

where *abcd* are any monovalent radical not containing an asymmetric carbon atom, while R is a dextro-rotatory bivalent radical. The first molecule formed, according to equation (1), will have the underlined C dextro or laevo, being determined by R being dextro.

When this decomposes according to equation (2) the C will still be active, and one active molecule of  $\begin{array}{c} \text{Ca}_2\text{H} \\ | \\ \text{CabH} \end{array}$  will have been produced.

But  $\text{RH}_2$  is reproduced, and is consequently capable of repeating this action indefinitely, and of making an indefinite number of  $\begin{array}{c} \text{Ca}_2\text{H} \\ | \\ \text{CabH} \end{array}$  molecules with activity of the same sign.

Furthermore, this action is scarcely to be distinguished in principle from the reactions brought about by unorganised ferments or enzymes.

W. M. STRONG.

Helstonleigh, Champion Park, Denmark Hill, S.E.,  
November 1.

It appears to me that Prof. Kipping and Mr. Pope unintentionally attribute to me opinions which I have never expressed, and which I do not hold. I never for a moment imagined that in each separate crystallisation—either of molecularly symmetric substances which, like sodium chlorate, may form either right-handed or left-handed crystals, or of the externally compensated mixture of dextro- and laevo-rotatory sodium ammonium tartrates, in which the asymmetry is molecular—equal amounts of the two kinds of crystals would necessarily be deposited. I never thought of this equality as holding good, except as the mean of a great number of experiments. In my address, when referring to Messrs. Kipping and Pope's results obtained with sodium chlorate, I therefore used the expression "on the average." Besides, I was acquainted with Landolt's experiments on the subject, which prove the same thing. In the case of the dextro- and laevo-rotatory sodium ammonium tartrates, the Pasteur-Gernez method of separating these by starting the crystallisation with a crystal of one of the two kinds, and Jungfleisch's experiments, to which I will refer more fully later on, were sufficient to make me aware of the influence of initial bias on crystallisation, and to prevent me from expecting equality, except as a mean result.

In fact, in these crystallisations everything depends on this initial bias; and in this respect, Landolt's experiments are especially instructive. His method (*Ber. d. deutsch. chem. Ges.*, 1896, p. 2410) consists in precipitating aqueous solutions of sodium chlorate by addition of alcohol. A magma of minute crystals is thus obtained, each of which must be either right-handed or left-handed. The crystals are then filtered off and ground to a fine powder; and this powder, suspended in a mixture of alcohol and carbon disulphide, is examined in the polarimeter. If both enantiomorphous forms are present in equal quantity, there will be no rotatory effect; whereas a preponderance of either form will be indicated by a rotation in the corresponding sense.

Landolt found that if the precipitation was rapidly effected by the sudden addition of alcohol, the powder was either inactive, or had a very feeble rotation. On the other hand, if the alcohol was gradually added, so as to produce slow precipitation, the powder displayed a marked rotation, which was in some cases right-handed, in others left-handed. The reason is obvious. In the former case, the crystallisation starts simultaneously from a vast number of independent centres; the chances are equal in favour of each centre being right-handed or left-handed; each centre will propagate its own kind; and thus the ratio of right-handed to left-handed forms will not differ very appreciably from unity. In the latter case, as Landolt points out, the crystallisation starts from only a few centres; in these, therefore, either the right-handed or the left-handed form may predominate, and, as sufficient time is given, the dominant form will influence the course of the crystallisation.

In Messrs. Kipping and Pope's slow crystallisations, every opportunity is afforded for any initial asymmetric bias to exercise its influence; and I should have been greatly surprised if the authors had obtained a result other than that which they describe.

There is, therefore, a marked difference between the formation of enantiomorphous crystals and of enantiomorphous molecules respectively, *under symmetric conditions*: namely, that the crystals propagate their own asymmetry, whilst the molecules, as I pointed out in my reply to Prof. Karl Pearson, do not—at least, so far as experiment informs us.

In the crystallisation of the externally compensated mixture of dextro- and laevo-rotatory sodium ammonium tartrates, one would expect a similar state of things to prevail, modified however by the circumstance that in this case the two opposite asymmetries of the molecules themselves are pre-existent in the solution and must influence the result. One would expect variations, within limits, of the relative quantities of dextro- and laevo-salts deposited; but according to our present views, *the mean variation should occur equally in opposite directions*.

It is therefore with great surprise that I learn that in the ten experiments which Messrs. Kipping and Pope have hitherto made on this point they obtained in every case a strongly dextro-rotatory deposit. It is true that Jungfleisch (compare *Chem. News*, vol. 40, p. 231) published, in 1884, similar results. Jungfleisch's procedure was somewhat different from that of Messrs. Kipping and Pope. Instead of allowing the solution to crystallise spontaneously, he introduced simultaneously into the supersaturated solution, at opposite sides of the crystallising dish, a crystal of the dextro-rotatory and one of the laevo-rotatory salt. In all his experiments the former crystal showed the greater increase in weight, and the mother liquor contained a corresponding excess of the laevo-salt. I confess that I have always attributed Jungfleisch's results to mere coincidence. It will doubtless be within Prof. Kipping's recollection that in April last I wrote to him calling his attention to these results, in connection with his joint work with Mr. Pope. Prof. Kipping replied (I quote from memory), that Jungfleisch's results were opposed to all that was known of the behaviour of enantiomorphs; that he (Prof. Kipping) did not regard them as conclusive; and that he proposed to repeat them. This he and Mr. Pope have now done; but from what I have just said, it is evident that the outcome of this repetition must have been as great a surprise to him as it is to me. I am glad to find, however, that the authors put forward their results with reserve, and that they contemplate the possibility of discovering some disturbing influence. If no such influence is at work, it is not merely the small matter of my particular application of the principles of molecular asymmetry that will have to go: it is these principles themselves. But I do not anticipate any such catastrophe. The vast accumulation of verified prediction of which the science of stereochemistry can boast, does not point to premises so unsound.

I will wait, therefore, for further light on Messrs. Kipping and Pope's later experiments before drawing any conclusion from them. As regards the experiments with sodium chlorate, I am at a loss to understand in what way they militate against my views. I no more expect equal numbers of right and left crystals to separate in any given crystallisation of sodium chlorate than I expect an unvarying alternation of heads and tails in tossing a coin. Nor do I perceive the application of the experiment in which they obtained none but right-handed crystals of the chlorate; inasmuch as, if they go on repeating it, they will obtain, just as often, none but left-handed crystals. The process will not *constantly* yield the same form.

I must also repeat that I do not regard the spontaneous formation of "optically active mother liquors," or other partial separations of enantiomorphs, especially when the separation may occur in either direction, as a solution of the problem. The separation of enantiomorphs in the living world is, in the overwhelming majority of cases, as I have pointed out in a previous reply, not partial but *total*; and it occurs *constantly* in one direction—only one form survives. This is another case where the word "constantly" applies—a word quoted by the authors, but otherwise ignored by them, except where they are discussing experiments which they themselves regard as requiring confirmation.

I think, moreover, that my critics might in justice take into account the disadvantage under which I laboured in having to compress into the limits of a brief address an account of a subject so vast and intricate. Many of my statements on important points were necessarily somewhat summary and inadequate; and most of the misunderstandings into which my various critics have fallen are, I am aware, due to this circumstance.

The University, Aberdeen, November 10. F. R. JAPP.

MR. STRONG'S suggestion is very ingenious, and I must admit that, *granting his premises*, the chance production of an unlimited quantity of a single asymmetric compound is conceivable. I had not thought of the possibility of one asymmetric molecule acting as a catalytic agent in the way he suggests. It would, however, have been perhaps simpler and more in accordance with the behaviour of enzymes, with which class of ferments he compares this supposed catalytic agent, if he had represented the second stage of the process as a hydrolysis; in which case, of course, the asymmetric group of the resulting compound would have contained hydroxyl in place of hydrogen.

Mr. Strong admits that the process is "purely hypothetical." I think I should go further than this, and say that, considered as an actual process occurring under chance conditions, it is grotesquely improbable.

The "volcanic explosion" carrying "one molecule" of an asymmetric compound into "a certain pool of water," seems to be a reproduction (on a reduced scale) of Prof. Errera's "vortex" which whirls "one simply asymmetric particle" into a particular "planet" (see NATURE, vol. lviii. p. 616, col. 2).

F. R. JAPP.

The University, Aberdeen, November 5.

### Mental Calculations of a High Order.

THERE are probably among your readers some who are interested, by curiosity or for scientific purposes, in freaks of memory. I am not sure that what my memory has done is remarkable, although it is quite novel to me.

For many years I have been in the habit of using some useless exercise in mental gymnastics to divert my mind from the occupations of the day, and so get quickly asleep. Sometimes it would be extracting the letters of the alphabet successively from some passage in prose or poetry, keeping the number of each letter in mind, and finally counting all the letters in the passage, to make sure that I had allowed no letter to pass by me unnoticed. Again, I would try to think of all the famous poets, or generals, or sovereigns, or statesmen of all time, whom I could recall, in alphabetical order. Whatever might be the task I undertook I resumed it night after night, beginning as nearly as possible where I left off, and continue until I had completed it.

About a year ago the fancy took me to see how far I could go in raising, by mental process only, the number 3 to its high powers. At the beginning I would not have believed it possible to remember fifteen figures in their order. To my surprise I succeeded in raising 3 to the 44th power, making, if I remember rightly, a number of 22 figures. I did it by successive multiplications by 3, and without shortening the process in any way. I did not put a figure on paper until I had reached the 24th power; but always proved every result as far as I could by the nine test—a safeguard against substantially every error save those that might arise from transposition of figures. At the point mentioned I set the product down, and performed on paper this short process:

$$3^2 = 9; 3^4 = 81; 3^8 = 6561; 3^{24} = 6561 \times 6561 \times 6561.$$

As my mental result was wrong in four or five figures in the middle, so to speak, I knew that I must have transposed two figures somewhere between power 15 and power 20, so I went back and began over again. At the 24th power I was right, and so I was when I reached the 44th power.

That seemed to be as far as it was worth while to go, and I then began a more difficult exercise: to ascertain, as in permutations, the product of the numbers from one upward as far as I could go. I have carried the process up to, and including the number 37. The product is a number of 44 figures, whereof the last eight are o's, which do not add to the effort of memory to retain them. To remember 36 digits in their correct order may not be a wonderful feat; it is so easy to me that I do not suppose it is unusual. But I can now remember, and have to-day written down and then repeated to my stenographer, successively, the product of the numbers to 35, to 36, and to 37, having respectively 33, 34 and 36 figures, beside the eight o's with which each number ends, or 103 figures in all. How much further I could carry the process I do not know; I do not purpose attempting to ascertain.

I will add some facts that may be interesting.

(1) I verify my result after each multiplication: first, by proving that the sum of all the digits is a multiple of 9; secondly, by dividing it by 7, 11 and 13, not attempting to

remember the quotient, but only the successive remainders, to be sure that the number divides evenly.

(2) Almost every product has some peculiar combination of numbers. For example, in the 35 result there are four 6's together; in the 36 the figures 6789 occur; the first six figures of the 37 product are 137,637; and so on.

(3) The work is done in groups of three figures, and almost every new factor in the multiplication gives some short process of multiplying. Before I began with 37 it seemed impossible to multiply 34 figures by such a number, odd, large, and a prime number. But the fact that  $37 \times 3 = 111$  soon suggested the way to make the process easy. The last nine figures (omitting o's) of the 36 product are 481,508,352. Now  $352 = 360 (3 \times 12 \times 10) - 8$ . Then we have  $111 \times 12 = 1332 \times 10 = 13,320$ .  $37 \times 8 = 296$ .  $13,320 - 296 = 13,024$ . The multiplication of the next group is easy.  $37 \times 500 = 18,500$ ; add  $296 (37 \times 8) + 13$  ("carried") = 18,809. The next group, 481, is taken as  $= 500 - 20 + 1$ , and the number with the 18 "carried" from the last multiplication becomes 17,815,809,024.

(4) Does this exercise put me to sleep? O, yes, very quickly! Boston, U.S.A., October 24. E. S.

### The Leonids in 1868.

I WITNESSED the magnificent shower of Leonid meteors on the night of November 13-14, 1866. But I do not recollect seeing any published account of such a display in England in 1868. It occurs to me that the following observations may be worth publishing.

On November 5 in that year I was in Venice. Returning through Milan, I crossed the Alps in a sledge by the St. Gothard in a terrible snow-storm on the 7th, and reached Calais at midnight on the 13th. Neither sun, moon, nor stars had been visible since I left Venice. The Calais boat started for Dover about 1h. 30m. on the morning of the 14th. As we were leaving the port the clouds suddenly cleared off, and a splendid display of Leonids was visible. I judged the shower to be in every respect equal to that of 1866. Some of them were as bright as Jupiter, and left long trails in the sky which took two or three minutes to dissolve. The display kept up until we were within a couple of miles of Dover, when the clouds suddenly came on again and the sky was completely obscured.

Coventry, November 12.

WM. ANDREWS.

### The Smell of Earth.

"SEE, the smell of my son is as the smell of a field which the Lord hath blessed." Thus poetically spoke the Patriarch Isaac. The man of modern science tells us, prosaically, that the odour of moist earth is due to a bacterium, named *Cladothrix odorifera*. I write to ask if any one has yet accounted for the well-known and peculiar odour, yielded by clay and clayey rocks when breathed upon. This odour can scarcely be due to bacteria, for it is manifested by cabinet specimens more than twenty years old. Pure alumina appears to be odourless.

Leeds, November 12.

C. T. WHITMELL.

### Breath-Figure of Spider's Web.

A FEW mornings ago I noticed in my bath-room a spider's web spun right across one pane of the window, but not in actual contact with the glass, there being room for a house-fly to buzz up and down the pane without touching the meshes. My morning ablutions giving rise to some considerable quantity of vapour, I observed a very distinct breath-figure of the spider's web upon the glass. I accordingly removed the web and the spider. Next morning, in the absence of the web, on the renewal of the vapour conditions the breath-figure reappeared. I then wiped one half of the window dry with a towel. Now, after five mornings, the breath-figure is quite distinct upon the half which was not touched, and can be faintly seen on the wiped portion.

OSWALD H. LATTER.

Charterhouse, Godalming, November 13.

### A Second Crop of Apples.

I THINK perhaps it may be worth noting that apple-blossom was gathered in the neighbourhood of Exeter last week. Still more remarkable is the fact that a second crop of apples has made fair progress, as some at the farm of Gras Lawn, close to the city, some "Red Quaranders" have been gathered, nearly the size of walnuts. Two of these, now somewhat shrivelled, are enclosed.

JAMES DALLAS.

Exeter, October 14.