servation can be usefully undertaken with these conditions in view.

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WAS MAGENDIE THE FIRST STUDENT OF VITAMINS?

FEW investigators have had the rare fortune to make two classical discoveries in one series of investigations. To Magendie must be ascribed such a feat. In a report entitled the "Nutritive Properties of Substances which Contain Nitrogen," which was published in 1816,[1] Magendie showed that dogs can not live upon fats, sugar and water but must have some form of nitrogen in their food. The importance of this discovery was such that it seems to have eclipsed the remainder of the report.

Magendie fed his dogs upon a diet of sugar and water. They lost weight steadily. The remainder of the experiment can not be expressed more concisely than in the author's own words:

> In the third week his [the dog's] thinness increased, his strength diminished, the animal lost his liveliness, his appetite was not so keen. At this same period a small ulceration was developing in the center of the transparent cornea, first on one eye and then on the other, it increased rapidly and at the end of a few days was more than a ''ligne'' [two millimeters] in diameter. Its depth increased in the same proportion; soon the cornea was entirely pierced and the fluid of the eye was flowing out. This singular phenomenon was accompanied by an abundant secretion of the glands of the eyelids.

This animal died. Autopsy showed little other than the effects of inanition. Magendie was a careful investigator. The experiment was repeated two times with identical results. Has any modern investigator presented a clearer picture of xerophthalmia?

In order to test the nutritive value of fats, a diet of olive oil and water was fed another dog. This animal died but showed no xerophthalmia. Another dog fed butter fat and water developed xerophthalmia in one eye! These results are of special interest in the light of modern work, since they are the reverse of modern experiments.

Magendie seems to have realized that he was dealing with a dietary deficiency, since he found normal chyle in his animals at the time of autopsy. His own statement is that "it is thus evident that if these diverse substances [fat, sugar, gum and water] do not nourish, we should not attribute this to the fact that they are not digested."

Magendie noted the marked changes in the urine, feces and bile that resulted from a diet lacking protein and suggested: "Can we not reasonably presume, after the experiments which I have reported, that by diminishing the nitrogenous substances in food we diminish the proportions of materials in the urine which give rise to gall-stones?"

Not only did Magendie record the production of xerophthalmia in animals but he recognized the analogous conditions in man as a result of a restricted diet. He reported this as follows:

> A very interesting experiment has recently been done by an English doctor named Stark. This doctor wishing to estimate the nutritive properties of sugar lived on it exclusively for about one month, but he was then obliged to give up this régime. He had become very feeble and bloated. In his sight appeared livid red spots which seemed to announce the approach of an ulcer. He died a short time after his experiment and the people who knew him thought that he might have been the victim of it.

Magendie closes this classical report with, "I wish that physicians would be inclined to make trials of this kind. Physiology, animal chemistry and medicine can gain from it."

Must Magendie be termed the father of the vitamin hypothesis, as well as the discoverer of the need for protein in the diet?

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[1] *Annales de Chimie et de Physique*, 1 series, 3: 66, 1816.

DROSOPHILA ONCE MORE

IN a recent number of SCIENCE[1] Dr. Bessie B. League publishes a summary of her observations on the reduction divisions in *Drosophila melanogaster*. It is gratifying to find some one on this side of the Atlantic who will admit that the phenomena accompanying the reduction division in this extremely variable species are abnormal. I have found genetical and cytological colleagues most determinedly of the opinion (not, however, expressed by publication) that the meiotic divisions of this species are quite normal, and so much so that it had not been worth while to figure them. I have had to point out that one so presumably well acquainted with the genetics and cytology of Drosophila as Professor Morgan admitted so late as 1925 that little or nothing was known of the reduction division in this species. I find great diversity of explanations offered by genetical colleagues of the peculiar situation which exists in regard to our knowledge of the reduction division in this species. I have already pointed out that although sex chromosomes in practically every other case among animals and plants are recognized and described in the reduction division, in *D. melanogaster*, in striking con-

[1] SCIENCE, 71: 99, January 24, 1930.

trast, in spite of the enormous amount of work on the species, no adequate account of the reduction division has ever been published until very recently. This in itself is a surprising anomaly which the Drosophilists owe it to themselves to explain.

Dr. League's observations, interestingly enough, correspond closely with those recently published by Zuitin in the Genetical Bureau of the University of Leningrad.[2] The article is entitled in the English summary which accompanies the Russian text, "On the Peculiarities of Spermatogenesis in *Drosophila melanogaster*." This author's account confirms the observations made by the present author, and he reaches the conclusion that *D. melanogaster* is an interspecific hybrid, on the basis of its abnormal cytology. The article is accompanied by a number of figures, which in some cases closely parallel those published by the writer in 1925.

It is thus clear, on the evidence of three independent observers, that there are abnormalities connected with the reduction division of *D. melanogaster*. It seems obvious that it is impossible to explain these abnormalities by reference to *D. melanogaster* alone. Obviously the general situation must figure largely in connection with the whole discussion. We know that in the case of many hybrids there are lagging chromosomes and chromosomes extruding into the protoplasm, such as are not found in the parent species of these hybrids. This is particularly true of hybrid plants, because our information in regard to these is very much fuller than it is on the zoological side. The cytological peculiarities of reduction division of *D. melanogaster* exactly parallel those found in hybrid plants. Dr. League explains the lagging chromosomes as resulting from the fragmentation of plasmosomes. This explanation is very difficult to accept in view of the fact that quite normal plasmosomes have been described both by Stevens and the present author.

A very strong point in connection with this discussion is the fact that synapsis has been observed in the reduction division, and has been figured in two of the articles published by the present writer. The synaptic mates which, as is well known, appear in diakinesis and the metaphase of the reduction division in plants and animals are distinctly present and distributed along the spindle in a way which is common for a number of known hybrids, and for some extremely variable species which are probably of hybrid origin. While it may be possible to explain small clumps of apparent chromosomes occurring on the spindle and in the protoplasm in *D. melanogaster* as of plasmosomal origin or even as mitichondria, the synaptic pairs of chromosomes can not possibly be interpreted in this way. Further, fragmentation of chromosomes, as well as ejection into the protoplasm, is not uncommon in hybrids. It is thus extremely probable, in the light of all the facts, that the material lagging on the spindle and ejected into the protoplasm in the reduction division of *D. melanogaster* is of the nature of chromatin, especially as no corresponding structures are present in the somatic divisions. This view gains a high degree of credibility from the fact that we have these fragments of chromatin only in the heterotypic and homotypic divisions, precisely as is the case in known hybrids. This appears to show clearly that they have a peculiar and characteristic relation to meiosis. They parallel, in fact, in their behavior, exactly the conditions found in many known hybrids in plants and animals. It is accordingly assumed that the reduction phenomena in *D. melanogaster*, viewed from the general standpoint of our present knowledge of the cytology of hybrids, indicate clearly the hybrid origin of that species.

If the whole situation is summarized for *D. melanogaster*, it is very apparent that it manifests in a striking degree the peculiarities of an interspecific hybrid. In the first place, the species is heteroploid, showing not only eight and twelve somatic chromosomes but also dysploid modifications of these. This is a recognized heterozygous feature. Further, not only within this much-investigated species does heteroploidy occur, but in others of its large and variable genus, in which the specific enumeration has already reached two hundred. This is a feature of resemblance to other large and mutable genera which manifest all the characteristic features of hybrid contamination. Further, *D. melanogaster* is characterized by a high degree of sterility, another known heterozygous feature. Finally, we have at last the reasonably complete account of the reduction division from three independent, remote sources, which all set in relief the abnormalities in meiosis, which are precisely those found in hybrids. It is true that Dr. League claims that the real chromosomes of the species under discussion behave as in normal species. That is frequently the case in hybrids. Moreover, they do not behave normally in the crucial stage of the metaphase, in which the synaptic mates are present. Further, all recent investigators admit the presence of lagging material on the spindle and in the cytoplasm, precisely as is the case in known hybrids. Zuitin and the present writer claim that this material is chromatin, or at least contains chromatin, while Dr. League maintains the difficult position that it is of plasmosomic origin. If it were, we would expect to find similar material present in the somatic divisions, which is not the case. This consideration apparently eliminates the interpretations of both Dr. League and Belar of the

[2] *Bulletin* of the Bureau of Genetics, Vol. 7, Leningrad, 1929.

lagging and ejected material. Moreover, true plasmosomes are present in the spermatocytes and spermatids of *D. melanogaster*. Finally, the conduct of the lagging and ejected material is precisely that found in many known hybrids.

It is thus clear that *D. melanogaster* has so many of the characteristics of a hybrid that only its duplication by the experimental crossing of recognized species could supply additional evidence in this direction. It is further indicated that it is time that we had surcease of speculations on the part of geneticists as such on the general problems of evolution and the origin of species. No amount of Mendelian moiling with the stabilized variants of a mutable species is likely to throw any permanently valuable light on the question of the origin of species. The real problem is clearly that of the origin of mutability in modern types of plants and animals, a matter upon which the conscientious and even contentious elaboration of the laws of Mendel throws no light whatever. Cytology and the experimental crossing of species are obviously destined to lead the way to new and fundamental advances in our knowledge of the origin of contemporary species of plants and animals.

IN a recent number of this journal[3] Professor Huettner passes some criticisms on my work on *Drosophila melanogaster*. These are largely expressions of the dis-esteem of himself and his group of Drosophilists for my work on this species and includes some quite erroneous statements in regard to my qualifications. As these matters are of little scientific interest, it will be well to refer to the only significant feature of his paper, namely, the question he raises as to the lagging of the chromosomes in the species under discussion. He commends strongly in this connection the use of Feulgen's reagent for the identification of the real chromosomes. Putting aside the question as to the extreme abnormality of the reduction division in *D. melanogaster* as in itself a suspicious circumstance, my critic is referred both to the article cited above and more particularly to a recent paper by Woskressensky and Scheremetjewa on spermatogenesis in *D. melanogaster* published in the *Zeitschrift für Zellforschung und mikroskopische Anatomie*. The latter authors use the method of Feulgen, and their figures show a very large amount of lagging in the chromosomes of the species as diagnosed by this method. Further, their account agrees with that of the present writer in respect to the multiplication in number of the chromosomes beyond that to be expected from the somatic conditions. The tide in regard to the interpretation of *D. melanogaster* has apparently definitely turned, as the newer literature has abandoned the contemptuous attitude which has been in general adopted by Drosophilists on this side of the Atlantic.

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[3] SCIENCE, 71: 241, February 28, 1930.

SCIENTIFIC NAMES IN ZOOLOGY

A VERY considerable number of interested zoologists believe that real progress is being made in zoological nomenclature. A sane use of the highly valuable principle of priority is being made, despite the unwillingness of an ultra-conservative group to adopt any change from the nomenclature it is accustomed to and therefore regards as the proper one, and despite the objections of another group now also being forced into a conservative position by the progress which is being made. This second group would insist on the resurrection of any available name which has priority, no matter how much inconvenience the change would introduce. The International Commission on Zoological Nomenclature is in general steering a median course, by using its plenary power very wisely in eliminating from the rule of priority those occasional names which would clearly create a really wide-spread confusion and instability, and in consistently refusing to pass favorably on cases in which the replacement of the established name affects only a few systematists or in which the desirability of the replacement is clearly debatable or in which the proposed change involves taxonomic judgment rather than the application of the rules.

Those of us who view the progress in nomenclature more or less optimistically are perturbed by the expression of views which can only serve to obstruct the advance which is being made. We feel that such an article as that of Professor Needham[1] is particularly reactionary. Since this account is well and boldly written and since a considerable weight of authority is carried by the author, the remarks of Professor Needham are meeting in some quarters with a reception which we believe to be unjustly favorable. We feel constrained to attempt a reply.

Needham's "smouldering impulse first burst into flame" over some modern names to which he objects. Of course not all names are of equal euphony or brevity, and it is possible to search out some which are flagrant examples of poor style. Zoologists alone are not responsible for such unpleasant productions. They blight the terminology of other sciences. Indeed, some words in common use are not simple.

The discussion of these long names of the day by Dr. Needham gives a very unfair picture. Many modern names are a joy to the zoologist and the classicist alike; many old names are badly constructed and long. But that two names of record length "are far worse than anything pre-Linnaean" is not the truth. The first name quoted belongs to a set which the In-

[1] SCIENCE, 71: 26–28, 1930.