surface outflow, he could trace an underflow of sea-water up the channel; and this he could attribute to nothing else than the slight excess of *downward* and therefore *lateral* pressure in the *outside* column, depending on the continually-maintained reduction in the mean salinity of the *inside* column, which more than compensated for any slight excess in its level.

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The Freshwater Medusa

IN NATURE (vol. xxii. p. 190) Prof. Lankester refers to a statement of mine in the preceding number, that I had arranged with Mr. Sowerby some methods of observation from which I hoped to obtain data for the determination of important points regarding the development of the freshwater Medusa, and expresses a desire to be informed as to the nature of the proposed methods.

The obvious and only practicable course to be adopted with this view was arranged with Mr. Sowerby by Mr. Busk and myself, and consisted in the separation of specimens from the Victoria tank and their confinement in glass jars, which, in order to secure a continuance of the necessary temperature conditions, were to be retained in the same house with the tank in which the Medusa had shown itself. The examination from time to time of these jars would probably bring to light facts having a direct bearing on the development of the animal. This method of observation, indeed, is so obvious that it must have occurred to any one engaged in the investigation it was designed to aid.

Prof. Lankester now says that Mr. Sowerby informs him that he had undertaken no experiments except such as had been carried out at his request; but as it seems that these are identical with those proposed by Mr. Busk and myself, nothing has been thereby lost.

Residing at a distance from London, my opportunities of studying the life-history of the Medusa are at this moment comparatively few. Prof. Lankester, however, being on the spot, and having an unlimited supply of subjects for investigation, will doubtless avail himself of the advantage thus afforded, and will render our knowledge of this remarkable little animal more complete than would otherwise have been possible.

Prof. Lankester refers to the difference of opinion between himself and me, and promises to bring proofs of his own views. When these proofs are offered I shall gladly accept them. My desire is that no previous expression of opinion shall blind me to evidence in favour of a contrary position. The only important points, however, on which my conclusions have been absolutely at variance with those of Prof. Lankester are the presence of a circular canal and the perviousness of the distal extremities of the radial canals. With regard to these there cannot in my opinion be the slightest doubt.

The nature of the marginal bodies is also a point of much importance in this investigation, but I have expressed only a conditional opinion with regard to it. While Prof. Lankester considered these bodies as undoubtedly tentacular, I held that the evidence afforded by adult and by comparatively young specimens is in favour of their velar origin; but at the same time I stated that this point cannot be decided without the evidence obtained from development.

I also drew attention to the remarkable attachment of the tentacles, whose adnate basal portion occupies exactly the position of the *peronia* in the Narcomedusæ and Trachomedusæ, but I failed to find evidence of the presence of true peronia as described by Prof. Lankester, who now admits that the peronia while present are rudimental.

The other points, namely those which concern the systematic position of the Medusa, are necessarily only hypothetical. It appeared to me that while there are certain features in the structure of the adult Medusa which point towards the Trachomedusæ, there are others which connect it with the Leptomedusæ, to which on the whole it seemed to be more closely allied, though holding a position intermediate between the two; but I regarded the data in our possession as insufficient for the final determination of this point, which can be absolutely settled by the study of development alone.

Prof. Lankester promises details of his observations in this month's number of the *Quarterly Journal of Microscopic Science*, and I look forward to what I doubt not will be a valuable contribution to hydroid zoology.

As to the name of the Medusa, Prof. Lankester, while abandoning his generic name in favour of mine, declares it to be his intention to retain his own specific name for the animal. This is to me a matter of complete indifference. Science can gain nothing from personal contention about names, and the time so occupied might with far greater advantage be devoted to more useful and lasting work.

J. ALLMAN

On the Simplest Continuous Manifold of Two Dimensions and of Finite Extent

So far as I am concerned Mr. Frankland answers too soon (p. 170), for I am sorry to say I have not read Klein in the meantime. Therefore my reply is provisional. A hint was given of Mr. Frankland's explanation by Mr. Newcomb in a phrase quoted by Mr. Halsted (*American Journ. of Math.*, I. iii. 275, paper on the bibliography of hyperspace, &c.): "The first elements of complex functions imply that a line can change direction without passing through infinity or zero." We do not require even the first elements of complex functions to tell us that we can get to the other side of a point without passing through it, provided we can go round it. But the question was not whether "a line" simply could be thus reversed, but whether it could be so with the geodetic perpendicular in question described in a uniform continuous manifold of two dimensions. Mr. Frankland's explanation expressly takes account of a third dimension. It supposes the moving line to generate a sort of skew helicoid about the fixed line to which it is perpendicular. But how can even initial portions of successive generators be in the same plane, Euclidean or other? This point may seem incidental, but I think it is essential, so I omit further questions.

Somewhere in his "Dynamic" Clifford says that Klein's double surface is a sphere in which opposite points are considered as one. In this light the mystery disappears. There are two perpendiculars: considered as one they never change sign; because, considered as two, they periodically exchange signs. But if opposite points do not coincide, they may be "one," but they are not one point; if they do, is the manifold they compose a surface? Mr. Frankland has not called it a surface: but is it continuous?

There is a very well-known manifold which obviously obeys the laws worked out by Mr. Frankland and Mr. Newcomb, a system of straight lines, not vectors, through a common point; or, reciprocally, a system of planes. To measure of curvature answers density; if this is constant, the geodetic distance from a point to a geodetic line is represented by the angle between a straight line and a plane.

It may be worth while to note one or two oversights in the writing or printing of Mr. Frankland's letter. For $\frac{1}{2}l\sqrt{-1}$ we ought to have an expression involving the angle between the geodetics. The sentence "If a being," &c., is a quotation, and the last word should be "position," not "poise."

Both Mr. Newcomb and Mr. Frankland understand my intention as more negative than it was. I said (xv. 547) "it could hardly fail to be instructive if Mr. Frankland would explain," &c. Probably I underrated the difficulty, in this Euclidean world, of making it clear that one means just what one says.

C. J. MONRO

Hadley, June 29

A Fourth State of Matter

IT seems to me that Mr. Tolver Preston in his letter on the above to NATURE (vol. xxii. p. 192) has somewhat overlooked the context in the objections he urges against Mr. Crookes's remark that "an isolated molecule is an inconceivable entity." It is plain that Mr. Crookes meant this statement to apply to the *quality*, not the *existence* of a molecule, and granting Mr. Crookes's premisses regarding the constitution of matter, it appears a very fair deduction; since if the three states of matter (as we know it), viz., solid, liquid, and gas, owe their different *qualities* merely to different modes of motion of the ultimate molecules, it is quite conceivable as well as logical to suppose that the latter have a nature totally unlike that of the effects of their motion, and therefore inconceivable to us by reason of its dissimilarity to anything of which we at present possess any knowledge.

Again, with reference to the remark, "solid it cannot be,"