tific library of the Bureau of Science in the Philippines, which "the Japanese wantonly destroyed," suggests that there is an additional way in which libraries damaged by the aggressor nations could and should be repaired. Germany and Japan, to be sure, can not make adequate financial reparation, but their surviving scientific books and journals can replace at least some of those that they have destroyed in allied and "neutral" countries. The control that we now possess over the resources of these two countries should make it feasible to bring about this adjustment. Possibly this reparation in kind is already being arranged—if so, so much the better. If not, let us hope that it will be urged upon the allied governments by appropriate scientific organizations.

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OSTEOPATHY AND UNIVERSITY PRESIDENTS

To the Signatories to the Petition to President Truman for the Deferment of College Science Students

DEAR SIRS:

IN the November 16th issue of SCIENCE appears a letter signed by you and seven other educators appealing to President Truman to alter certain current Selective Service practices. Although I am in hearty sympathy with the motives which undoubtedly prompted your action, I am, nevertheless, impelled to protest vigorously about one element in your statement which casts a serious shadow over the entire document.

In the second paragraph you mention, as deserving deferment, students of "osteopathy" in the same general category with students in such recognized disciplines as medicine, dentistry, pharmacy and engineering. To do so stultifies the entire argument because no American university recognizes osteopathy as a scientifically based healing art, and there is no reason to believe that the biological science faculties of the institutions you represent consider the system of osteopathy to be other than a fraud upon a gullible public. The stupidity or cupidity of some Selective Service official in originally classing students of osteopathy with the others you have listed in granting deferment several years ago is not an adequate excuse for responsible officials of respectable institutions of learning to compound the error now. When university and college heads plead for special consideration for students in the various cults of this type our academic standards and ethics have fallen to a new low. Since when has expediency superseded principle in academic practice? And since when have the institutions you represent and administer given their academic blessing to medical cultism? If those universities, including California, Cincinnati, Cornell, New York University, Vanderbilt and Yale, among others, intend to promote osteopathy it is certainly time for American medical and other biological scientists to take stock of their position.

Actually I am confident that the unfortunate implications of the naming of osteopathic students in this way were not apparent to most of you. Nevertheless, the seriousness of its occurrence, even by inadvertence if such it was, can not be over-emphasized in a world in which the layman looks to science for miracles and can not distinguish between scientific fancy and fact because of an inadequacy of background information which, right or wrong, he looks to persons like yourselves to possess and use.

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SCIENTIFIC BOOKS

THE NATURE OF SPECIES

Experimental Studies on the Nature of Species. II. Plant Evolution through Amphiploidy and Autoploidy with Examples from the Madiinae. By JENS CLAUSEN, DAVID D. KECK and WILLIAM M. HIESEY. Carnegie Institution of Washington Publ. 564. vii + 174 pp. 86 figs. 1945. Paper, $1.25, cloth, $2.00.

THE purpose of this valuable and stimulating contribution is the classification of polyploids in terms of the "biosystematic" categories already made familiar by the same authors: the ecotype, the ecospecies and the cenospecies. The origin and characteristics of three synthesized amphiploids are first described, one of which is found in nature. Then follows an examination of other polyploids and the classification proposed. This in turn is followed by discussions of the ecological characteristics of both autoploids and amphiploids and the course of evolution when polyploidy is involved.

The authors "propose to limit autoploidy to the multiplication of genomes within the limits of one ecospecies. By this definition, *autoploidy* applies to cases ranging from the homozygous individual with multiplied chromosome number, at one extreme, to the polyploid derivatives of a hybrid between subspecies or ecotypes of a species of the other. *Amphiploidy,* in contrast with autoploidy, is the addition of all the

chromosomes of distinct species. In the strictest sense of the term amphiploidy, these species belong to distinct cenospecies. In the widest sense, amphiploidy would also include the addition of the chromosomes of ecospecies of one cenospecies, but these cases are transitional in character and generally less stable."

Ecospecies are defined as "species capable of a limited interchange of genes with one another," cenospecies as "species entirely unable to exchange genes with one another." This ability of species to exchange genes is considered by the authors to rest upon "constitutional barriers" which are "based in the genic structure." Hence the proposed classification rests primarily upon the degree of interfertility of the parents. The result is a bimodal segregation of the known examples, each mode being characterized by differences in cytological behavior, fertility and morphology and, in nature, by different spatial and ecological relationships to the parents.

The weakness of the proposed classification lies in the fact that ecospecies and cenospecies are defined by sterility barriers without reference to other isolating mechanisms. But the existence in nature of such mechanisms may prevent gene interchange despite a relatively high degree of interfertility. Hence it is legitimate to inquire to what extent ecospecies and cenospecies, defined thus, correspond to the realities of nature.

A case in point is furnished by two well-defined diploid species of *Delphinium* (Lewis and Epling, unpubl.) which produce fertile hybrids when crossed in the garden, and backcross readily to both parents. Their interfertility is of the order which would lead to their classification as ecospecies. Here would appear to be a channel sufficient to permit an appreciable exchange of genes between these species. They are sympatric over a large area and in places actually grow together. Their flowering periods overlap. Yet, there is no evidence that even F_1 plants occur in the mixed colonies. These species are seemingly able to exchange genes, but, so far as has been determined, they do not. Hence, the conclusion is difficult to avoid that barriers other than fertility are at play in nature and that rather than being "ecospecies," capable of a limited exchange of genes, as might be indicated by the breeding experiments, they are in fact, "cenospecies," as suggested by the facts of distribution.

Perhaps the question devolves in part upon the term "gene interchange." When fertile hybrids and their recombination products are formed in nature, genes are exchanged (or recombined) in a limited sense. But to become of consequence in speciation such hybrid products would either need to spread and establish themselves, or else they would need to become an actual channel for a flow of genes from one population to the other. Although fertile hybrids may provide such a channel, isolating factors may intervene and prevent a gene flow sufficient to modify either of the parent populations. *Salvia apiana,* for example, is known to hybridize locally with *S. mellifera,* forming local intermediate populations. Yet there is no certain evidence, as yet, that either species has been modified thereby.

Hence, the assumption made that, because fertile recombination products are formed in the frequently observed crosses of *Aquilegia formosa* and *pubescens,* these species are therefore "exchanging" genes and are no more than subspecies, and that the whole genus is a cenospecies, is open to debate. To establish this assumption it would seem necessary to demonstrate an actual absorption of genes from one to another. *A. pubescens,* a member of a section otherwise represented in the Rocky Mountains, is widely separated from its congeners. Its range in the Sierra Nevada is entirely encompassed by that of *A. formosa.* These facts suggest that the species have been in contact for a considerable period. If it can be shown that any appreciable part of *A. pubescens* is more like *A. formosa* than its Rocky Mountain allies, then, it would seem, a basis might exist for demonstrating an actual interchange of genes. So far as known, it maintains its identity. Again, it would appear that although these species are apparently able to exchange genes, they seemingly do not except in the sense of local recombinations.

It may be that, in the final analysis, the barriers are ecological and that these supposed species are in reality only "ecotypes." Nevertheless, before their actual status can be settled, the presence of other than sterility barriers must be reckoned with and the means by which they operate must be ascertained. Hence, the usefulness of the concepts ecotype, ecospecies and cenospecies would seem to be impaired so long as they are defined in terms which are potential, rather than those that are realized in nature. Defined in the latter terms, used for them as alternatives to the concepts subspecies and species might disappear, save in Turesson's original connotation.

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BEETLES IN STORED PRODUCTS

A Monograph of the Beetles Associated with Stored Products. By H. E. HINTON, I. viii + 443 pp. 505 figs. 20 pp. refs. British Museum (Natural History). London. £1-10s.

A WORK representing a series of very important investigations recommended by Professor J. W. Munro