surely 'real' species, whether other forms called species are 'real' or not. We find no evidence that such species could not or do not originate, sometimes at least, through slight fluctuations acted upon by selection in segregation. We do not know that the effects of selection have any final limit except in certain cases where the limit is mechanical. It is not yet clearly shown that there is any real and fundamental difference between continuous and discontinuous variation, and most zoologists regard the conception and cycles of variation in the history of a species as an ingenious suggestion rather than as a part of science.

It is evident that there is much—very much—about animals and plants, which can be learned only from experimentation under changed conditions, as there is much that can not be known or even imagined without the aid of the microscope, and much that can not be known or imagined without the comparative study of many individuals and the comparison of faunal and floral areas. We must welcome the study of pedigreed individuals, animals or plants, as a most hopeful line of investigation, and it is certain that the discoveries it may yield can not be forestalled in advance. If they could the investigation would be unnecessary. So far as species are concerned, it is clear that a large part of the problem demands the study of the structure of forms and their relation to environment. There is much truth in Darwin's words that "One has hardly a right to examine the question of species who has not minutely described many."

As to the suggestion of the possible hybrid origin of Œnothera, the writer is not a botanist, and very much of botanical investigation escapes his notice. He is pleased to learn that the possibility of such origin on the part of *Œnothera lamarckiana* has been considered and fully disproved. A detailed account of the experiments which show this would be interesting. It would also be interesting to know the degree in which Burbank's hybrid walnuts of the second generation, showing 'every conceivable kind of variation,' conform to the Mendelian theory.

As to the theory that species are permanently changed by the direct impact of environment, which most faunal zoologists in America seem to accept, the writer thinks that Dr. MacDougal is probably right in claiming that "no evidence has yet been obtained to prove that the influence of tillage, cultivation or the mere pressure of environmental factors has produced any permanent changes in hereditary characters of unified strains of plants," or of animals either.

DAVID STARR JORDAN.

VULCANISM.

I HAVE read the article of Elihu Thomson,[1] much of which is necessarily true, with considerable interest; but I doubt whether I can go so far as he does, partly because I have a pet theory of my own to nurture. What I miss in Thomson's article is some definite estimate or clear-cut specification of the actual conditions involved: how much stuff is moved; what work is spent; how much heat is generated. I have endeavored to picture the occurrences to myself in a cursory way for a normal case, somewhat as follows: The work done per cubic centimeter will in any distortion be half the product of the stress and the strain. This work will be elastically potentialized if the solid remains intact. If there is rupture it will appear as heat largely near the surface of separation. If it yields viscously (as is much the more probable) it will appear throughout the volume. The strain is probably a shear. The question at issue is then under what circumstances of torment must one shear a rock in order to melt it. Suppose we say the shear is one half, *i. e.*, if the tangential thrust is horizontal all initially vertical lines will be inclined thirty degrees; or in general there will be corresponding changes of inclination of thirty degrees, which seems to me to be enormous, but may, nevertheless, be admitted for the purpose of argument. We may then write, if the density of rock is 3, its specific heat .2, its igneous melting point as low as 1,000° C.,

$$\tfrac{1}{2} \times \tfrac{1}{2} \times F = 3 \times .2 \times 1000 \times 42 \times 10^6,$$

[1] SCIENCE, XXIV., p. 161, 1906.

to determine the tangential stress at F involved. It follows that $F = 10^{11}$ dynes/cm.^{2} or 10^5 kg./cm.^{2}; or since in a shear the tangential and the normal stresses are equal per square centimeter, about 100,000 atmospheres, even for the excessive strain in question. Now in a region where differential stresses of this value abound, the pressure itself must be at least of the same order, and hence if we compute such pressures hydrostatically (a case most favorable to shallowness of the seat of reaction) with ten feet of rock to the atmosphere, this would be equivalent to a depth below the surface of one million feet or 190 miles, where a shear of the value of one half is surely out of the question. Imagine the earth radii all flexed by this amount at their outer ends. Besides we are much too far down for practical vulcanism. Of course, we can get nearer the surface with bigger strains and smaller stresses, or we may imagine the energy of a fault all spilled upon the surface of rupture; but even in this case while the work done will depend on the volume displaced, it will also in a large measure be dissipated within that volume and by no means on the surface of separation alone.

The picture as a whole is not alluring because it is vague, to me at least, who am all the while fondling my own little notion. In fact I once came as near being a physical geologist as Elihu Thomson, though nobody seems to have found it out. Yet in the days when I still deluded myself into thinking such things interesting I happened upon an astonishing result in the endeavor to dissolve hot glass in hot water.[2] It did in fact dissolve to an eventually solid substance, which for hardness and optical character was not distinguishable superficially from the igneous glass; and it did this completely (in water, not in steam) at a temperature certainly much *below* 200° C. and in such a way (this is the point) that the system of igneous glass and liquid water contracted on combination as much as 20 or 30 per cent. of the initial total volume. Think of this; the contraction of concentrated sulphuric acid upon admixture with water is but 2 to 3 per cent., and even granting that molecular changes and not volume contractions are the truly important features, the case for water-glass can not be so easily dismissed. Whoever has tried to coerce a solid-liquid system (as I did) knows that he has a task on his hands; and whoever tries to diminish bulk 20 or 30 per cent. (which I didn't do) is destined to fall very short of his hopes. I argued, therefore, that the solidification of water in glass, under the exceptional conditions stated could hardly take place without the evolution of heat such as accompanies any solidification of the liquid. All efforts to prove the inference directly miscarried; but in case of a reaction which proceeds very gradually, in small compass and under high pressure, failure is almost a foregone conclusion. Nature, as Elihu Thomson truly states, in her ideal laboratory can garner the heat of a slow process, while such heat slips irrecoverably through our fingers in the workshop. At this point then my argument is based not on direct but on circumstantial evidence, and if I were the reader, and not the writer, I would merely grant a fused glass at a temperature presumably somewhat higher than the melting point.

To me the picture obtainable from the bearing of these experiments on vulcanism is more attractive than any other with which I happen to be acquainted, even if I have to chaffer uncomfortably for the excess of heat above mere fusion which seems to be present, as if my glass had gnawed its way convectively into the higher temperatures of deeper isotherms. The idea[3] here is important: it is probable that the water in a magma at 200° will diffuse into a magma at 300° (and higher in turn), across the surface of contact. The region of fusion is, therefore, essentially *sinking* in character in its avidity for magmas at

[2] *American Journ*, XLVI., p. 110, 1891; VII., p. 1, 1899; IX., p. 161, 1900. *Phil. Mag.*, XLVII., p. 104, 461, 1899.

[3] Inferred because the rate of solution increases rapidly as temperature rises. Moreover the higher temperatures of deeper isotherms are being continually brought from lower to higher levels by convection, as solution proceeds.

higher temperatures. If the supply of water holds out above, the fused region will enlarge downward and laterally until, with excessive size, the rigidity of its confines breaks down.

To keep water liquid at 200° C. it is merely necessary to tap the ocean at a level greater than 500 feet below the surface, while a depth of five miles of water may be available. The 200° isotherm may also be put at a distance of about five miles below the solid surface, but it is correspondingly lifted up on the shores of the ocean. Near the ocean, therefore, this earth level is potentially fused, if by whatever catastrophe the ocean penetrates as far as the 200° isotherm, barring the 3,000 atmospheres of pressure which one may ascribe to the given depth of the isotherm below the surface. In view of the rigidity of rock, such pressures are not yet irresistible, when burdening the solid framework of a region not too large. The effective pressures are smaller. Fusion will depend upon the character of the rock magma found in place; it will be rapid if basic, slow if acid, but will in every case constitute a local source of heat, since as in Thomson's case, the region of reaction is nicely jacketed in a way to guarantee the utmost amount of mischief. More than this; water-glass becomes saturated subject to temperature and pressure, after which the heat reaction ceases, and the chances for vulcanism become extinct. Furthermore, there is a chance for periods. Finally one would expect the region of volcanic activity to correspond in depth with the depth of the ocean; and again to be on the margin of the ocean without being necessarily absent in the interior of continents. Could anything be more cleverly dovetailed? What if the heathen rage and say 'qualitative' or 'inadequate'; what if throughout all the turmoil of the Pelée eruptions, not a soul has thought it worth while to quote my results. I am now doing this myself.

But Elihu Thomson will have none of it. 'No water would enter a hot stratum unless forced in by pressure in excess of that which the steam would acquire upon its generation,' etc. Unfortunately we have to do with much more than a mechanical phenomenon. The chief pressures in question are capillary and osmotic pressures. Steam will pass through porous rock against very considerable pressures.[4] I remember that I once had an occasion to pass a very fine spray of air through water. Nothing seemed simpler: I tried to force the air through a submerged cup of unglazed porcelain. But it would not work! A little consideration showed me afterwards that it takes ten atmospheres to do that, and the wretched old trap blew up before this pressure was reached. Through dry porcelain the air escapes jauntily enough, but it will not do so if the pores are stoppered with water. One may estimate in the same way that pores of molecular dimensions, as in case of osmotic phenomena, and diffusion would call for many thousands of atmospheres if water is to be forced through, so that the pump which feeds the vulcanic boiler to use Thomson's image, is not a cast-iron contrivance. But apart from this, having once fused my glass I am at liberty to putty up every fissure that may interfere with my business. I am quite unwilling to leave Elihu Thomson a single crack to puff away my steam, unless it be the cataclysmal break-up by which my glass, pumiced or not, or any of its ingredients, water and mud, are finally ejected. Here I can accommodate him periodically.

Major C. E. Dutton's recent article[5] breaks off entirely with his old-fashioned comrades and looks at volcanoes from a new point of view. I always read Dutton at arm's length when I differ from him, lest the trenchancy of his style rob me of the charm of my own convictions. And the case here in question is even more disquieting. Whoever invokes radio-activity silences most of us; for if the incantation be potent enough, there is very little that the wily electron can not do. But in this instance, not a few have been in quest

[4] I have not the data at hand, but they will be found, if I remember, in Oscar Peschel's 'Erdkunde.'

[5] 'Volcanoes and Radioactivity,' *Nat. Academy*, April 17, 1906.

of radio-active fortunes supposedly stored in the bowels of the earth. In one of the last annalen, August Becker,[6] studying the lavas of Vesuvius in the Lenard's laboratory, detects no unusual radioactivity in the magmas from deep sources, while Lord Kelvin has lately girded his gravitational vestments anew, and is thundering in the *Times* for a return to the simple life, free from radio-active refinements.

We may summarize, therefore, that in each case specific evidence for the adequate occurrence or the localizations of volcanic heat is wanting. Apart from this the manufacture of volcanoes is as easy as an after-dinner discussion. Suppose, for instance, we all got to work conjointly; let me supply the broth, as I trust, thick and hot, while Elihu Thomson kneads in the energy and Major Dutton bombards the whole with a particles. Could anything withstand us? True there has been stuff predicted

> "Impenetrable, impaled with circling fire,
> Yet unconsumed,"

but this need not be mentioned (at least not in the summer), as it is gravely questioned whether it will fit into the periodic law, and it does not concern us if we are good.

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THE RIGIDITY OF THE EARTH.

To the Editor of Science: In his discussions of the interior condition of the earth (Science, September 7, 1906, and elsewhere), Professor T. J. J. See advances the proposition that the interior matter of the earth is at the same time fluid and highly rigid. Taking the words in their accepted meaning this is a contradiction in terms. If the intended meaning is that deep-seated material is kept solid only by pressure, it is of course no new hypothesis. The experimental evidence for rigidity, which has been adduced by Kelvin, Darwin and others, concerns, however, only the actual present rigidity of the earth, and has no bearing upon the question whether this is or is not due to pressure.

[6] *Annalen der Physik*, XX., p. 634, 1906.

Professor See's own supposed deduction of the earth's rigidity (*Astronomische Nachrichten*, 4104) apparently rests upon a complete misunderstanding of the meaning of modulus of rigidity. He quotes from Kelvin a definition of this modulus stated in a somewhat unusual form which seems to have misled Professor See as to its meaning, although this is made quite clear by the context. The definition quoted is from the article on Elasticity, Encyclopedia Britannica, Vol. VII., p. 805, and is as follows:

> The modulus of rigidity of an isotropic substance is the amount of normal traction or pressure per unit area, divided by twice the amount of elongation in the direction of the traction or of contraction in the direction of the pressure when a piece of the substance is subjected to a stress producing uniform distortion.

The context shows that this definition refers to a body subjected to a traction in one direction, an equal pressure in a rectangular direction, and zero stress in the third rectangular direction. The accompanying strain is the 'uniform distortion' referred to in the definition. With this understanding the definition is exactly equivalent to the more common definition which immediately precedes the one quoted, and which reads as follows:

> The 'modulus of rigidity' of an isotropic solid is the amount of tangential stress divided by the deformation it produces.

For a fluid the value of the modulus of rigidity as thus defined is necessarily zero. Professor See, however, apparently infers from the definition quoted by him that the modulus of rigidity of any body, solid or fluid, is equal to the normal pressure to which it happens to be subjected. At all events this is the basis of the method by which he computes the rigidity of the earth and of other planets. Assuming Laplace's law of density and the resulting distribution of interior pressure, he computes the average pressure throughout the earth and calls this the mean value of the modulus of rigidity for the earth. Of course, Kelvin's definition admits of no such interpretation.

L. M. Hoskins.

Palo Alto, Cal.,
September 13, 1906.