

accompanied by a diminution in the total action, and vice versa. This seems to imply something like an interchange of mass between the sun and Venus and perhaps between the sun and other planets. In other words, I think Dr. Brown was right when in 1914 ("Report of the British Association," pp. 319-21) he attributed the fluctuations to a surge spreading through the solar system, and I think he has too readily adopted the theories of others who have tried to explain them as changes in the length of the day.

On this subject I have expressed the conclusion: "We may therefore declare with confidence that while the earth's rotation may be affected by a secular retardation, it is certainly not affected by a fluctuation" (*Monthly Notices*, Dec. 1926, p. 163).

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Rotation of Dielectric Bodies in Electrostatic Fields.

THE phenomenon of rotation of dielectric-surfaced cylinders between the poles of a Wimshurst machine, described by Dr. Richardson in *NATURE* for Feb. 12 (p. 238), and recently demonstrated by him, does not appear to differ in any essential feature from that exhibited by the old toy "electrostatic motor," consisting of several insulating spokes radiating from an axle, and each surmounted by a light celluloid or other ball. This rotated between oppositely charged knobs. The cause of rotation is presumably identical in both cases. The same result could doubtless be obtained by using a conducting surface broken up by insulating strips, as on the commutator of an ordinary D.C. motor.

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Biological Fact and Theory.

I HAVE noticed that people who are good at solving cross-word puzzles are also very good at understanding those complex statements of Mendelian results. I am distressed at my inability to worry out cross-word exercises and also those F_1 , F_2 , etc., synoptical charts, for I feel I may be missing something that will help in an understanding of genetical problems. It would be comforting to know if there are other biologists who admit the same disability.

What, however, are the 'fundamentals of genetics,' and what is this 'whole discipline of biology' that is going to give us the key to the processes of development? Is breeding cats, and cocks and hens, and flies, and so on, such fundamental research? A 'gene' is, I suppose, a physico-chemical entity: at least, it ought to be such so long as we study development by chemical and physical methods. We know that it is something that grows, like a crystal of alum selects molecules of alum from its mother-liquor, or 'environment'—the two kinds of growth differ, of course, but let that pass. It selects materials from the nutritive environment and then it reassembles these materials in new chemical forms, but it also reassembles the chemical products in typical morphological constellations. How? This is the fundamental genetical problem.

Then a 'gene' has a quasi-independence even if it is 'linked.' It is interesting to see how genes retain their quasi-independent activities so as to continue to give us 'bar-eye' and 'spot' and 'plain' and 'red-eye,' for example, but still a red-eyed *Drosophila* is always a *Drosophila*, and so also with the four hundred other characters which presumably express the activities of as many genes. All the time we are looking at the morphological entity that we call *Drosophila* and 'bar-eye,' or 'red-eye,' or whatever it be, is always related to nervous system and muscles and wings and so on in a typical manner.

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Can there be a 'super-gene'; in other words, what is the physico-chemical mechanism that results in the development of *Drosophila*? This, I take it, is really what is fundamental to a knowledge of the developmental process. The appearances in the cell nucleus do not help us much, for no one now takes the crude view that the parts of the chromosomes are the genes. It is difficult to see how we shall advance along these lines by "building on the ample foundation already obtained by tens of thousands of breeding tests."

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Illinium.

IN a copy of *Gaz. Chim. Ital.* (56, 862; 1926) received a few days ago, Prof. Rolla, of Florence, claims priority for the discovery of element No. 61, and proposes for it the name Florentium on the basis of a "Plico Suggellato" filed in June 1924. Prof. Rolla began his search for the element early in 1922 (see *Z. anorg. allgem. Chem.*, 157, 571; 1926). In making his claim for priority he was, apparently, not aware of the following facts:

In 1919 the University of Illinois and the U.S. Bureau of Standards entered on a joint investigation of the arc spectra of rare earth elements, using materials resulting from long-continued fractionations carried out at the University of Illinois. The results of this investigation were published in the U.S. Bureau of Standards Scientific Papers, 421 (1921), 442 (1922), 466 (1923). In the second of these papers, published at about the time that Prof. Rolla began his work and two years before his "Plico Suggellato" was deposited, Dr. Kiess, who carried out the spectrometric studies, reported 130 spectral lines which were common to the spectra of neodymium and samarium, in the samples submitted to him by Prof. Hopkins, and says, "These lines are of unknown origin and may belong to the missing element of order No. 61, coming between neodymium and samarium."

In January 1924, again five months before the deposit of Prof. Rolla's document, L. F. Yntema published an article, "Observations on Rare Earths. XV. A Search for Element 61," in which he gives five additional lines in the ultra-violet region, and repeats the statement that these probably belong to Element No. 61 (see *J. Amer. Chem. Soc.*, 46, 37; 1924). Finally, on the basis of still further work, including the finding of two X-ray lines of the *L* series, J. A. Harris with B. S. Hopkins announced the discovery of element 61 and proposed the name Illinium (see *J. Amer. Chem. Soc.*, 48, 1594; 1926).

In the light of these facts it would seem that the honour for the discovery of No. 61 belongs primarily to Prof. Hopkins, and that the element should be called Illinium rather than Florentium. This does not detract from the credit which Prof. Rolla should receive for his independent discovery of the element. Both Prof. Rolla and Prof. Hopkins realise that a large amount of additional work must be done before the element can be fully accepted.

W. A. NOYES.

Urbana, Ill., Jan. 29.

ERRATUM.—We regret that the inscription appearing below Fig. 1 in Prof. John Percival's letter in our issue of Feb. 19, p. 280, was incorrectly printed. It should read: "Grains of wheat found in a vase on the site of a Sumerian house (3500 B.C.). Below are two rows: the upper row of the Sumerian grain, the lower of modern grains of Rivet wheat (*T. turgidum*) for comparison. (Natural size.)"