flects directly and definitely the rate or the proportionality of absorption of particular elements by a plant. Indirectly it may, and often does, indicate likewise the utilization of these elements by other organs, especially those of a higher metabolic rate and hence of a more rapid development. Due to a close physiological correlation of the various organs of a plant there is a continuous removal and diverting in unequal proportions of certain elements from particular organs, like leaves, and their reutilization for the development of other parts—fruits, shoots and roots. This is particularly true of woody perennials, like *Vitis*. It has been demonstrated by one of the writers[8] and by others[9, 10] that every one of the three elements—nitrogen, phosphorus and potassium —is removed from the lower leaves of the tomato plant whenever a shortage of a particular element for the vegetative extension or fruit development occurs. Yet Lagatu and Maume base all their interpretations solely on the analysis of basal leaves of fruiting canes of *Vitis* and of tuber-bearing plants of *Solanum*. It is not difficult to see how the concentration of any two of the three major elements of soil nutrients may increase in the leaves when one is *in minimo* for the development of other metabolically more active organs. This increase, therefore, is due to two major factors (not one): An unbalanced fertilization of the plant as a whole and an unbalanced nutrition of particular organs. In either instance (below and above ground, the "intake" and "outgo," absorption and utilization) the law of the minimum seems to hold true.

The detailed mechanism and interpretation of the metabolism and physiology of organic correlation may be analyzed on the basis of "metabolic gradients," as suggested by Child.[11]

A. E. MURNEEK
E. J. GILDEHAUS

UNIVERSITY OF MISSOURI

THE MECHANISM OF CROSSING-OVER

DARLINGTON recently published in SCIENCE[1] (73: 561–562) a criticism of my work[2] which might give the reader the impression that Darlington was not given credit for previous suggestions concerning the mechanism of crossing-over. In 1929 Darlington[3] did suggest that crossing-over is due to a reduction of chiasmata by breaking. In the three sentences which refer to this theory (pp. 50, 51 and 52) he does not explain how chromatids might break and recombine nor does he present any evidence to show that he had any conception of the significance of his suggestion. However, his suggestion was acknowledged (Sax, 1930, p. 209) as follows: "It seems very probable, however, that crossing-over between homologous chromatids is associated with the reduction in the number of chiasmas between diplotene and diakinesis, as Darlington (1929) has suggested."

In a paper which was published shortly before mine went to press, Darlington[4] does not mention his earlier suggestion that crossing-over is caused by breaks in the chiasmata, but expresses the view that cross-overs determine chiasma formation. The cytological evidence for this view is supported by a few diagrams and text figures which by no means can be considered as a "cytological demonstration of genetic crossing-over."

The fact that Darlington discarded his earlier "suggestion" on crossing-over in no way discredits or weakens my theory. Darlington has made so many assumptions concerning chromosome pairing, and has changed his mind so frequently that one would necessarily have to cite one of his numerous theories in any discussion of crossing-over.

Darlington also states that I have used diagrams and terminology borrowed from his 1929 paper. The only term used which might be credited to Darlington is "terminalisation." No figures were borrowed from him, although my figures 9 and 10 are based on his work. For this phase of my interpretation of crossing-over Darlington is given credit as follows: "The behavior of the chromosomes in triploid Hyacinths described by Darlington (1929) seems to offer an explanation of triploid crossing-over" (p. 214). The fact that Darlington did not fully appreciate the genetic significance of his cytological results in no way discredits his ability as a technician.

According to Darlington, my "genetical remarks might be taken to favor either hypothesis—for there is no decisive evidence between them." This statement is of more than doubtful validity. In none of Darlington's papers is there any explanation of the cause of breaks in chromatids, why they unite in new associations, why the two homologous chromatids almost always cross over at the same loci, of how gene duplication or deficiency could occur, or why one cross-over interferes with another. In his most re-

[8] A. E. Murneek, *Plant Physiol.*, 1: 3–56, 1926.
[9] J. H. MacGillivray, *Jour. Agr. Res.*, 34: 2: 97, 1927.
[10] G. Janssen and R. P. Bartholomew, *Jour. Agr. Res.*, 38: 8: 447, 1929.
[11] C. M. Child, "Individuality in Organisms" and "Senescence and Rejuvenescence," Univ. of Chicago Press, 1915. Also *Plant Physiol.*, 1: 1–3, 1926.
[1] C. D. Darlington, "The Mechanism of Crossing-over," SCIENCE, 73: 561–562, 1931.
[2] Karl Sax, "Chromosome Structure and the Mechanism of Crossing-over," *Jour. Arnold Arb.*, 11: 193–220, 1930.
[3] C. D. Darlington, "Meiosis in Polyploids. II. Aneuploid Hyacinths," *Jour. Gen.*, 21– 17–56, 1929.
[4] C. D. Darlington, "A Cytological Demonstration of 'Genetic' Crossing-over," *Proc. Roy. Soc.*, B. 107: 50–59, 1930.

cent paper Darlington[5] (1931) simply ignores these difficulties and has to assume several improbable hypotheses to account for the absence of crossing-over in the *Drosophila* male. All these genetic phenomena are easily and logically explained on the theory that crossing-over is due to breaks in chiasmata (Sax, 1929). The genetic analysis of chromosome behavior in *Drosophila* as developed by Morgan and his associates is a far more precise and accurate tool for the analysis of chromosome behavior than any method now available to the cytologist. Any theory of crossing-over which does not meet the strict genetic requirements can not be considered seriously.

Darlington also states that my "genetical remarks" . . . "are vitiated as evidence by his using the word chromosome in three different senses." In the paragraph referred to the term chromosome was not confined to one particular phase of meiosis, as should have been the case, but by no consistent or logical method of reasoning would it be possible to conclude that the word chromosome was used in three different senses or that the genetic evidence is vitiated.

KARL SAX

ARNOLD ARBORETUM,
HARVARD UNIVERSITY

THE DIVINING ROD AND FAKERS

I READ with much interest articles in SCIENCE as to belief in the efficacy of a divining rod. My explorations have taken me throughout the West and the South for nearly forty years. We have frequently tried experiments, or rather permitted those who have faith in "rods," to test for buried objects. I have never, in all my experience, known a single instance in which the test was of either practical or scientific value.

To be specific: During the exploration of the Cahokia Mounds, southern Illinois, there was much publicity, hence various persons with divining rods appeared. One man's apparatus was different from the usual forked stick. It consisted of a sphere about the size of a baseball at the end of a leather thong. This contained certain ingredients which were "secret" and he claimed the ball would oscillate over treasure as well as Indian remains. We afforded him facilities —the test was a failure. A rather wild-eyed individual appeared and told me that if I would give him a "thigh bone of a big Injun" he could lay hands on said bone and thus come in contact with the Indian's spirit. He was assigned quarters in a tent for a few hours, left in seclusion, and loaned a femur. I asked him afterwards how he, speaking English, could communicate with a prehistoric Cahokian. His reply was that all people in the spirit world spoke the same language!

The next season a middle-aged woman appeared and claimed ability to commune with departed Indians "now residing in the spirit world." My assistant and a workman were inclined to make sport, yet we gave her a tent and table at her request. She communed for two hours. Then she announced that we would make a great discovery somewhere within 2,000 feet, but was not specific. The price to locate accurately was ten dollars!

Some weeks later a man appeared who claimed that he was an astrologer, contended that he had spent numbers of moonlight nights wandering over the Cahokia Mounds, that said mounds were orientated according to certain stars, etc., that if we would employ him he would spend clear nights, when stars were visible, in his studies. He entered into a lengthy and somewhat erratic explanation, the upshot of which was that there were certain points where lines drawn from one star to another crossed, and the point exactly beneath [on earth] would yield important archeological specimens, etc. He was very insistent but could not designate any precise spot in which we should excavate. He also desired pay.

In southern Ohio a "Hill Billy" appeared in our camp with the usual divining rod, and was given a thorough test. We sent to the bank and got $25 in silver. We buried it secretly a few inches below the surface, scattering dead leaves, removing all fresh earth. The ball would sway at various points but didn't move when our friend passed over the "buried treasure."

At Cartersville, Georgia, a man came to our camp with his "divining rod" talk and begged for a chance to exhibit his prowess. He remained with our party two days and was given two or three men with shovels to excavate wherever he indicated. It seems that the other men made sport of their comrades who accompanied "Mr. Divining Rod" and, therefore, to keep peace in my field party it was necessary to detail a fresh crew each half day! Many test pits were sunk, nothing was found, but the owner always had the same excuse of any other person with a divining rod, *i.e.*, that the treasure was further down. In brief, each one had a good alibi. In instances where we struck ledge or boulder clay, or outcrop of limestone, etc., the explanation was the same.

In every instance, in several states, I kindly explained to these people afterwards the utter futility of such efforts. Each one, however, had supreme faith in his divining rod. Our friend in Georgia, who was with us longest, a simple-hearted and ignorant man whose family, I was informed, was in need, listened with some impatience when I urged that he throw away his rod and go to work. He answered

[5] C. D. Darlington, "Meiosis in Diploid and Tetraploid *Primula sinensis*," *Jour. Gen.*, 24: 65–96, 1931.