of the moon mass from the area which is now the Pacific was responsible for this peculiar fact.

In 1926 I had a number of mimeographed copies made of a short article on "The Origin of the Moon" which consisted of about 14 ordinary letter size sheets of typewritten material.

By 1927 I became impressed more with the earthly effects and had a twenty-six page booklet printed giving a large number of facts which seemed to me to support the theory that the removal of a large mass of crustal material, from what is now part of the Pacific Ocean, caused the shifting of the axis of rotation of the earth, caused the magnetic poles to become closer to each other on the Pacific side of the globe and created the major outlines of the continents. The title of this booklet is "The Formation of the Continents and Oceans as We Know Them."

In the September, 1928, issue of the *Pan-American Geologist* an article of mine on "Symmetric Disposition of Tertic Mountain Systems" was published. This calls attention to a very remarkable symmetry which is created when, on a globe, the magnetic poles, together with underlying continents, are placed back in their assumed original positions.

In the March, 1929, issue of the same journal another article of mine on "Continental Drifting in Northwestern Europe" was published. This article was not confined to a statement of the one bit of contributory evidence which the title indicates, but covers briefly some of the major features of my theory and the evidence supporting it.

In the May, 1930, issue of the same journal an article which I contributed on "Bilateral Symmetry of Earth's Largest Continental Block," with an illustration, described a symmetry of Europe, Asia and Africa around a great circle passing through the south magnetic pole, which I attribute to the removal of a large mass from part of the Pacific, which mass may now be our moon.

Before the Geological Section of the American Association for the Advancement of Science, at their Des Moines meeting last winter, I read a paper, with lantern illustrations, setting forth my theory and the facts on which it was built as well as a small part of the supporting evidence.

I have seen no other mention of this peculiar relationship that seems to exist between the magnetic elements of the earth and the major features of the earth.

Some of the conclusions which the evidence in the case has forced me to are almost revolutionary.

My theory, in a very peculiar manner, seems to fit in, to a certain degree, with Wegener's theory of continental drift of the Americas, so I submitted my theory to W. A. J. M. van Waterschoot van der Gracht, who recently conducted a symposium on the theory of continental drift. Of my theory and the facts which I advance in support of it he recently wrote me as follows:

> These curious magnetic facts must have *some* explanation, and they may be very important for further speculation as to the internal constitution of the earth, and also for the changes in its facial expression. . . . I think that your work brings some very interesting new facts and arguments into the discussion of this most involved problem. . . . Your discussion of the magnetic situation is very interesting and certainly deserves further work and thought.

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THE CONCEPTION OF BALANCE WITH RESPECT TO THE ABSORPTION OF NITROGEN, PHOSPHORUS AND POTASSIUM BY PLANTS AND THE INFLUENCE OF THE LEVEL OF NUTRITION

IN a recent paper, the writer[1] called attention to the remarkable and consistent results obtained over a 10-year period by Lagatu and Maume[2] from a series of field experiments conducted with the grape (*Vitis vinifera*). These authorities concluded that the depression in yields produced by the application of an "incomplete" fertilizer, *i.e.*, one containing only two of the principal fertilizer constituents, nitrogen, phosphorus, potassium, is not due to a *depression* of the absorption of the remaining elements, but, on the contrary, to a nutritional lack of balance owing to an *increased* absorption of these elements.

Field experiments have also been reported[3] in which it was noted that the omission of potassium from a fertilizer applied to a soil deficient in this element resulted in an *increased* absorption by the plants of the nitrogen and phosphorus present in the "incomplete" fertilizer. But in these experiments of Wallace it is to be noted that the omission of nitrogen from the fertilizer resulted in a *decreased* absorption of phosphorus and potassium, and the omission of phosphorus in a *decreased* absorption of nitrogen and potassium.

In the course of experiments[4] with *Pyrus malus* L. grown under controlled conditions, the writer has had a unique opportunity of examining the principles

[1] Walter Thomas, SCIENCE, 70: 382–384, 1929.

[2] H. Lagatu, *Compt. Rend.*, 172: 129, 1921; H. Lagatu and L. Maume, *Compt. Rend.*, 179: 782, 1924; *ibid.*, 179: 932, 1924; *ibid.*, 180: 1179, 1925; "Communication au Congrès des engrais azotés de synthèse à Montpellier," Juin 1, 1927, pp. 1–15.

[3] T. Wallace, *Jour. Pomol. and Hort. Sci.*, 7: 130–145, 1928.

[4] Walter Thomas and R. D. Anthony, *Proc. Am. Soc. Hort. Sci.*, 81–87, 1926; Walter Thomas, *Plant Physiology*, 2: 109–137, 1927.

enunciated by Lagatu and Maume. It has been found in our experiments that the course of absorption of nitrogen, phosphorus and potassium from fertilizer mixtures containing only two of them is in accordance with the deductions made by Liebig.[1] Thus, a comparison of the absorption graphs of trees receiving additions of two only of the elements, nitrogen, phosphorus, potassium, with the absorption graphs of trees which received additions of all these elements indicates a depression of the absorption of the elements from the "incomplete" or unbalanced fertilizer.

Since, therefore, generalization of Lagatu and Maume's principles is not permissible, it is pertinent to seek an explanation of the causes operative. The discrepancy between the results of the experiments under discussion may not be attributed to differences in the ratios, amounts or composition of the fertilizers applied, for these are very similar. The conception of physiological balance resulting from Loeb's[5] pioneer experiments and its further expansion by McCool,[6] Osterhout[7] and others have stimulated a vast amount of investigation by plant and animal physiologists and, although quantitative experiments[8] with plants have been made to ascertain the factors influencing the selective absorption (diffusion) into the cell of one salt (or ion) by another present in the nutrient solution, the discovery of a general law applicable under all conditions has not been forthcoming. It is, however, apparent from such experiments that there exists for each species a physiologically balanced nutrient solution—which may in actual field practice be determined by the method of Mitscherlich[9]—from which normal permeability occurs; and that a departure from this balance will produce a disturbance in the rate of absorption relations of the various ions that may have a profound effect on metabolism.[7] Normal permeability has been explained[10] on the basis of antagonistic salt action; but the extent to which salt (or ion) antagonism is responsible for the maintenance of normal permeability of plants grown under field conditions is problematical. In the field experiments here cited the results do not appear to be applicable on the basis of simple antagonism—at least in the sense defined by Loeb[10] and as discussed more recently by Loehwing[11]—for plants grown on these soils containing normal concentrations of calcium ions absorbed more (not less) potassium from a fertilizer containing only nitrogen and phosphorus. There is evidence,[7] moreover, to show that antagonism becomes weaker and weaker as the concentration decreases. Thus, Osterhout found that although 0.05 M NaCl + 0.06 M $CaCl_2$ exerted a marked toxic effect on root growth, 0.001 M + 0.0012 M solutions, respectively, of this mixture had no antagonistic effect. We should expect, therefore, little or no antagonistic effect in solutions of such low concentration as that of the soil solution. For example, the concentration of salts in the soil in the present experiments varied from 350 to 640 p.p.m. according to the season. In all nutrient culture experiments with seedling plants in which the phenomenon of antagonism has been observed, the concentration of salts present is from five to twenty times that of the salts in the soil solution from normal soils.

However, although ion antagonism may be of negligible consequence in solutions as dilute as the soil solution, experimental evidence exists to show that the concentration of nutrients (level of nutrition,[9, 12] *i.e.*, rate of supply of nutrients[13] or supplying power[14]) of the soil solution may be the factor of greatest influence in determining the course of absorption. Thus, from Remy's[15] numerous field experiments it is apparent that the addition of any two of the elements, nitrogen, phosphorus, potassium, to soils relatively deficient in these elements, such as the Hagerstown clay loam soil used in the experiments of the writer, results in a *decrease* in the absorption of the remaining element in accordance with the deductions of Liebig.[1] This decreased absorption does not occur on soils—such as that used by Lagatu and Maume[2]—well supplied with these elements, but always an increased absorption.[15,16] In this connection it is of interest to note that Waynick[17] observed

[5] Jacques Loeb, "Opperheimer's Handbuch der Biochemie des Menschen und der Tiere," Zweiter Bd., Teil I, Gustav Fischer, Jena, 1909.

[6] M. M. McCool, Cornell Univ. Agr. Exp. Sta. Memoir No. 2, 119–216, 1913.

[7] W. J. V. Osterhout, "Injury, Recovery and Death in Relation to Conductivity and Permeability," J. B. Lippincott Company, Philadelphia, 1922.

[8] D. D. Waynick, *Univ. Calif. Pub. Agr. Sci.*, 3: 135–242, 1918; D. R. Hoagland and J. C. Martin, Univ. Calif. Pub. Agr. Exp. Sta., Tech. Paper No. 8: 1–26, 1923; H. S. Reed and A. R. C. Haas, *Jour. Agr. Res.*, 24: 801–814, 1923; Univ. Calif. Pub. Agr. Exp. Sta., Tech. Paper No. 11: 1–23, 1923; Univ. Calif. Pub. Agr. Exp. Sta., Tech. Paper No. 17: 1–75, 1924; D. R. Hoagland and A. R. Davis, *New Phytologist*, 24: 99–111, 1925; E. Pantanelli, *Protoplasma*, 7: 129–137, 1929.

[9] A. Mitscherlich, "Die Bestimmung des Düngerbedürfnisses des Bodens," Paul Parey, Berlin, 1925.

[10] Jacques Loeb, "Dynamics of Living Matter," Columbia University Press, New York, 1906; *Biochem. Ztschr.*, 32: 308–322, 1911; SCIENCE, 36: 637–639, 1912.

[11] Walter F. Loehwing, *Plant Physiology*, 3: 261–275, 1928.

[12] H. P. Cooper, *Plant Physiology*, 5: 193–214, 1930.

[13] D. R. Hoagland, *Jour. Agr. Res.*, 18: 73–117, 1919.

[14] Burton E. Livingston, Proc. Intern. Congress Plant Sciences, Ithaca (1926), Vol. 2, pp. 1107–1121, the Collegiate Press, Menasha, Wisconsin, 1929.

[15] Theodor Remy, "Untersuchungen über das Kalidüngerbedürfnis der Gerste," Paul Parey, Berlin, 1898.

[16] F. Sekera, *Ztschr. für Pflanzenernähr. u. Düngung*, 7–B: 533–539, 1928.

[17] D. D. Waynick, *Univ. Calif. Pub. Agr. Sci.*, 3: 135–242, 1918.

in culture solutions greater antagonism between 0.04 M $MgSO_4$ + 0.18 M KCl than from mixtures containing higher concentrations of $MgSO_4$.

The factors involved in the absorption of salts (or ions) by plants have been discussed by the writer,[18] and a recent paper by Cooper[12] presents some stimulating new ideas on the subject. The causal relations are known to be extremely complex. Nevertheless, although the factors producing differential absorption and influencing utilization of elements within the plant at the different planes of nutrition may not at the present stage of our knowledge be identified, the interpretation advanced to account for the discrepancy between Lagatu and Maume's results and those of the writer is the only one that, at the present stage, accounts for the observed facts. Details of the experiments will be published elsewhere.

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THE ORGANIC WORLD AND THE CAUSAL PRINCIPLE: A CRITICISM[1]

WITH some reservations, the theory of evolution as propounded by Darwin three quarters of a century ago is accepted by most psychologists of the present day. We use accepted advisedly. To the psychologist the view-point and all the accumulated data are gifts. As a group we have done little to advance this illuminating principle nor have we been greatly interested in understanding the far-reaching significance of its many aspects. We have been content to believe but not to strengthen the basis for belief. Ours has largely been a lip service to Darwin and this in spite of treatises of imposing titles purporting to deal with one or another aspect of the evolution of mental life. Having accepted evolution as fundamental to our science we have not oriented our concepts with regard to it. Not uncommonly we observe that an author may profess to a purely mechanistic view-point on one page and on the next offer "inhibition" as the solution of some felt difficulty. The "inhibition" is not evaluated in the light of the "mechanism." Inhibition, in any form in which we have seen it stated, is in opposition to at least one of those general principles which we have come to call the laws of nature.

Perhaps it is the feeling of a lack of critical evaluation of our concepts which leads our students to question whether or not psychology is a science. Possibly it is the same vague feeling on our part which motivates us either to spend valuable time and energy in demonstrating in our text-books and class-rooms that psychology is a science, or to assume the "I don't care" attitude. That this lack has been felt is indicated in a new note that has recently been struck by Warren in his vice-presidential address. Warren clearly sees that a vast amount of revision must be made in our mode of thinking if we are to make full use of the principle of evolution. Primarily his article is an attempt to demonstrate that principles of causation characteristic of organisms may be assumed without damage to the mechanistic conception of life. Two such principles, he concludes, are natural selection and anticipation.

It is in hope of furthering rather than opposing the general point of view that we raise the question: Are these two principles characteristic of biological systems and are they causally related to evolution? There seem some grounds for believing that they are not, but before entering a discussion of causation it will be well to specify what we conceive the term to mean. A cause, we understand, is any event which directly or indirectly delivers energy to another event. A clear distinction must be made between causal factors and limiting factors. Silver nitrate in a transparent container undergoes certain changes when exposed to the sunlight. Causal efficacy will hardly be attributed to the container in so simple a case. Its only influence is to limit the amount of energy delivered to the solution by the sun. It is further evident that so long as we are dealing with one time frame the delivery of energy will take place only in the forward direction, and that the assumption of retroactive causation will make a hodge-podge out of all science. Cause must precede effect.

Of the supplementary causes which Warren conceives to be characteristic of organisms he says, "The first of these supplementary principles is that of natural selection. . . . It does not occur—it has no meaning whatever—except in connection with those peculiar groupings of molecules which we call organisms. . . . It is perhaps unnecessary to-day to emphasize the importance of selective adaptation[2] in promoting organic evolution. Through its means the organization of matter takes on an entirely new trend."

In communication with Warren he informs us that he does not conceive of natural selection as a cause but rather that it is based upon causation, and nowhere in his speech will the term "cause" be found when he refers to these supplementary principles.

[18] Walter Thomas, *Soil Sci.*, 27: 249–270, 1929; *Plant Physiology*, Vol. 5, No. 4, 1930 (forthcoming).

[1] A paper appearing in SCIENCE, February 21, 1930, bearing the same title, was the publication of Howard C. Warren's address as retiring vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Des Moines, December, 1929.

[2] Natural selection and selective adaptation are used synonymously by Warren.