

LETTERS TO THE EDITOR.

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Asymmetry and Vitalism.

THAT portion of Prof. Karl Pearson's letter in NATURE of Nov. 10 which deals with chemical problems is largely based on misconceptions of the meaning of terms used by chemists. Thus, after quoting a statement of mine regarding optically active compounds, he says: "An optically active compound means merely a preponderance of one kind of enantiomorph." That is precisely what it does *not* mean; that would be an optically active *mixture*. No chemist ever uses the word "compound" when he means "mixture"; I meant one kind of enantiomorph and one only. Moreover, I explained this point in detail in my first reply. Prof. Percy Frankland, as a chemist, has of course found no difficulty in following my meaning; he says that the question which I raised was: "the possibility of producing, without the interference of a living agency, an optically active substance *unaccompanied by its enantiomorph*." A great part of Prof. Pearson's letter is therefore devoted to combating an opinion which I never expressed, and I am consequently relieved from the necessity of further discussing this part, or of calling attention to similar misconceptions which it contains. One point, however, I must notice. Prof. Pearson complains that I have supposed that he meant "twenty" molecules and no more, when in reality he referred to twenty tosses of a coin; and he adds that he was willing to assume the formation of a million molecules. I was led to take his words in the former sense by my impression—as it now appears, a mistaken impression—that he really understood that I was arguing about *single* asymmetric compounds; and I imagined that he purposely assumed the formation of only a small number of molecules in order that they might conceivably be all of one kind of asymmetry.

The part of the letter which I wish especially to consider is that in which Prof. Pearson suggests a hypothetical symmetric mechanism by which he believes a separation of enantiomorphs might be effected. This suggestion, if valid, strikes at the root of a generally accepted principle of molecular asymmetry. I can sincerely say that it is with the utmost diffidence that I venture to call in question any result that Prof. Pearson has arrived at by a mathematical process. But in the present case, I have tried in vain to follow his reasoning; whilst, if I work out the problem in my own way, I arrive at a conclusion exactly the opposite of his. I have no choice, therefore, but to state my results, and to ask Prof. Pearson to correct me if I am wrong.

The tetrahedral representation of two enantiomorphous molecules, each containing a single asymmetric carbon atom, is given in my address (NATURE, vol. lviii. p. 455). The tetrahedra (Figs. 1 and 2) are assumed to be irregular; the four different atoms or groups are situated at different distances from the central carbon atom to which they are attached; the two tetrahedra are enantiomorphous. It must be carefully borne in mind that in these two structures *all corresponding molecular dimensions are identical*; the two structures differ only in their opposite asymmetry.

Prof. Pearson imagines a thin cylindrical sheet of optically inactive mixture to be whirled round the axis of the sheet; and he argues that owing to the position of the centroid in these enantiomorphous tetrahedra, the one kind might be in *stable* equilibrium when, say, their x' angle sets inwards, and the other, when this sets outwards; "or at least some similar like difference of positions will differentiate like from unlike enantiomorphs." Then, on allowing a strip of the cylindrical surface placed horizontally to fall through a viscous fluid, the difference of resistance caused by this difference of position may effect a separation of the two kinds.

As I have said, I am unable to follow this reasoning. I should discuss the problem as follows:

Let H, x', y', z' (Figs. 1 and 2, *loc. cit.*) represent the four different atoms or groups attached to the central carbon atom; and, as regards their masses, let $H < x', x' < y',$ and $y' < z'$.

Then, supposing a thin cylindrical sheet of substance consisting of equal numbers of the two enantiomorphous tetrahedral

forms to be whirled about its axis, each tetrahedron, whether right-handed or left-handed, will be in stable equilibrium when the distances of the foregoing groups from the axis of rotation of the sheet are in the order H, x', y', z' . Therefore the edge $y' z'$ of either tetrahedron will be nearer to the outer surface of the sheet than the edge $H x'$; and each of these edges will be inclined towards this outer surface so that the ends z' and x' , of these edges, are respectively nearer to it than the ends y' and H ; and the inclination of corresponding edges will be the same in both tetrahedra. A line joining the centroid of the face $H x' y'$ with z' , and produced through z' to meet the outersurface of the sheet, will form the same angle with this surface, whether the tetrahedron be right-handed or left-handed. Right-handed and left-handed molecules will therefore be affected in exactly the same manner when a strip of the cylindrical surface is placed horizontally and allowed to fall through a viscous fluid; and no separation of the two kinds will occur. The "difference of positions" which Prof. Pearson demands, does not extend beyond the fact that a continuous curve passing towards the surface of the sheet through the groups H, x', y', z' in succession, will in the one set of tetrahedra describe a right-handed, in the other a left-handed helix.

I am unable to arrive at any other conclusion than the foregoing.

Prof. Percy Frankland's suggestion of a mechanism by which, starting with a single asymmetric molecule, an optically active compound might be produced unaccompanied by its enantiomorph, practically coincides with that published a little later by Mr. Strong. Such an action is, as I admitted in noticing Mr. Strong's communication, certainly conceivable, although, as an actual process occurring under chance conditions, it is exceedingly improbable. I regret that I overlooked the possibility of such an action.

Prof. Frankland's other suggestion is that, prior to the existence of life on the earth, "the asymmetry of solar radiation may originally have determined the exclusive synthesis of one enantiomorph." I had already considered this possibility. It seems to me that the earth's rotation, to which this asymmetry of solar radiation is due, is so slow as compared with the atomic and molecular motions involved in the production of chemical compounds, that it is difficult to understand how it could perceptibly impress its asymmetry on chemical action.

Although, in view of the arguments adduced by Prof. Percy Frankland and Mr. Strong, I no longer venture to speak of the *inconceivability* of any mechanical explanation of the production of *single optically active compounds asymmetric always in the same sense*, I am as convinced as ever of the *enormous improbability* of any such production under chance conditions. The processes suggested by Prof. Frankland and Mr. Strong are purely hypothetical and are likely to remain so.

The University, Aberdeen, November 17. F. R. JAPP.

Early History of the Great Red Spot on Jupiter.

HAVING collected a number of observations and drawings of objects bearing a suggestive resemblance to this feature, and made during the period from September 5, 1831, to November 14, 1869, I have been enabled to determine the rotation period during that time. This, taken in combination with my discussion of the observations from November 14, 1869, to July 30, 1898 (NATURE, August 4, 1898, and *Monthly Notices R.A.S.*, vol. lviii. No. 9), extends the whole interval over which the spot can be pretty certainly identified to nearly 67 years, or 24,435 days, during which the mean rate of rotation was

$$9\text{h. } 55\text{m. } 36\cdot 2\text{s.}$$

and the total number of rotations

$$59,071.$$

My investigation, though quite satisfactory so far as it goes, would be rendered more certain if further observations or drawings could be secured for the period prior to 1869. I should be much obliged, therefore, if any of your readers having such materials in their possession would supply copies, or allow me to have temporary use of the originals. The red spot has varied its appearance so much that it may either appear as a red oval mark, as an elliptic ring, or be practically invisible as at present, though its place may be clearly indicated by a marked hollow in the southern side of the south equatorial belt. Old