male-sterile line with chromosomes from normal lines has no effect on the sterility. Through the use of Mendelian markers (genes) it was possible to show that 9 of the 10 linkage groups were free from any factor or factors causing the sterility. Tests are incomplete for the tenth group.

2. Pollen from partially sterile plants carry no transmissible factors, either genic or cytoplasmic, for male-sterility.

3. The genetic constitution of the male parent crossed with a male sterile individual has no demonstrable effect on the degree of sterility.

4. Cytological investigation shows the meiotic divisions in microsporogenesis to be normal. The degeneration of the pollen occurs usually after the first vegetative division.

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TREES WITH TWISTED BARK

IN SCIENCE for February 13 there is an article[1] by Mr. C. K. Wentworth on "Twist in the Grain of Coniferous Trees." Mr. Wentworth points out that the bark of conifers often shows a decided twist, and that in the cases of several hundred trees which he has examined the twist is usually right-handed. He adds that he has not noticed any similar twist in deciduous trees, but supposes it may perhaps occur.

These statements have recalled to me a twist which I noticed some years ago in the barks of maples and elms. I supposed that if the effect were at all common it was probably well known to botanists. However inquiry from two botanists did not indicate that either of them happened to know of it, and in connection with Mr. Wentworth's paper I am venturing to report my observations.

The number of trees which I have observed is small, and all of them are in the city of Northampton, Massachusetts. The maples often show a rather pronounced twist in the bark, and this twist seems to be almost always right-handed. On elms a twisted bark seems to occur less frequently, but when it does occur it is usually left-handed. In one location there are two large elms with strong left-handed twist growing near to a large maple that has a strong right-handed twist.

The number of trees which I have observed is too small to permit of drawing any general conclusions, but in this small number the twist seems to be more frequent in maples than in elms, and seems to be usually right-handed in maples and left-handed in elms.

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[1] Chester K. Wentworth, SCIENCE, 73, 192, 1931.

PANAMAN OR PANAMANIAN?

IN numerous journals, including SCIENCE, and in the press, the use of the adjectival form, Panamanian, has been noted. The proper noun, Panama, does not seem to differ in any essential way from the many other proper names, geographic and personal, which end in "a."

The well-nigh universal practice in forming the adjective from such nouns is the simple addition of a final "n." Thus African and American instead of Africanian and Americanian. Other common examples of such words are: Alabama, Atlanta, Asia, Australia, California, Caligula, Dominica, Florida, Guatemala, Inca, Iowa, Korea, Maya, Montana, Nebraska, Nicaragua, Olympia, Peoria, Persia, Polynesia, Russia, Seneca, Utica, Utopia, and Volga, and most of the rest of the long roll. For these we write, in the adjective form, Albertan, Incan, Mayan, Nebraskan, Polynesian, Utican, etc., but never the double suffix, as Iowanian, Mayanian or Nebraskanian. Therefore, why Panamanian instead of Panaman?

There are some exceptions to the general rule, of course. It would not be our English language if there were not. Canada becomes Canadian and Carolina is transformed to Carolinian, probably partly from ideas as to euphony and partly from resemblance to those adjectives formed by adding "n" to a final "ia," as, Asia: Asian. But, even so, one never sees Canadanian or Carolinanian, which are exact counter-parts of Panamanian.

Another exception is China, although there is nothing wrong with Chinan except its unaccustomedness —and one never meets with Chinanian. Let us make it unanimous for Panaman!

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ESTRUS

THERE has been recently some discussion in your columns on the right use of Greek and Latin case endings in scientific nomenclature. May I draw to your attention the misuse of the word "Estrus" which has crept into the literature. This word was introduced by Heape to denote the period of sexual desire in the female and was correctly spelt by him "Œstrus," but many writers prefer the form "Estrum." It is inconceivable that the gadfly which chased the sprightly heifer, Io, into Egypt could have been anything but masculine, so this form must be, not the neuter gender but the accusative case, which is undefendable. The adjectival form "estrous" is often badly distorted too. The modifications "postestrum," "metestrum," "diestrum" and "anestrum" are

naturally correct, being the accusative cases after the prepositions.

In regard to the perversion of the initial diphthong, I have nothing to say; that is usually the result of editorial pressure and it is far from my desire to kick against the pricks.

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AN APOLOGETIC CORRECTION

IT has come to my attention that I have, on the basis of insufficient information, done an injustice to the memory of George Compere in my recently published "History of Applied Entomology." In that book I have indicated that George Compere was responsible for the introduction and establishment in California, under the impression that it was a primary enemy of the black scale, of a secondary parasite (now known as *Quaylea whittieri*) which has destroyed very largely a very important primary parasite of the scale.

Mr. Harold Compere, son of the late George Compere, himself an advanced student of parasites and a thoroughly sound scientific worker, has access to the correspondence of his late father and has found in this correspondence a distinct warning sent by George Compere to Alexander Craw (at that time in charge of the California state introductions of parasites) to the effect that this particular parasite might be a secondary and that he should be very careful about it.

This particular parasite belongs to the subfamily Encyrtinae, and at that time I knew no hyperparasite belonging to that group. Mr. Craw sent specimens to Washington for naming and for advice. I sent him a manuscript name given to it by the late Dr. W. H. Ashmead and told him that I knew of no hyperparasites of this group. This would seem to place the onus on me, and would do so were it not for the fact that I was in the habit of visiting California each year and had repeatedly talked with Mr. Craw on the subject of these parasites, warning him again and again not to liberate anything without making careful life history studies in the little quasi laboratory that he had fitted up in his office near the ferry building in San Francisco.

I gladly publish this statement, since George Compere was at one time a friend of mine and since I have the highest admiration and regard for his able son.

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SCIENTIFIC BOOKS

La Parthénogenèse. By A. VANDEL. Vol. 7 of Bibliothèque de Biologie Générale. Paris; Gaston Doin et Cie., 1931, 412 pp., 42 figures in text. Price 32 francs.

IT is a matter of surprise that prior to Professor Vandel's publication no thorough review of the subject of natural parthenogenesis had appeared, despite the prevailing tendency to summarize and digest the subject-matter of the different branches of biology. It is true that in 1920 Winkler collected a very large amount of the available findings in this field, but, aside from using it in weighing Ernst's hypothesis of hybridization as a cause of apogamy among plants, he made little use of this gathering of information. Vandel's book is therefore the first to give an adequate treatment of a subject, the extent and difficulty of which are attested by the 700 odd titles in the bibliography (which lists publications up to and including 1929). In accomplishing his task, Vandel takes up the subject under a number of wisely chosen headings. The treatment of the field observations and the breeding work under these headings is followed by a critical examination of the cytological findings, and this in turn is succeeded by a consideration of the possible points of contact with artificial parthenogenesis and a discussion of the question of sex determination. Throughout, Vandel gives a concise and clear exposition of the questions at issue and I am in sympathy with his unexpressed view that in the present state of the subject it is always preferable to broach an hypothesis rather than to leave a question entirely open. If his wholly admirable suggestions are interspersed with a few that will hardly stand up under a strict test, it must be said that all are advanced with a commendable caution. Vandel's firm and well-founded insistence that haploid and diploid parthenogenesis are basically unrelated I can only applaud.

The book has few faults. The cytological illustrations are often inadequate, but this criticism applies more to their reproduction than to their selection. The fact that the bibliography is given under chapter headings instead of being presented in one continuous alphabetical list is not without its disadvantages, although the numbering of the titles is of some aid in locating references.

It is with pleasure that I commend this work, which, far from betraying the drudgery which must have accompanied its preparation, is written with spirit and brightness.

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