron $\delta a . \delta \beta . \delta \gamma$, then after the *finite time*, t, this will still form a tetrahedron $\delta x . \delta y . \delta z$.

Now I cannot see that one can assume this ; that—to use the words in a late article of NATURE—"if two people have once shaken hands they can never be 100 miles apart."

And this inseparability of the particles of a fluid thus *assumed* bears a very close relation to the permanence of the vortex filament which we wish to prove.   W. L.

Cheltenham, May 29

[It appears to us that our correspondent here confuses between the permanence of any fluid filament and the permanence of the vortex character of the filament. The assumption that every filament remains continuous cannot be said to be equivalent to assuming that the direction of the filament at every point remains coincident with the axis of rotation of its constituent elements at that point, which is what Helmholtz has taught us.—ED.]

The Aurora Borealis and its Colours

WITH regard to Drs. De La Rue and Müller's paper on the Aurora (NATURE, vol. xxii. p. 33) there is still a point I should like to see explained. Is it considered by physicists that in electric discharges similarity of colour is sufficient to indicate similarity of constitution, even when their spectra are quite unlike ? The paper, together with the reply to Prof. Smyth, certainly seems to imply this ; though I have not previously seen it stated to be the case.

With regard to the red part of auroræ, so far as my observations indicate its position, they show it to be above the greenish part in the auroræ seen here ; though according to Weyprecht's observations, it is below the green in the Arctic regions.

Sunderland, June 9   T. W. BACKHOUSE

A New Audiphone

FURTHER experiments on the timbre of musical instruments as rendered by the audiphone have led me to the selection of the following as a distinct improvement on the birchwood veneer, both for musical purposes and also for ordinary conversation. It has the same advantage as my previous form in not requiring to be held by the hand, it costs nothing, and requires no making. Take a sheet of stiff brown paper about 11×15 inches, the paper being such as is ordinarily used for making up heavy parcels. Put the ends together, the middle forming a loop, and hold the ends between the teeth. The paper must be pretty stiff, as the loop must stand out round and full, and of course the paper must be without folds or creases.   THOS. FLETCHER

Museum Street, Warrington

Crystal-Ice

IN reference to the "crystal ice" proposed by Dr. Calantarients, of Scarborough, for skating upon with ordinary skates,