cancer seems to be unnecessary, since it can be explained by a physicochemical fault, and if bacteria are found it is probably accidental.

Boveri[3] (1902) suggested that irregular somatic mitosis might well account for the peculiar behavior of these peculiar cells. He did not include other mutations, however.

Cancer cells show abnormal metaplasia. Instead of "flattening out" like normal epithelial cells they remain "swollen up." There is also a shift of the nucleus towards the center. They become parasitic and live at the expense of their sister cells.

Now something has happened in such cells to change their normal metaplasia. Bacteria have been held as the causative factor in this "change of stream," but this conviction is growing less and less. *True sarcoma,* however, has been found around tape-worms (Francis C. Wood) and other intestinal worms (Philadelphia Zoo Laboratory). Another factor often advanced has been a genetic factor where cancer is regarded as a true Mendelian recessive.

This hypothesis, it might be argued, would stand or fall on whether the Mendelian law is applicable to cancer families.

However, this hypothesis holds that it is a mutation of a *somatic* cell and not of a *germ* cell, that the mutation itself dies with the individual and is not transmitted directly, that the normal resistance to such imbalance may be weakened and this lowered resistance may be transmitted as a dominant or recessive characteristic or as a multiple factor. This factor would be subject to Mendelian laws.

Note: It is a well-known fact, genetically, that mutations experimentally can be speeded up tremendously by exposure to stimulating amounts of radiation and the X-ray effect, where greater doses are destructive. Perhaps the frequently observed "skin-cancers" in Roentgenologists are due to such mutations occurring from *stimulating* exposure to the X-ray effect (the release of the cathode ray).

Would it be possible that some such agency is active in the greatly increased prevalence of cancer? Of course, it is understood that other factors are concerned both in its relative and actual increase.

Evolutionary variations in species, due to mutations in the germ cells, have been very irregular in their intensity. There have been periods of extreme activity in this field, gradually shading down to a comparatively quiescent state. It is held by some that these periods of great evolutionary expansion have coincided with the great crustal revolutions of the earth, and that they may have been due to the influence of emanations from the radioactive ores that were released in these upheavals.

It is possible that in this intensive electrical age there may be radioactivity and rays that are beyond our present knowledge, which might be sufficient to stimulate mutations in the chromosomes of somatic cells. Again this same electrical influence might be of cosmic origin.

Babcock and Collins[4] performed an experiment with *Drosophila* comparing the rates of occurrence of sex-linked lethal mutations in a street-car tunnel in San Francisco and in their laboratory. A location was discovered in the tunnel where the natural ionization radiation was fully twice as great as the radiation in their laboratory in Berkeley. The difference in rate was 2.5 times the probable error for the flies which had been exposed in the tunnel. In other words, the rate of lethal sex-linked mutation was more than doubled when the flies were transferred to a more highly *ionized* location.

Hanson and Heys[5] performed independently similar experiments in a carnotite mine in Colorado where the air was strongly ionized. Their results check closely with those of Babcock and Collins.

While these two experiments fall short of being statistically significant, it is clearly shown that ionization plays an important part in the rate of mutation of the germ cells. It would not be too fanciful to assume that such ionization would also affect the rate of mutation of the somatic cells as well.

We admit that such a suggestion would be hard to determine by research, but study of the effects of X-ray radiations of a less intensity than necessary to inhibit all reproductive activity in cells might be of value.

ROBERT S. MCCOMBS
ROBERT P. MCCOMBS

[3] Republished, "Origin of Malignant Tumors," Williams and Wilkins Company, Baltimore, 1929.

THE MAGNETIC POLES OF THE EARTH AND THE BIRTH OF THE MOON

DR. OLIVER JUSTIN LEE'S article on "The Magnetic Poles of the Earth and the Birth of the Moon" in SCIENCE of July 25 interests me greatly.

A number of years ago I was impressed with the same fact, namely, that the magnetic poles are not on the axis of the earth, which would seem to be the logical place for them, nor are they even antipodal to each other. When I found that the shortest distance between them was across the center of the Pacific I immediately began to wonder if the removal

[4] E. B. Babcock and J. L. Collins, "Natural Ionizing Radiation and the Rate of Mutation," *Nature,* 124: 227–228, 1929.

[5] F. B. Hanson and Florence Heys, "A Possible Relation between Natural (Earth) Radiation and Gene Mutation," SCIENCE, 71: 43–44, 1930.

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.

D. W. LONGFELLOW

ELK RIVER, MINNESOTA

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.