the undertaking is possible but that it is very widely welcomed.

Biological science does not have back of it extensive, well-organized and highly paying commercial organizations dependent on the progress of the science and contributing to its development and support, such as exist in the case of chemistry, for instance. Although the applied aspects of biology in medicine, in agriculture and fisheries are of at least equal human significance they are not organized as commercial enterprises, and hence can not contribute directly to the support of *Biological Abstracts*, as can the commercially organized chemical industries to *Chemical Abstracts*. For a long time to come, then, *Biological Abstracts*, if it is to continue, must depend on the support of enlightened philanthropy. At present its earned income from subscriptions and other sources is sufficient to pay manufacturing costs only. This agrees with the original estimates drawn up before the project was under way. But the great items of cost, consisting of editorial, indexing, bibliographic, secretarial and clerical services, which make up from two thirds to three quarters of any adequate operating budget, must be specially provided. This constitutes an enormous "overhead" which exists whether the subscription list be large or small. It is estimated to amount to over $100,000 for 1931. This is obviously a situation in which every biologist can help, by his subscription. It is, however, not expected that even with the largest list of subscriptions practically possible, the overhead charges can be paid by the receipts of the journal.

The whole enterprise of biological research is, however, so vast and its human usefulness so inestimably great that such a sum seems to be only a small tax upon it; indeed, almost vanishingly small compared with the immense sums required for primary costs of the research and original publication. If the journal should acquire still more of an international character, the tax on American biologists and American philanthropy might be correspondingly reduced; but it seems to the writer that American biologists and philanthropists who have the advancement of science at heart should not withdraw their support until the future of this comprehensive abstracting service is adequately safeguarded.

FRANK R. LILLIE

UNIVERSITY OF CHICAGO

TWISTED TREES

I HAVE read with interest the notes in SCIENCE for February 13 and March 27 dealing with trees with twisted bark. My observations covering a large part of the province of Ontario, Canada, may be of interest in this connection. In this region I have often noted the twist of evergreens, especially of the cedar (*Thuja occidentalis*), white pine (*Pinus strobus*), Norway pine (*Pinus resinosa*) and Jack pine (*Pinus divaricata*). I have never noted it on a "hardwood" in the region. In the cedar the twist is very common, straight-grained trees being far less abundant than twisted-grained. Last summer I camped on an island in Lake Kahnipiminanikok, and my party amused itself one rainy day noting this twist on cedars, some one having discovered the predominance of right-handed twists. We counted (from my notes) 312 cedars on the island; of these 219 were twisted; of these 187 were right-handed twists. Later an Indian emphasized the need of straight-grained cedars in the hewing of paddles, and the difficulty of obtaining such grains in that vicinity. He also pointed out the fact that the twist is more common in large trees than in young ones, indicating that this character is acquired by some environmental factor. This twist is not alone in the bark, but in the wood as well. It is frequently so extreme as to be a spiral. Among the white and Norway pines the twist is far more common in trees exposed to severe weather conditions, especially to strong winds. Thus I noted that twisted trees occurred more commonly on exposed rocky cliffs and small, open islands, where they receive the full brunt of winter gales. A twisted tree in the heart of the forest is quite rare. But why do they twist so predominantly to the right?

A. R. CAHN

URBANA, ILLINOIS

THE MECHANISM OF CROSSING-OVER

DR. SAX[1] has recently put forward a theory that crossing-over is due to the breaking of chiasmata in the course of terminalization. This theory is based on a suggestion of mine[2] that "crossing-over is occasioned by breaking of chiasmata." Moreover, Sax uses diagrams and terminology that are borrowed from my studies and therefore imply an interpretation in accordance with my findings.

I should like to point out therefore that I do not consider the original conjecture in any way supported by Sax's observations. I discarded the idea a year ago for reasons that are described in the accounts of studies conducted in this laboratory by Erlanson,[3] Philp and Huskins[4] and myself.[5,6,7,8,9] Briefly, the

[1] K. Sax, "Chromosome Structure and the Mechanism of Crossing-over," *J. Arnold Arboretum*, 11: 193–220, 1930.

[2] C. D. Darlington, "Meiosis in Polyploids, II. Aneuploid Hyacinths," *J. Genet.*, 21: 17–56 (see p. 52), 1929.

[3] E. W. Erlanson, "Chromosome Organisation in *Rosa*." *Cytologia*, 2 (in the press).

[4] J. Philp and C. L. Huskins, "The Cytology of *Matthiola incana* R. Br.," (especially in relation to the inheritance of double flowers), *J. Genet.*, 24 (in the press).

reasons are twofold: (1) Frequencies of chromosome pairing and of kinds of configurations occur that can be predicted only on the assumption that chiasmata do *not* break in terminalization: (2) configurations occur in polyploids and structural hybrids that are only compatible with the alternative hypothesis that crossing-over has preceded (and determined) chiasma formation. Sax's genetical remarks might be taken to favor either hypothesis—for there is no decisive evidence between them. They are however vitiated as evidence by his using the word "chromosome" in three different senses, *e.g.*, p. 209, l. 25 to mean one chromatid, l. 27 (?) two chromatids, and l. 28, four chromatids.

C. D. DARLINGTON
JOHN INNES HORTICULTURAL INSTITUTION,
LONDON

ADVANCES IN THE BIOLOGICAL SCIENCES

THE inclusion of the notes compiled by Science Service on the advances made in various branches of science during each year is a valuable feature of your paper. Permit me, however, please, to make a few comments on two of the 1930 items.

The tannic acid treatment of burns, originated by Dr. E. C. Davidson, of Detroit, was first published by him as far back as 1925. In 1929 the British Medical Research Council published a memoir by Mr. W. C. Wilson, of Edinburgh, and the results given in it "wholly confirm the claims made by Dr. Davidson for the tannic acid treatment."

Another item given in the 1930 list is the use of a neon lamp, connected in parallel with a condenser, to measure a small electric current. This device was applied by Dr. H. C. Rentschler, director of the research laboratories of the Westinghouse Lamp Company, to the measurement of the photo-electric current from a uranium cell, sensitive to ultra-violet light only. As far as I know his work was published during 1930. The same method, with minor differences, had, however, been published by Dr. J. H. J. Poole, of Trinity College, Dublin, in 1928, in the *Scientific Proceedings* of the Royal Dublin Society. When reading his paper before the society, Dr. Poole established that the rate of flashing was proportional to the photo-electric current from a sodium cell, and demonstrated the flashing to the audience by means of a two-valve amplifier, so that each flash was heard as a rap.

Through the courtesy of Dr. Poole I was able similarly to demonstrate the flashing in a course of lectures in London University in May, 1929.

At the joint discussion on photo-electric cells, held by the Physical and Optical Societies in London on June 4, 1930, J. H. J. Poole and H. H. Poole communicated a further paper on the neon discharge tube method, and gave the results obtained for the absorption coefficient of the water of Lough Bray, in the Dublin Mountains, during August, 1929. In the autumn of the same year Dr. H. H. Poole and the writer collaborated in the use of the method for submarine photometry. For this it has many advantages, as it integrates the current over a suitable time interval and permits one to obtain correct values in which the effect due to dancing of the waves is averaged out. About the same time, and since, we used the apparatus for measuring in various situations the color of daylight (and other light sources, carbon arc, etc.) with a photo-electric cell, and more recently for the study of a mercury vapor arc. Accounts of these applications are now in the press.

The method has many uses. It appears to have been invented by Dr. J. H. J. Poole, and later by Dr. Rentschler, quite independently.

W. R. G. ATKINS
MARINE BIOLOGICAL LABORATORY,
PLYMOUTH, ENGLAND

LITERATURE RELATING TO COD LIVER OIL

IN our recent "Report to the Empire Marketing Board on the Relative Values of Cod Liver Oils from Various Sources" (E. M. B. 35) an unfortunate blunder has arisen during the revision and proof-reading of the introductory chapter.

Reference 6 on page 9 is incorrectly given and should, of course, refer to the important paper by Professor Steenbock and his colleague Dr. Boutwell in the *Journal of Biological Chemistry* (1920, vol. 42, p. 131).

Immediately our attention was drawn to the error we wrote to Professor Steenbock expressing our great regret and, although he desired that we should give no further attention to the matter, we feel that we must override his wishes and make public our correction of the mistake.

On the same page the reference to Professor E. Mellanby's pioneer researches on the causation of rickets might suggest that his results were not published before 1921. We were, of course, well aware of his earlier contributions to the subject but thought it better, in a brief summary, to refer the reader to the full account of his earlier work which was published by the Medical Research Council.

[5] C. D. Darlington, "A Cytological Demonstration of 'Genetic' Crossing-over," *Proc.* Roy. Soc., 107: 50–59, 1930.

[6] C. D. Darlington, "Studies in *Fritillaria* III: Chiasma Frequency and Chromosome Pairing in *Fritillaria imperialis*," *Cytologia*, 2: 37–55, 1930.

[7] C. D. Darlington, "Meiosis in Diploid and Tetraploid *Primula sinensis*," *J. Genet.*, 24: 65–96, 1931.

[8] C. D. Darlington, "Meiosis," *Biol. Rev.* 6, (in the press).

[9] C. D. Darlington, "The Cytological Theory of Inheritance in *Oenothera*," *J. Genet.*, (in the press).