taxis—which implies only the arrangement of leaves on stems—in order to criticize the absence of studies on growing-points, betrays confusion on his part. Moreover, readers will find that the points of view of the reviewer have been met in an article called "Hélices foliaires, point végétatif et stèle chez les Dicotylédones" which appeared in the *Revue générale de Botanique* (**54**, 49; 1947).

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PROF. PLANTEFOL'S chief criticism of my review seems to be that I have taken as the most important feature of his book the conclusions concerning leaf formation at the apex which he has based on his observations of the relative positions of leaves or of their scars on mature stems. He claims that even apart from these conclusions his observations have value and lead to a law which solves various morphological problems. But his book and also his recent paper make it appear throughout that this law or theory, the theory of the foliar helices, was indeed intended as a causal theory of leaf formation. Thus, for example, on p. 195 of his book, in discussing *Lilium candidum*, he refers to the three foliar helices which he finds as being a "système fait de l'activité de trois centres générateurs de feuilles, entités douées chacune de propriétés mitotiques particulières, qui se transmettent de proche en proche suivant la ligne que trace l'hélice". I maintain, therefore, that I was right in regarding his theory of the foliar helices, supplemented by his theory of the apical organiser, as being essentially a causal theory of leaf formation at the apex, though based on observations made on mature parts of shoots. If he now proposes to regard this theory as being essentially only a descriptive rule, then how can such a descriptive rule concerning the positions of leaves on mature stems explain anything, or lead to anything more than a classification of facts?

My criticism of the theory that leaves are formed along the foliar helices was that these helices are at best only one set arbitrarily selected from the two well-known sets of intersecting parastichies, or paths along which the bases of leaves or other lateral members are in contact. Prof. Plantefol claims that his helices are not arbitrary, since the bases of the lateral members are in contact along them. But so also are they in contact along the helices or parastichies of the other set which crosses the first set, as can easily be seen on a pine cone, for example; and there are no valid grounds for preferring the one set to the other. The same can be seen in the figures of bud sections in his recent paper, in which he marks the parastichies of one set as being his foliar helices, but ignores those winding round the apex in the opposite direction. Also there is no rule that the parastichies of either set are only two in Dicotyledons, though the number two is rather common for the reasons given before. So the theory is another theory of leaf formation along lines continuing parastichies into the apex, something like that of Church[1], but with the disadvantage that it is based on an arbitrary selection of parastichies. In addition, all such theories are in conflict with evidence now available from experiments on stem apices[2] which reveal no *leaf-forming* influences extending into the stem apex from below or obliquely below, but only effects *delaying* leaf-formation which are exerted by the young leaves just below the apex. The only indications of leaf-forming influences from below are at present those obtained by Weisse[3] in various ridged succulents: and those influences do not follow the parastichies but the nearly vertical ridges.

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[1] Church, A. H.. "On the Relation of Phyllotaxis to Mechanical Laws" (London, 1904).
[2] Papers in Symposia, *Soc. Exp. Biol.*, **2**, (Cambridge, 1948).
[3] Weisse, A.. *Jb. wiss. Bot.*, **39**, 343 (1904).

Threat of Disease in Tropical Crops

THE report by a committee of pathologists of three nations which has been inquiring into swollen shoot disease of cacao (His Majesty's Stationery Office, Colonial No. 236) contains matter of interest beyond the field of inquiry. Pathologists in the past have often been inclined to consider infection in a biological vacuum. The disease organism (assisted in the case of viruses by a vector) was regarded as an agent of destruction subject to no external conditions except those which might be imposed by pathologists.

This attitude has left its mark on the present report. It asserts that, "Drought, old age, lack of canopy, and poor soils have nothing whatsoever to do with it"—the disease. This is emphatic. But it is not a conclusion derived from experiments with swollen shoot of cacao. The genetically unstandardized condition of the cacao trees in West Africa would invalidate any experimental evidence that might be available on the question. None, in fact, is adduced by the committee.

In a word, there is no reason for supposing that the committee has discovered an exception to the biological law that external (as well as internal or genetic) conditions affect to a greater or less degree the incidence of every type of pest and disease of plants and animals.

On the other hand, the genetic variable is of the first importance, and this is recognized by the committee. It points out that "the development of varieties resistant to infection would be an ideal solution of the problem". But it omits this fundamental principle from its conclusions; and it has postponed the application by a dangerous misstatement: "It takes *many* years to obtain seed from a single crossing of two parents". In fact, it takes *two* years; and this time could be reduced by experimental improvement of growing conditions. Hence, the committee concludes, "This is a long term problem without any immediate practical significance". This is again emphatic. But what does it mean? It means that in prosperity plant breeding had no immediate significance. In disaster it has no practical significance. Thus in cocoa as in all other British tropical crops (except cotton) the ideal solution, the fundamental solution, has still to be treated as a subsidiary matter of no urgency. And there is no plan for attempting it. This is the immediate and practical significance of the report on swollen shoot in cacao.

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