asymmetry in opposite senses. But nothing of the kind is observed. Cellulose, starch, saccharose, have the same right-handed asymmetry, each in its particular degree, whether the plant that produces them grows north or south of the equator.

Mr. Bartrum suggests that two enantiomorphs may crystallise from their equimolecular mixture with an unequal distribution of the right and left crystals; that then partial re-solution may occur, "roughly on the lines of the distribution of the two varieties of crystals," giving an optically active solution. Unlike Prof. Fitzgerald, Mr. Bartrum does not propose to vivify this crystalline substance offhand; he merely suggests that it may have been "the first ancestor of *levo*-rotatory protein."

This is vague. As far as I follow its meaning I should read it: "Leave a soluble, crystallisable, asymmetric organic compound, of suitable character and composition, which has been formed and separated by the chance play of mechanical forces, long enough exposed to the action of other matter under the influence of these forces, and it will, in due course, first turn into protein and then come to life." I do not think that this statement misrepresents Mr. Bartrum's position, and I will leave it to speak for itself.

Mr. Bartrum's process of separation is also open to the objection that it would at best yield only an optically active mixture—i.e. with a mere preponderance of one enantiomorph; and as I have already pointed out, that is not a solution of the problem.

Mr. Herbert Spencer considers that I have ignored a universal law of "segregation" which he formulated in 1862 in his "First Principles," and which he there referred to three "abstract propositions" now quoted by him. He asserts that this law of segregation would account for the separation of dextro-protein and *levo*-protein, if these were once formed; and he instances the formation of hæmatite nodules and flints in chalk-formations as an illustration of the power of segregation in nature.

I think that Mr. Spencer does not quite realise to what extent enantiomorphous molecules are alike. Every symmetric form of energy (such as heat), and every symmetric material agent, is identical in its action upon two enantiomorphs: whatever happens to the one happens to the other. And in none of these facts is there the slightest violation of the law of the conservation of energy—although Mr. Spencer's corollary to his third proposition would suggest the contrary. As regards the separation of enantiomorphs, I do not know whether Mr. Spencer would interpret his third proposition to mean that they must be separable by diffusion; but from the foregoing illustration which he gives of segregation in the inorganic world he would seem to have some such process in his mind. In that case I may point out that, as indeed follows from what I have already said, the rate of diffusion of enantiomorphs is the same; no such separation is possible.

If Mr. Spencer will consider this absolutely identical behaviour of enantiomorphs under all symmetric influences, I think he will perceive that the phenomena of "the formation of hæmatite nodules and flints in chalk-formations, or of siliceous concretions in limestone"—phenomena in which only crystalline or crypto-crystalline compounds of symmetric molecular structure are concerned, and which occur under the influence of symmetric forces—are not comparable with the separation of two enantiomorphous colloids such as dextro-protein and *levo*-protein. Short of some asymmetric influence, nothing could separate these; and I am still waiting for my critics to tell me where, prior to the existence of life, such an influence was to be found.

Prof. Errera writes with special knowledge of the subject of molecular asymmetry, and I have nothing to criticise in his statements, so far as they deal with known fact or accepted theory. Some of his suggestions are exceedingly ingenious. I must admit, for example, that a force neither intelligent nor living—a symmetric mechanism—might be conceived which would pick out a single crystal from a mixture of crystallised enantiomorphs, and thus yield a single asymmetric compound. This is, then, so far a solution of the problem, although not a solution in the sense which I contemplated, since the mechanism cannot be trusted to effect the separation of the same asymmetric form twice running, whereas the living organism, or the intelligent operator, can do so any number of times. This is the essential difference between symmetric chance and asymmetric life. It is a feat which no mechanism could perform, unless its constructor had first embodied in it the idea of asym-

metry, when it would cease to be symmetric, and would be an asymmetric product of living intelligence.

Moreover, as Prof. Errera will perceive from my reply to the arguments of Prof. Fitzgerald and Mr. Bartrum, I do not consider that the separation of enantiomorphous crystals brings us much nearer to the spontaneous formation of those *non-crystalline* asymmetric substances that build up the living organism. Prof. Errera, it is true, goes a step further in this direction than my other critics by pointing out that—as indeed I emphasised in my address—the further chemical transformation of such an asymmetric compound by the introduction of new asymmetric groups need not yield more than a single asymmetric compound.

Prof. Errera admits that his suggestions as to the manner in which the separation of enantiomorphs may have occurred before the origin of life—thus, that different asymmetric crystals may have been whirled by a vortex into different planets—are not very probable. In fact, all my 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*.

In conclusion, I repeat that it is the impossibility of any mechanical (symmetric) force constantly producing the same asymmetric form, or constantly selecting the same one of two opposite asymmetric forms—a constancy which is manifest in the same processes when effected by vital agency—to which I referred in my address. I certainly nowhere used the word "constantly"; but the idea is present throughout.

Most of my critics clearly recognise this impossibility, and therefore seek to avoid the difficulty by supposing only a few asymmetric events—or even, only a single asymmetric event—to occur. The desired result having been obtained, the initial process is assumed to stop. But in making this assumption they seem to me to do violence to all probability. Given a practically unlimited period of time, why should a particular set of mechanical conditions, acting by pure chance in a given way, not act over and over again? One can understand a gambler stopping after a run of luck in his favour; but why should a mechanical process do so?

I see no reason to withdraw any of the conclusions at which I arrived, although, had I to write my address over again, there are parts which, to guard against misunderstanding, I might express differently.

I wish to point out that the term "tetartohedral," used in my printed address in describing the asymmetric facets of quartz, is erroneously given in the NATURE report (this vol., p. 454, col. 1) as "tetrahedral."

F. R. JAPP.

The University, Aberdeen, October 24.

Potential Matter.

ALLOW me to refer once more to the subject of my letter of August 18, in order to draw attention to two previous investigations with which, at the time of writing, I was unacquainted. Prof. Karl Pearson has, under the title of "Ether Squirts" (*American Journal of Mathematics*, vol. xiii. No. 4), worked out mathematically the theory of matter considered as sources and sinks of fluid, and draws attention to the fact that this theory implies the existence of "negative matter," which may exist outside the solar system. More recently A. Föppl, in a communication to the Munich Academy, dated February 1, 1897 (*Sitzungsber. der k. b. Akad. d. Wiss.*, 1897, i. p. 93), has published a short paper under the title, "Ueber eine mögliche Erweiterung des Newton'schen Gravitations-Gesetzes." Starting from the idea that there is a difference in kind between the electrical and magnetic fields of force on the one hand, and the gravitational field on the other, because the flux of force through a sphere converges towards zero with increasing radius of the sphere for the electric and magnetic fields, but not, as usually defined, for the gravitational field, Föppl gives the necessary extension to Newtonian law of gravitation in order to remove the distinction. This, of course, implies "negative matter." There is a marked difference between the expression for the energy of the gravitational field on Föppl's hypothesis with that which is derived from the ether squirt theory; but it is not necessary to enter into this question.

There are some points in my former communication, to which previous writers on the subject have, however, not, as far as I know, drawn attention. Among them is the insufficiency of the ordinary hypothesis to account for the rotational momentum

of our solar system which cannot be self-generated, the possibility of having evidence of anti-matter in comet tails and coronal streamers, and the idea of potential matter.

ARTHUR SCHUSTER.

Solar Radiation.

AT the conclusion of his British Association lecture on Phosphorescence,¹ Mr. Jackson makes a suggestion with regard to solar radiation which will doubtless receive due attention from those who are interested in solar physics. It is one of especial interest to me because, by an entirely different train of thought, Mr. Jackson has arrived at a possible explanation of the relation between sun-spots and terrestrial magnetic disturbances which is practically identical with a suggestion I have recently put forward in a paper on "The cause of the darkness of sun-spots," published in the *Astrophysical Journal* (April 1897).

In this paper I attempted to show that absorption by relatively cool material offers no satisfactory explanation of the darkness of sun-spots, and that the spectroscopic evidence is really quite compatible with a relatively high temperature even in the umbra of a spot.

But in abandoning the absorption hypothesis, one is brought face to face with an apparent contradiction of Kirchhoff's law. Thus it is certain from the low mean density of the sun that the interior region under enormous pressures must be vastly hotter than the photosphere. If, therefore, spots are really breaks in the photospheric clouds through which we obtain a glimpse of the interior, why is it that the radiation from them is apparently so much less intense than from the photosphere? The clouds of condensed matter may, of course, possess a much higher radiating power than the gaseous mass below them; but this, according to Kirchhoff's law, should be entirely compensated by the enormous depth of the feebly radiating interior mass.

To meet this difficulty I suggested that the radiation from the interior, at the transcendent temperatures which must exist even a few thousand miles below the sun's visible surface, may possibly not be apparent as visible light, but may occur in wave-frequencies of a higher order than the known spectrum; and "may be effective in producing those magnetic disturbances which are characteristic of large umbrae."

Mr. Jackson however, if I have rightly understood him, supposes that it is not so much a question of temperature as of molecular structure that determines the wave-frequency of the radiation; and he regards the light of the photospheric clouds as a phosphorescent glow induced by undulations of a high order of frequency which are emitted by the simpler uncondensed materials. The condensed clouds containing more complex molecular groups acting as a screen, and converting the invisible radiant energy of high frequency into ordinary light.

With regard to this interesting speculation, one would like to know more particularly what is the nature of the evidence on which the idea is based that very simple molecular systems give rise to undulations of high frequency? There can scarcely be any analogy between the behaviour of matter in highly exhausted tubes and under the enormous pressures and temperatures which must exist within the photosphere.

The case of the phosphorescent limes is an exceedingly interesting one; but is there any ground for the belief that the lime obtained from organic salts, and giving a blue phosphorescence, is really simpler in molecular structure than a lime which glows red?

J. EVERSHED.

Kenley, Surrey, October 14.

Hibernating Reptilian Embryos.

WILL you allow me space to correct an error that has crept into the account given in the *Christchurch Press*, and reprinted in the last number of *NATURE* (p. 609), of Prof. Dendy's successful investigation of the development of the egg of the Tuatara lizard, *Sphenodon*.

The fact of an embryo hibernating within the egg was not, as stated, unknown among vertebrates, an exact parallel being offered by no less well-known a reptile than the European pond-tortoise (*Emys orbicularis*). This was first observed in Austria, in the last century, by Marsigli, whose statement has been corroborated by Miram in 1857, eggs laid in his garden at Kieff in May hatching eleven months later, and by Rollinat in 1894, the latter author concluding that hatching does not, as a

rule, take place in France before the twenty-second or twenty-third month after oviposition.

I need hardly add how pleased I feel at the result of Prof. Dendy's investigations showing the close resemblance which the development of *Sphenodon* bears to that of the tortoises, since I believe to have been the first systematist to follow Cope (1885) in placing the Rhynchocephalia in close proximity to the Chelonina with the remark: "The affinities of the Rhynchocephalia to the Chelonina are at least as great as to the Lacertilia" (Cat. Chelon., 1889, p. 1).

G. A. BOULENGER.

British Museum (Natural History), October 23.

Organic Variations and their Interpretation.

I SHOULD be glad if Mr. Cunningham would tell us upon what evidence he founds his opinion that, in crabs, "it is certain that the number of ecdyses depend on age, not on size."

This assumption lies at the base of Mr. Cunningham's criticism of Prof. Weldon's arguments; but, even apart from that, the matter is one of such general biological interest that I hope he will respond to an invitation to substantiate a view which to me, at any rate, is altogether novel. I have always understood that exuviation was a phenomenon essentially connected with the process of growth in Crustacea rather than with the mere passage of time, and it is needless for me to remind Mr. Cunningham of the familiar facts and published statements which support this generally accepted view. Will Mr. Cunningham, on the other hand, tell us how many cases of exuviation, unaccompanied by growth, he has observed among Crustacea?

Unless Mr. Cunningham can revolutionise the present state of knowledge on this subject, his criticism, based on the greater relative growth of young crabs in 1893 than in 1895 and 1898 (which in itself is probable enough), falls to the ground; for he admits that "change in the proportions of a crab occurs only at the ecdysis." In assuming that, on the whole, similarity of size in young shore-crabs indicates an equal number of moults, Prof. Weldon appears to me to be quite in accord with our present knowledge of the subject. Certainly—to modify Mr. Cunningham's phrase—the frequency of exuviation in different *Carini* corresponds much more closely with their relative growth than with the periods of time occupied.

Plymouth, October 22.

WALTER GARSTANG.

Wall Mirages.

MR. R. W. WOOD, who describes a mirage on city pavements, in *NATURE* of October 20 (p. 596), may like to refer to the second volume of *NATURE* (p. 337, August 25, 1870), where he will find an account of mirages seen by looking closely along a wall, which was exposed to a hot afternoon sun. The mirage must be very common, and needs only looking for. Mr. Wood's interesting letter may lead others to photograph this curious phenomenon in our own country. A wall will be easier to deal with than a pavement.

C. T. WHITMELL.

Leeds, October 22.

A White Sea.

I HAVE received several letters respecting this phenomenon (see p. 496), and have distributed the samples of water to two gentlemen who were desirous of examining it. Will you permit me to say that subsequently I received another application from a bacteriologist on the continent, and that the letter was unfortunately lost before complete perusal. Hence my failure to acknowledge its receipt.

JAMES W. BARRETT.

22 Cavendish Square.

SURFUSION IN METALS AND ALLOYS.¹

THE author points out that metals and alloys may be maintained in a fluid state at temperatures which are many degrees below their true freezing points, and states that this fact has been but little studied. As regards salts, the question of surfusion has recently received much attention. Ostwald (*Zeit. für Physikal. Chem.*, 1897, vol. xxii. p. 3) has shown, as the result of an investigation

¹ "Surfusion in Metals and Alloys." By Prof. W. C. Roberts-Austen, C.B., F.R.S. (Abstract of a paper read at the Royal Society, May 26.

¹ *NATURE*, October 6, p. 562.