public, but the treatment of a vast and complex subject which approves itself to one thoughtful man can not be expected to satisfy all his readers. If, then, we dwell upon points of disagreement, we are not the less conscious that Sir Frank's paper compares favorably with the lucubrations of most administrators.

In the earlier part of his paper he emphasized the novelty of the departure made by the government in 1915, and, without the assertion in so many words, rather implied that our government has handled the problem of national research with more courage and on more satisfactory lines than did that of the Germans. While we agree that the course followed here since 1915 was the best in the circumstances, we are emphatically of opinion that this is only true in consequence of past errors; that the idea inspiring the memorandum of v. Humboldt, quoted by Sir Frank Heath, is correct, and that the system of the German government was in principle thoroughly sound.

The German ruling caste appreciated the importance of scientific knowledge a century before ours, and conceived that the best way to foster research was to create a number of adequately equipped university departments; they believed that the multiplication of opportunities for disinterested investigation would lead to the production of trained minds capable, in Sir Frank Heath's words, "of extending the powers and capacities of man in relation to the world in which he lives." They had their reward; all that scientific ingenuity and foresight could do to safeguard the Teutonic hegemony was done there was no need of hasty improvisations. The German state system has perished in scenes of death and disaster, but of the many crimes and blunders committed by its makers, the neglect of science is not one. In this country, generations of neglect have compelled us to adopt in our hour of need an expedient which would not have found a single defender if proposed as a normal method of evolution. The courage of the government in 1915, which Sir Frank Heath extols, was the courage of despair; we could not then, we can not now, escape the penalty of a hundred years' sloth. It is too late to build from the ground on the German model, but we need not pretend that we have discovered for ourselves a better model, but should, with humble and contrite hearts, try gradually to improve our temporary structure into something like a real university system, keeping it free from such defects and abuses as in Germany that system revealed in practise; of these the worst was the prostitution of scientific appointments and scholarly reputations to the uses of political propaganda. —*British Medical Journal.*

SCIENTIFIC BOOKS

Bastardierung als Ursache der Apogamie im Pflanzenreich. Eine Hypothese zur experimenteller Vererbungs- und Abstammungslehre. By ALFRED ERNST, professor of botany in Zürich. Jena, Fischer. 1918. Pp. 650, with 172 figures and 2 plates.

The ultimate practical aim of the theory of mutation is avowedly to discover the means of producing new qualities in plants and animals at will and in arbitrarily chosen directions. Some investigators assume that one of the chief causes of mutation is to be looked for in crossing, whereas others think that crosses are far too rare in nature to have had any appreciable effect in the production of species, except for the polymorphous genera. Obviously the best way to decide between these two opinions is to study the influence of hybridizing on the origin of a new character. The author of this book has attacked this problem from a special side, proposing to try to induce a definite character, viz., apogamy, or the production of seeds and spores without fecundation, by means of artificial crosses. The book does not bring any new results, but a collection and discussion of the facts, available for the choice of the material and the method of experimentation to be used.

From this point of view it may be commended to the student of rich questions. It gives a full description of all known cases of apogamy, including algæ and fungi on one hand, *Marsilia, Antennaria, Alchemilla* and

Hieracium on the other. The doubling of chromosomes, the terminology of parthenogenesis, the nucellar embryos, the lessened fertility and many other effects of hybridizing, as well as those of vegetative propagation are extensively dealt with. From this survey the author concludes that *Chara crinita* seems to afford the best material for further studies and gives an ample review of the mode of propagation of this algæ.

It is a dioecious plant, which has a parthenogenetic variety. The latter has been described by *Alexander Braun* as early as 1856 and since by numerous authors. The species is rather rare; in some stations it is found without the variety but in the larger number of localities only the apogamous form occurs. In some, however, both grow together, indicating the possibility of a repeated origin of the variety from the dioecious type. Moreover it is shown that the differences between the two types are of such a kind, that they can not have originated slowly and gradually but must be assumed to be due to a sudden change (p. 104). This is the well-known way in which in other cases mutations are seen to arise. The probable difficulties of the intended investigation are then amply discussed. To these the reviewer might add the objection that it is a species which has already produced an apogamous form, and probably more than once and which therefore may be expected to repeat the mutation from time to time, even without the aid of experimental interference. Furthermore, the experience with the evening primrose has shown that mutations occur in crossed progeny as well as in pure lines and the research of Baur on *Antirhinum* and of Morgan on *Drosophila* have amply confirmed this result. Among hybrid progenies they seem to be more numerous, but only in consequence of the fact that such cultures usually embrace many thousands of individuals more than are kept in the pure stocks. The same will be the case in the cultures of *chara crinita* and the expected occurrence of apogamous mutations in hybrid families can, therefore, not be regarded as a proof of their origin by means of hybridization.

But it seems highly desirable that the experimental trials should be made, the more while in any case the gain for the theory of mutation must be expected to be of the highest importance.

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PRELIMINARY REPORT OF EXPERIMENTS ON THE ACTION OF DICHLOROETHYLSULFIDE (MUSTARD GAS) ON THE CELLS OF MARINE ORGANISMS[1]

The toxic action of a sample of "mustard gas" sent us by Major H. C. Bradley, of the Chemical Warfare Service, has been investigated on a number of typical marine organisms, including various swimming larvæ (sea-urchin, starfish, squid, the annelids *Nereis* and *Arenicola*), the developing eggs of sea-urchin and star-fish, the spermatozoa of sea-urchin and starfish, and young and adult fish (*Fundulus*). The most satisfactory objects for experimentation have proved to be the developing eggs of the starfish (*Asterias forbesii*), and most of our work has been carried out with this material. Changes in the rate and character of cleavage in the eggs after treatment with "mustard," the production of abnormalities of form and structure in the larvæ, and the degree of ciliary activity, furnish a very delicate index of toxic action. Valuable information has also been obtained with *Arenicola* larvæ and with small fish (*Fundulus*).

In the experiments with fertilized starfish eggs we have investigated the influence of solutions of the "mustard gas" in sea-water upon the cleavage and early development (up to the gastrula stage). The procedure chiefly employed was as follows: A small quantity of the "mustard gas" (ca. 5 grams) was shaken vigorously with one liter of sea-water in a

[1] This preliminary report in its present form was sent to the Medical Section, Chemical Warfare Service, September, 1918. A more detailed account of these experiments will be published in the near future.