

(NATURE, vol. lviii. p. 592, col. 2) so as to include these special cases.

Similar considerations would apply to the action of polarised light on a mixture of enantiomorphs. The most that could occur would be the production of an equal rotation of enantiomorphous molecules in opposite senses, corresponding with their opposite asymmetry. That would not cause separation.

Again, take Mr. Spencer's first "abstract proposition" (*loc. cit.*), which runs: "Like units subject to a uniform force capable of producing motion in them, will be moved to like degrees in the same direction." How does he reconcile this statement with the fact that enantiomorphs have the same heat of formation: *i.e.* that the same atoms are moved by the same amounts of energy, not in the same direction, but in directions of opposite asymmetry, so as to form two asymmetrically distinct compounds?

I probably do Mr. Spencer no injustice if I assume that in 1862, when he formulated these "abstract propositions," he was not acquainted with the theory of molecular asymmetry, which at that time was not generally current, even among professed chemists. And if I might do so without offence, I would suggest that he should read the portion of Van't Hoff's "Arrangement of Atoms in Space" (second edition) dealing with the question of molecular asymmetry, especially the section which describes the character of the isomerism due to the asymmetric carbon atom. He may then be able to recast his "abstract propositions" so as to include, at least more explicitly, the formation and behaviour of enantiomorphous molecules under symmetric influences.

F. R. JAPP.

The University, Aberdeen, November 2.

I AM not sure that much is to be gained by continuing this discussion further, but perhaps I may be permitted to add something to my first criticism of Prof. Japp's standpoint in view of his communication in last week's NATURE.

The statement of Prof. Japp's, which I specially criticised, was the following (where I italicise the words to which I wish to draw particular attention):—But the *chance* synthesis of the simplest optically active compound from inorganic materials is *absolutely inconceivable*.

To this I replied and still reply, it is *not* absolutely inconceivable. An optically active compound means merely a preponderance of one kind of enantiomorph, and *chance* will always produce this, given enough trials and length of time to make them. Prof. Japp twits me with the ineffectiveness of twenty molecules, but I spoke not of twenty molecules, but of twenty coins, in order to bring home to Prof. Japp what a deviation from the average in the theory of chance really means. The probability of a deviation of 5000 in 1,000,000 molecules is easily calculated, and such a deviation is quite "conceivable," even if it be very infrequent. A deviation of 5000 in 1,000,000 molecules would give an optically active solution, whether sufficiently intense to be observed by the means at our disposal is another question. The statement that on the theory of *chance*, an optically active compound is *absolutely inconceivable* is, I take it, absurd. It may be very *improbable*, but this is not the term used by Prof. Japp.

Prof. Japp writes in his letter: "Prof. Pearson's twenty non-living asymmetrical molecules formed by the chance play of mechanical forces, would, so far as experiment informs us—although I freely admit that mere negative results are not conclusive—have no more influence on the asymmetry of other molecules formed in their neighbourhood than one toss of a coin has upon another toss." I reply that I think experiment shows they *have*. It is possible in Jungfleisch's process to get crystals which are purely right- or left-handed up to the size, say, of half an inch, sufficiently large for picking out. Now I take it that it is chance which produces a slight majority of one type of enantiomorphs at one or other point, and what I have termed "breeding," which encourages the collection of that type at the given centre until we get crystals purely right- or left-handed up to a size of half an inch. That a number of molecules of one kind, such as are required for these crystals, should be frequently formed, is totally opposed to the theory of *chance*, but I take it that a slight chance preponderance sets the "breeding" going.

Take a dish of such crystals and throw them out at random, and they scatter in all directions; one such crystal coming into a few drops of fluid forms an optically active medium consisting of enantiomorphs of one kind only. Thus even a total dis-

appearance of one kind of enantiomorphs is not impossible, or "absolutely inconceivable" on the theory of *chance*. Prof. Japp speaks of the "vague and elastic" way in which I speak of the "breeding" process—I notice that Prof. Errera also uses the phrase "asymmetry begets asymmetry as life begets life." Let us confine the term then, for the present, simply to the process (of which so far the mechanism is unintelligible) by which chance having given a slight local preponderance of one type of enantiomorph, a group of the same type, visible and touchable, is formed there. It is perfectly conceivable that this is only a visible representation of the process by which living asymmetry selects its like, even in a non-crystalline compound. It is only the mechanism which is vague, not the fact.

Prof. Japp really complains in his address that an "eminent physicist" should say that an explanation of rotatory polarisation is still wanting. It is still wanting, because no *kinetic* theory, which is what a physicist requires, can be provided by what is after all only a geometrical *schema* of the chemist. Prof. Japp now writes that every chemist recognises that it is only a geometrical hypothesis, and he did not think so obvious a qualification needed statement to an audience of chemists. Then why, I ask, should Prof. Japp go out of his way to say that the theory was unknown outside the circle of organic chemists, and cite the "eminent physicist" as an example of such ignorance?

The fact is, that the moment we look at Prof. Japp's tetrahedron atoms, Figs. 1 and 2 of his paper, as *dynamical* systems, the right-handed and left-handed molecules do not respond in the same manner to symmetrical forces. The atoms not being identical, the centroid will not necessarily be the centroid of the tetrahedron; say, it is somewhat nearer to Z' than H. Now whirl a thin cylindrical sheet of optically inactive mixture round the axis of the sheet, left- or right-handed rotation is indifferent; the left-handed tetrahedra will not be in *stable* equilibrium relatively to the centroid of the molecule in the same position as the right-handed. Consequently the former will all set, say, their X' angle inwards, and the latter outwards; or at least some similar like difference of positions will differentiate like from unlike enantiomorphs. Now let a strip of the cylindrical surface be placed horizontally and allowed to fall, say, through a viscous fluid, the resistance to a tetrahedron going X' foremost, may well be greater or less than one going HZV' foremost, and if so the left-handed molecules will be separated ultimately from the right. All this is purely hypothetical, but I introduce it because Prof. Japp asserts that it is "impossible" for any mechanical (symmetrical) forces to constantly select one of two opposite forms. I reply that the impossible is conceivable, if he will treat his molecules not as geometrical *schemas*, but as dynamical systems.

One last word. Prof. Japp refers in his address to a "vital force" which does not disobey the law of energy, but is purely *directive* of motion. I have seen such an idea several times mooted. The question is not, however, if something called vital force obeys the law of the conservation of energy, for the principle of energy *never* fully defined any motion, something else is also directly or tacitly assumed. In itself it only leads to *one* equation, not sufficient to describe any motion. The problem is whether "vital force" obeys *all* the laws of motion—for example the conservation of momentum, angular and linear, which it could hardly do if it changed the direction of motion. I am quite unable to realise why some chemists and physicists seem to think a disregard for the conservation of momentum less miraculous than a disregard for the conservation of energy. I do not see why the less important principle should be made more of a fetish than the wider reaching principle. If "vital force" does obey all the laws of motion, then it can only be a rather bad name for some piece of mechanism, to which the most ardent supporter of a mechanical theory of the universe (such as Büchner or Moleschott, not I) could not possibly object.

KARL PEARSON.

University College.

WHILST Prof. Japp is to some extent justified in saying that all his critics "seem to be moving in that unreal world where a fount of type, if jumbled together sufficiently often, ends by setting up the text of *Hamlet*," still it must be borne in mind that he himself provoked a discussion in such an imaginary region by raising the question as to the possibility of producing, without the interference of a living agency, an optically active