

clearly I placed the screen nearly in contact with the eye. On touching the back of the screen with the finger, just under the luminous patch, there was seen to be a perceptible alteration in intensity of the light.

On pressing the screen with the finger the light seemed to become less intense, and on removing the pressure the intensity was restored. A piece of metal, whether cold or hot, produced the same effect as the finger, and therefore vital action is not necessary for the phenomenon.

At first sight it seemed as though the effect was caused by the muscular effort of the observer, for pressing the back of the head against a wall whilst the screen was in contact with the eye apparently made some difference in the brightness of the light, as also did clenching the hands, but in some cases the light was brighter and in others fainter, so that not much reliance can be placed upon the observation. Another person's hand was as efficacious as that of the observer.

I tried the experiment on two other persons without telling them what was to be observed, and in both cases they said "the light is fading."

The experiment is very easily repeated, and it might be interesting to know whether it can be seen more readily than the *n*-ray manifestation. The screen must not be very bright; a zinc sulphide screen does not answer at all.

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Earth Structure.

PROF. MILNE, in reviewing Mr. T. Mellard Reade's book, "The Evolution of Earth Structure, with a Theory of Geomorphic Changes," emphasises the demand at the present time for some theory which shall explain "pulsatory movements by which large tracts have been alternately raised and lowered." Mr. Reade has attempted to supply the want, but, as Prof. Milne points out, his explanation is in some respects not very fundamental.

Just before any solidification had occurred, the hot viscous gases which originally composed our earth, under the combined influence of gravity and diffusion, arranged themselves in such a way, each according to its density, that the heaviest swarmed towards the centre, where the pressure was also highest, and diminished in concentration towards the outside, where the pressure was at the same time lowest. In this way an infinite number of layers would have been formed, in which the change of composition varied gradually but continuously from the centre to the outside, and the total composition at depths far apart was widely different. When such a mass goes on cooling, where will solidification take place?

The temperature of the earth's centre is probably much higher than the critical temperatures of substances with which we are acquainted, and such substances would therefore be gaseous there. Possible exceptions are the platinum metals, a few other elements, and *endothermic* compounds. The latter are quite stable and almost certainly easily formed at these temperatures and pressures, and they can also most probably remain solid at enormously high temperatures. If so, it is obvious that solidification in the mass of the earth's gases would have very soon occurred, not merely on the outside, where the temperature gradient was always very steep, but at some one or more, possibly deeply situated places where the layers happened to be of a composition more suitable in the circumstances, for the making of infusible *endothermic* substances—to us probably unknown—than those nearer the outside. There would then be layers of fluid sandwiched in between layers of solid. If we suppose the temperature coefficient of expansion in these localities to be similar to those met with on the earth's surface, any particular solid layer there would, as the whole earth cools, contract more quickly than the layers underneath it, until the solid would at length give way, and an escape of magma through the rupture would relieve the tension. All the layers above, including the outer crust, would respond to this explosion; but in time the customary steady rate of cooling and contraction would be resumed, which ultimately leading up to another "blow off," would culminate in the continued repetition of the whole process.

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Endothermic compounds can store enormous quantities of internal energy, which in suitable circumstances offering conditions of less stability can be discharged. The simple rupture would therefore be complicated by the production of chemical changes, by expansion and contraction locally, and possibly even by regelation. If in the earth's interior the coefficient of expansion increases with the temperature or with the pressure, there would be a tendency for the centre to shrink away from the layers above, and vesicles would result, as pointed out long ago by Fisher. As these vesicles would get larger and larger, a solid layer anywhere here would be subjected to a strain as a result of the differential motion above and below it, and a collapse—obviously tending to become recurrent—would occur, which would ultimately affect the outer crust periodically or spasmodically.

It is tolerably certain that under oceans the outer crust is much thicker than it is under the continents; the temperature gradients teach us this, and plumb-line measurements are also probably, most simply so explained. When a movement occurs below, we should consequently expect that the crust would "give" more under the continents than under the oceans. This would in the long run be an agency tending to counteract the effects of denudation, &c., so that continents should rise relatively to the oceans; and though no doubt local conditions could easily modify the easy applicability of this generalisation to any particular case, this seems an eminently useful conclusion.

It is not at once obvious that there is anything, so far, at variance with any of the well-known facts discovered by the labours of Hopkins, Kelvin, Delauney, Darwin, Fisher and others; on the contrary, the joint results of their work seem to require a combination of solid and fluid which here appears to find adequate satisfaction. When it is so generally assumed that the first portion of the earth to solidify was the outermost part, it is, perhaps, not unnecessary to point out that that is, as a matter of fact, not quite true. In the atmosphere and hydrosphere we have the lightest—and fortuitously the most volatile—of those gases which originally cooled to make our planet. Quite irrespective of what may happen below us, we know for certain that the outside of the earth must cool some 200 degrees before our descendants or their helium-breathing and demon-like representatives will find solid air among the constituents of the rocks under our feet.

Kristiania, February 5.

CHARLES J. J. FOX.

Asymmetric Synthesis.

WITHOUT wishing to detract from the ingenuity displayed by Dr. W. Marckwald in his "asymmetric synthesis" of valeric acid, noticed in your issue of February 25, we desire to point out that it is not a true asymmetric synthesis in the ordinary sense.

The ion of the acid or acid salt of ethylmethylmalonic acid may exist in solution in two enantiomorphously related forms



On the addition of brucine the least soluble salt will crystallise out. This is no more than the resolution of an externally compensated ion by Pasteur's well-known method. Both components of the salt will be active. The subsequent decomposition will, if no racemisation occurs, of necessity give rise to an active acid. Now this is a very different thing from the problem attacked by Fischer and Slimmer, Cohen and Whiteley, and Kipping. In these cases the original substance which is to be rendered active is not capable of existing in enantiomorphously related forms until it is submitted to chemical change in combination with the active substance.

It is for this reason that we consider that Dr. Marckwald cannot claim to have accomplished a true asymmetric synthesis.

J. B. COHEN.

T. S. PATTERSON.

The Yorkshire College, Leeds, February 26.