What are the reasons for the present state of affairs? To some extent, I think, the competition of *Biological Abstracts* is responsible. The *Abstracts* covers a large field not dealt with by the *Zoological Record* and in this field is simply invaluable. But its zoological taxonomy appears to me to be not only very incomplete, but also from its manner of publication of comparatively little use. For my own work I find it practically useless. I understand that it was seriously considered that this part of the *Abstracts* might be dropped, and it seems to me that this should be done. As long as it is there, librarians and officials, and even heads of departments, will imagine that it covers the field adequately, and that the *Zoological Record* is unnecessary.

There is, however, a deeper and more important reason for the non-support of the *Record*. It is the lack of interest in taxonomy. For this, I believe, our graduate schools are largely responsible, and perhaps it is not too much to say that in certain respects the graduate school is an enemy of sound science. This is due to the system, not to any particular fault on the part of those who administer it. Consider what we have. A great and increasing number of candidates for the M.A. and Ph.D., together with other less popular degrees. They overrun the departments in the large institutions, and the problem for the professor is to find subjects which these people may study, and on which they can write an acceptable thesis, in one to three years. The actual time available is much less than this statement might suggest because these students have other things to do, and very commonly are employed in the teaching, often handling all the quiz sections and correcting all the examination papers. In this situation, what subjects of research may be profitably chosen? Those which (1) require little previous knowledge, and (2) involve no great breadth of view. It must not be necessary to accumulate a special collection or library, and it is very important that the student should not bother the professor too much. As a typical example, I think of a brilliant girl I know, who was set to cutting off the tails of salamanders, in order to find out whether (under laboratory conditions, certainly not in the wild!) they grew any faster without a tail to support.

I do not mean to say that most of these theses do not possess some value, at least for those doing the work, and it is true that occasionally an important taxonomic monograph, involving many years of study, is accepted for the Ph.D. But broadly speaking neither the spirit nor the methods are those of profound scientific research; and taxonomy, which requires many years (if only that the worker may discover his own mistakes), is out of the question.

What have we left to rely on? We should expect and demand that the scientific departments of the government prepare monographic works on various groups, especially those of economic importance. Some very good work of this type has appeared, but I think not nearly enough. To be concrete, I do not see any valid reason why the Bureau of Entomology, with its really enormous appropriation and abundance of technicians of all sorts, has never given us a monograph of the Coccidae (scale insects and mealy-bugs).

Yet all that the governments can do is not enough, and it would be deplorable if the progress of science depended wholly on governmental agencies. There remains the amateur, the man of the type and spirit of Darwin and Wallace, who loves science and finds in it the means of satisfying the cravings of his mind, intellectual and emotional. It is the amateur who can rejoice in his slowly increasing collection, in the increments of his knowledge. Though he may spend only a few hours a week at his hobby, he becomes a learned man with the passage of time. When there are enough amateurs in a district, they form a society, a fellowship of the disciples.

There is no simple way to attain all these good things. But the first step is to desire them, and if we do that long enough and earnestly enough they will be realized in abundance.

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DECEMBER 20, 1929

RECENT CRITICISMS CONCERNING MEIOSIS IN DROSOPHILA MELANOGASTER

FOR the past few years some rather strongly adverse criticisms have been made by E. C. Jeffrey, in which he claims that the meiotic divisions of *Drosophila melanogaster* are atypical, resembling those of certain species hybrids in plants, and that this fly is therefore also a species hybrid.

Nobody who has worked with *Drosophila melanogaster* has taken Jeffrey seriously in respect to his statements. Being eminent in paleobotany, his venture into a specialized field of animal cytology seems to be a long and daring step. Any one familiar with the elementary laws of genetics can readily perceive the inaccuracy of his assertions. I do not intend to answer him on his latest contribution in SCIENCE of December 13, 1929, but I do feel that the cytologic status of *Drosophila melanogaster* should be briefly submitted to those readers of SCIENCE who are not specially versed in cytology or genetics. Having followed Jeffrey's periodic attacks since 1925, I have no desire to enter into any controversy with him, and my interest in this matter will end with this brief statement. A detailed account of the spermatogenesis of *Drosophila melanogaster* is now ready for the press and will be published in the *Archiv für Zellforschung und mikroskopische Anatomie.*

It may be well advised to discuss first the merits of methods employed. Professor Jeffrey claims that he obtains better preparations with Carnoy's mixture than with Flemming's. It is true that one may obtain fairly good material with Carnoy's, if the duration of fixation is definitely ascertained within the limit of a few minutes. But it is a very tricky fixing agent and for this reason is not as reliable as Flemming's or many other mixtures which are in general use. Flemming's solution is superior to any other fixing agent in its constancy of delicate preservation, and this point of view is held by most cytologists who have worked with *Drosophila melanogaster* (Belar, Bridges, Frolowa, Metz, Stern, etc.). Carnoy's may be an excellent fixative in plant cytology, but it certainly has a limited use in animal cytology. It must also be pointed out that a fixing solution which is good for one species is not necessarily the same for others, and Jeffrey's criticism that Belar used Carnoy's for Nematodes and avoided it for Drosophila demonstrates his inexperience in the methods of cytologic technique. I also must take exception to the assertion that Flemming's fluid does not preserve mitochondria. It was this very fixative which gave rise to the investigation of mitochondria, and our modern work in this phase of cytology is accomplished chiefly by a modification of Flemming's fluid from which the larger part of acetic acid has been removed.

Several years ago (1924) I described the maturation phenomena in the egg of *Drosophila melanogaster.* The first meiotic spindle is present when the spermatozoa enter. Within twenty minutes the polar bodies are formed, and the maturation divisions are so plainly visible in this large egg that I have used such preparations for classroom demonstrations. If Jeffrey's assertions were true and *Drosophila melanogaster* were a species hybrid, the atypical condition in meiosis should show itself most definitely in the meiosis of the egg and not be confined to the spermatogenesis alone.

To consider *melanogaster* a hybrid would necessitate the same view for other Drosophilae, certainly *obscura, virilis, Willistoni* and *funebris,* since their cytology is of the same type as that of *melanogaster,* and also that they give rise to many mutants. Many geneticists have tried to cross closely related Drosophila species with *melanogaster* without success (Morgan, Metz, Sturtevant, etc.). I spent the greater part of one year in such experimentation, going so far as to inseminate the unfertilized egg by means of a micropipette, without being able to secure normal development. All such eggs degenerated in a short time. The interspecific sterility in all Drosophilae is so pronounced that the possibility of ever obtaining viable species-hybrids has been abandoned by most geneticists. If Jeffrey's comparison of *Drosophila melanogaster* to species-hybrids of certain plants were true, where are the two possible species from which *melanogaster* could have been derived? Why is it impossible to cross *melanogaster* with *Willistoni* or *caribbea,* two species which are morphologically and cytologically so much like *melanogaster* that only an expert can tell them apart?

In 1926 Metz published an account of the spermatogenesis of *Drosophila virilis,* including his observations on *melanogaster* and several other species. Though I am inclined to disagree with him in minor details, in the main his figures are correct and are in line with other observers. He showed that meiosis takes place in a normal, typical manner. There is also no doubt about the correctness of Belar's interpretation of the primary spermatocyte. However, it is absurd for Jeffrey to take Belar's figures 5a, 5b, and 5c on plate II of his "Die cytologischen Grundlagen der Vererbung" for the purpose of substantiating his assumptions. These figures represent three aspects of one cell in the first meiotic division, and Jeffrey makes a strong point of the fact that one chromosome is situated on one side, behind the others. Having examined hundreds of such divisions, I have observed this chromosome many times. In 1909 it was identified by Miss Stevens as the XY combination. I have traced it from diakinesis through the first meiosis to the secondary spermatocytes and found it to lag during the entire process. Lagging sex chromosomes are so common in spermatogenesis that such behavior is the rule rather than the exception. It is due to this lagging that Bridges explained nondisjunction which occasionally occurs in *Drosophila melanogaster.*

In addition to the XY complex, the primary spermatocyte contains three other bivalents. Two of these can be readily recognized, but the third consists of chromosomes so small as to resemble a small dot. To make an issue over a chromosomal arrangement in an "equatorial plane" with lagging sex chromosomes and only two fairly large bivalents is to overreach oneself in unimportant detail. But in spite of Jeffrey's assertions to the contrary, the bivalents do come to the approximate center of the first spermatocyte spindle and separate there into univalents. Belar's photograph 5a shows this condition splendidly. The division of the secondary spermatocyte takes place after an interkinesis. The univalents are there separated into four chromatids, so definite in outline that their identity and number can be ascertained without question.

Jeffrey's divergent results from those of the large number of cytologists who have worked on *Drosophila melanogaster* must be ascribed to his inexperience in Drosophila technique. Judging from his figures, published in previous papers, he apparently mistakes certain cell inclusions which stain black with iron hematoxylin for chromosomes. This difficulty can be overcome when the Feulgen nuclear reaction, preceded by formol-alcohol-acetic acid fixation, is applied.

I wish to repeat that this account is not written in a controversial spirit, but is merely given as a point of information. I also fully realize that the geneticists could disprove Jeffrey's assumptions even better and more effectively than a cytologist, but I doubt whether any one of them would take the time to do so.

Since this article has been written, Guyénot and Naville have published a most thorough account of the spermatogenesis in *Drosophila melanogaster* in *La Cellule,* Vol. xxxix, No. 1, 1929. They also repeated my investigations on maturation divisions of the egg in which they agree entirely with my work. Their criticism of Jeffrey's work is almost a duplicate of mine given above.

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THE "FERTILIZATION" MEMBRANE OF ECHINID OVA

IN SCIENCE[1] for October 11, 1929, Professor A. R. Moore contributes a note on "The Function of the Fertilization Membrane in the Development of the Larva of the Sea-urchin." Against his conclusion I wish to enter a protest.

I had supposed that even beginners in marine embryology realized that the "fertilization"—vitelline—membrane of echinid ova plays no rôle in development after its complete separation from the vitellus. Every such student knows that by centrifuging, pressure, and the like, uninseminated ova of the sea-urchin are easily deformed—can, for example, be pulled out into long strands—with a return to normal form. This is due to the elasticity of the closely adherent vitelline membrane which encloses the almost watery egg contents and which plays a rôle in the metabolism of the egg. After insemination not only does the membrane stand off from the egg; it becomes stiff, brittle and easily removable; it has changed chemically, as Harvey has shown, and it plays no part in the metabolism of the egg. Removal of the membrane (except by micro-dissection?) from the uninseminated egg is practically impossible. Its removal after insemination has been frequently accomplished and this without injury to the egg or impairment of development. Finally, every student of the living sea-urchin egg has doubtless observed its "hatching," *i.e.,* the escape of the swimming form through the ruptured membrane. What justification, then, has Moore for the conclusion in his note concerning the function of the vitelline membrane in development?

[1] 70: 360.

Perhaps Moore did not mean the vitelline ("fertilization") membrane. In that case he should have given his note a different title. If, on the other hand, he meant the hyaline plasma layer the statement in his conclusion is superfluous; here again, every student knows that the hyaline plasma layer is part of the developing egg.

In his experiments, Moore finds that after exposure to an isosmotic solution of urea (he does not give the pH of the solution) uninseminated eggs are capable of fertilization and development without the "formation" of either the "fertilization" or hyaline membrane. Obviously, this might mean simply that the preformed cortex which during and after membrane separation builds up the hyaline plasma layer is so injured by urea that the normal cortical changes underlying the separation of the vitelline membrane are abnormal. The result would then be not the failure to "form" hyaline plasma layer but the rapid disintegration of this layer after it "forms."

If Moore's interpretation of his experiment on the effect of urea—namely, that it inhibits formation of the hyaline plasma layer—be correct, then he has been most unfortunate both in the choice of his title and in the statement of his observations.

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PEDOLOGY OR SOIL SCIENCE

IN reply to the comments of Dr. William A. Hamor in the January 17, 1930, issue of SCIENCE relative to the use of the term *pedology* to refer to soil science, attention should be called to the fact that pedology was first used by the Russian soil scientists in 1865, over thirty years before the child scientists adopted it. The latter, as Dr. Hamor notes, are using an incorrect spelling of the word. The term they should employ is *paedology* or *paidology*. In view of the prior use of pedology to refer to soil science and as the psychologists are using the word incorrectly and also because of the general acceptance and use of the term in Europe in place of soil science, the American Soil Survey Association at its annual meet-