sistant agent of the Bondsville Bleachery Company of Bondsville, Massachusetts, has accepted the position of head of the department of textile engineering in the Texas Technological College.

DR. JOHANNES NÖRR, professor of pathology and physiology at the University at Giessen, has been called to Munich.

FATHER FREDERICK W. SOHON has succeeded the late Father Francis A. Tondorf as director of the seismic station of Georgetown University.

DISCUSSION

RECENT DISCUSSIONS OF THE REDUCTION DIVISION IN DROSOPHILA MELANOGASTER

IN his well-known monograph on "The Genetics of Drosophila," published in Bibliographia Genetica, Vol. II, 1925, Morgan makes a statement that little or nothing is known of the reduction divisions in *Drosophila melanogaster*. Later in the same year, in collaboration with Professor G. C. Hicks, of the University of Buffalo, the present author described the meiotic or maturation phenomena in *D. melanogaster.* It was there pointed out that the absence of any adequate description of the maturation phenomena in *D. melanogaster* was deplorable in view of the heavy weight of biological theory that has been put upon its chromosomes. The results of Professor Hicks and the present author made it clear that the reduction divisions in this species are entirely abnormal and present a marked and apparently significant resemblance to the reduction divisions present in known hybrids. This general situation was correlated by us with the great variability of *D. melanogaster,* and it was emphasized that, both on account of variability and its unusual meiotic phenomena, *D. melanogaster* was to be regarded as a natural hybrid.

A significant silence has prevailed on this subject of Drosophila, until recently. In an excellent and beautifully illustrated volume[1] Belar has put forth the claim that the reduction divisions in *D. melanogaster* are normal, and is of the opinion that certain photomicrographs which he presents prove the correctness of this statement. In his Plate II, an actual photographic reproduction of his photomicrographs, in figures 5a, 5b and 5c, he shows the first division (reduction division) of the spermatocyte from the same nucleus in three different focal planes. In all these three figures, representing different foci, at least one chromosome of those appearing is far removed from the equatorial line. This situation corresponds exactly with the description furnished by Hicks and Jeffrey. It is obvious from Belar's own illustrations that there is no normal equatorial plate in the reduction division of *D. melanogaster.* Belar criticizes the use of Carnoy's fluid for preservation of the material. This criticism appears captious inasmuch as the author himself describes his own use of the same preservative in such difficult cases as fixing divisions in Nematodes. Further, Morgan himself has made use of Carnoy's solution in his important studies of the chromosomes in Aphids. It may be further added that Carnoy's solution is universally recognized by cytologists as a reliable reagent for the fixation of chromosomes. Belar's conclusions are all the more remarkable because he considers some of the structures seen in the nucleus and protoplasm in *D. melanogaster* to be abnormal vestiges of mitichondria produced by the reagents. This is all the more surprising because he claims to have obtained well-preserved mitichondria in his own preparations by the use of Flemming's fluid. It is surely universally known that Flemming's fluid will not preserve mitichondria. It accordingly is difficult to attach any great importance to Belar's criticisms, which seem to rest more on prejudice than a critical examination of the facts.

In the interval since the first paper on *D. melanogaster* we have examined a large amount of material of this species from various sources and from the wild, and have used chrom-osmioacetic mixture as well as Carnoy's preservative. The only difference that we have found between the former and the latter is that Carnoy's solution gave very much better preparations, showing a better fixation of all the tissues than did Flemming's solution. The situation in regard to the reduction division appeared in the addition material substantially the same as in our original description, and we accordingly stand by the results there obtained. It may be added that a very wide use of preservatives in connection with a range of studies covering both plants and animals has quite confirmed in our mind the approval of Carnoy's solution as a nuclear fixative which is in general voiced by cytologists.

Less excusable than Belar's criticisms are those made privately by certain American geneticists who affect to believe that the reduction divisions in *D. melanogaster* are perfectly normal and have always been known to be so. The most charitable view that one can take of such statements is that the authors are unfamiliar with Morgan's monograph on the

[1] "Die cytologischen Grundlagen der Vererbung," Berlin, Gebrüder Borntraeger, 1928.

species. It is true that the somatic divisions of *D. melanogaster* are quite normal, but that in general is true of all somatic divisions of all hybrids, as has been recently emphasized by the present author. This confusion of thought is probably the cause of a highly critical paper published recently in this journal by Huskins.[2] He expresses the view that a contrast in regularity between the somatic and reduction division is of no importance from the standpoint of diagnosis of hybrids. This opinion seems to have remarkably little justification. We are now acquainted with the remarkable meiotic or reduction phenomena in many hybrids, both plants and animals, and an outstanding feature of the maturation metaphase is, in the great majority of cases, the lagging of chromosomes, polypolidy, sterility and extreme variability. It is true there are certain hybrids which are fixed invariables from the first, and it is also true that there are forms of heterozygotes which present little or no sterility. This does not alter the fact, however, that sterility, variability and abnormal cytological phenomena are outstanding features of the reduction divisions in known hybrids.

We are now well acquainted with many large orders occurring in all parts of the world and including practically all groups of the higher plants in which all the criteria of hybridism are presented. Excellent examples of these are the genera Rosa, Rubus, Crataegus (in the northern hemisphere), and Eucalyptus, Acacia and Veronica (in Australasia), which manifest extreme variability, intergradation of species, polypolidy and the cytological phenomena of known hybrids.

It will probably become more and more apparent as a result of the increasing correlation of cytological and experimental work in biology that specific change more than anything else is the result of the crossing of species, in other words, of hybridization. Purely experimental work divorced from morphology seems to have as little future as the proverbial faith without works. The dangers of this way are well illustrated by the deplorable case of Paul Kammerer, whose experiments purporting to prove the production by experimental means of heritable characteristics were demonstrated by unprejudiced investigators to be based on fraud or extreme credulity or both.

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THE DECOMPOSITION OF OZONE IN THE PRESENCE OF NITROGEN PENTOXIDE

THE rate of the catalytic decomposition of ozone in the presence of nitrogen pentoxide has been shown by Schumacher and Sprenger[1] to follow the expression

$$-\frac{d[O_3]}{dt} = k_1[N_2O_5] + k[N_2O_5]^{2/3}\,[O_3]^{2/3}$$

where the first term on the right-hand side of the equation gives the rate at which ozone is used in reoxidizing the nitrogen dioxide formed from the decomposition of nitrogen pentoxide, k_1 being the specific reaction rate constant for the first order decomposition of nitrogen pentoxide. When an appreciable amount of ozone is present the first term is small compared with the second and the expression can be written in the simplified form

$$-\frac{d[O_3]}{dt} = k[N_2O_5]^{2/3}\,[O_3]^{2/3}.$$

Similar results on the rate of this decomposition will be found in a thesis which I presented to the California Institute of Technology in May, 1928. Experiments were carried out at 25° C. and 35° C. The partial pressure of the nitrogen pentoxide was varied in the ratio 1:30. The maximum partial pressure of ozone was forty millimeters mercury. Two pyrex reaction vessels were used, one of 20 cc volume and the other 300 cc volume. The pressure was followed by means of a click gauge, so the gases were in contact with only glass during the decomposition. Most of the experiments were carried out with a large excess of oxygen, the total pressure approximating one atmosphere, but in three experiments the pressure was reduced to one half atmosphere and in one experiment to one quarter atmosphere. All partial pressures were determined from direct pressure measurements, the start of the nitrogen pentoxide decomposition being marked by a "kink" in the pressure increase time curve, and also by the appearance of a brown color. Tank oxygen was used in all experiments.

In my work the order of the reaction was found to be two thirds with respect to the nitrogen pentoxide pressure, and slightly lower than first with respect to the ozone pressure. The ozone pressure was not varied over a sufficiently wide range to determine exactly the order with respect to the ozone.

The data have been fitted to the simplified expression given by Schumacher and Sprenger and constants calculated. These constants were compared with those calculated to 25° C. and 35° C. from the work of the above experimenters. An agreement within 10 per cent. was obtained.

The work was carried out at the suggestion of Professor R. C. Tolman, and the present note is

[2] SCIENCE, n. s., Vol. 69, No. 1789.

[1] Schumacher and Sprenger, *Zeit. für physikalische Chem.*, 140: 267, 1929.