lishing the identity of the centrioles seen in the sperm-forming divisions with the similar ones found in other types of mitosis, including the maturation-division of the egg. This comparison has recently been challenged with the contention that in the sperm-forming divisions the centriole is only secondarily associated with the astral centers, and that it is primarily a blepharoplast for the production of the axial filament of the sperm-tail. A crucial test for this interpretation is offered by *Ascaris megalocephala,* where the mature sperm shows neither tail nor axial filament. It was with this point in mind that the late Professor Robert H. Bowen suggested a reinvestigation of the facts in that animal.

My preparations of the sperm-forming divisions in *Ascaris megalocephala* make it perfectly plain that in all essentials the behavior of the centrioles conforms exactly to the classical scheme. There can be no doubt of the fact, uniformly seen in large numbers of cells, that in the first metaphase the centrioles divide at a time when neither the surrounding centrosome nor the aster show any indication of division. In the early anaphase the centrosome becomes ellipsoidal and divides, each daughter centrosome containing a centriole. It is toward the end of the telophase stage that these daughter centrosomes, each with its centriole, begin their movement to opposite sides of the cell, never losing their identity from this stage to the prophase of the next division.

These facts can easily be demonstrated and with perfect clearness. Since in this animal the sperm has no flagellum or axial filament there is no ground for considering the centrioles as blepharoplasts or as differing in any material way from those seen in the mitosis of other kinds of cells.

These results, confirming in all essentials those of the early cytologists, are to be reported in detail hereafter.

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MORE ABOUT SHIPWORMS

ON my return from the West Indies my attention was called to Dr. W. T. Calman's article, "The Taxonomic Outlook in Zoology," being the address of the president of Section D—Zoology, of the British Association for the Advancement of Science, published in SCIENCE, September 19, 1930. On page 281, Dr. Calman uses the sentence:

> Dr. Bartsch, of Washington, in his "Monograph of the American Shipworms" (1922), simplified his task by the assumption that any species found on the coasts of the American continent must, of necessity, be different from any found elsewhere, and he was thus able to write "n. sp." after twenty-two out of the twenty-nine specific names. It was soon shown, however, by other American zoologists that this assumption was without foundation, and that the most destructive species on both the Atlantic and Pacific coasts of North America was the European *Teredo navalis.*

Personally this note has called for no resentment on my part, but only provoked a smile; yet some of my best friends insist that it requires a "retort courteous," lest it be deemed that silence on my part be consent.

European shipworms, unlike men, are apparently as clannish as American shipworms. American shipworms have been carried by the Gulf Stream from the West Indies to European shores since the Gulf Stream came into existence, or, probably still better, since trees grew and were set adrift in the West Indies by various agencies. These trees have always fallen a prey to shipworm attacks, and American shipworms in such floating timbers have thus paid visits to European shores since time immemorial.

Jeffreys, who has done more work upon European shipworms than any other man, pointed out long ago that in spite of the constant immigrations from American waters none of these sea waifs had succeeded in establishing themselves in European waters. By a study of the extensive Jeffreys collection, which rests in the United States National Museum, I am able to confirm his conclusions. The environmental factors of the two regions are evidently sufficiently delimiting to prevent such colonization.

Why some American authors, and my critic, persist in claiming that European shipworms are less choice in selecting a habitat than the American forms has always been a puzzle to me, and seems explainable only on three grounds: (1) European man has found America good; why shouldn't shipworms? (2) Because we love to cling to ancient concepts and are loath to change them. In the days gone by, due to little comparative material much sloppy work was done in determining shipworms and many names belonging to European species were hastily and wrongly applied to American forms. (3) The game of playing politics in science has recently crept in, it being the belief of some naturalist that if we had only one shipworm, *Teredo navalis,* in all the waters of the world, we could get a better or easier hearing for the forming of an international attack upon this animal. Personally, I do not see that it makes any difference whether there is one species or a thousand species of shipworms. Shipworms, except where cultivated for food, as in Siam, are like the Indians of old, all bad, and undesirable.

I find upon careful study based upon a huge amount of material that shipworms are well-behaved mollusks following the laws of distribution that dominate the other bivalves, and I see no reason or justifi-

cation to change any of the views expressed in Bulletin 122 of the United States National Museum. The mass of material that has come to me since that paper was published is all confirmatory of the views expressed there. I fear, therefore, that Dr. Calman's arrow will prove a boomerang that is bound to return to the sender.

Incidentally I wish to refer the reader to two previous notes of mine published in SCIENCE, bearing upon this same subject: One, "The Status of Teredo beachi and Teredo navalis," a paper which appeared in SCIENCE for June 15, 1923, page 692, and evidently overlooked by Dr. Calman, in which I called attention to the fact that these two shipworms were not only not synonymous but did not even belong to the same sections of the subgenus *Teredo*. I gave in that paper the characters that separate them, so they do not need to be repeated here.

Again, "Stenomorph, a New Term in Taxonomy," published in SCIENCE, Volume 57, March 16, 1923, on page 330.

Quoting again from Dr. Calman:

> Nevertheless, the taxonomy of the group remains in a state of the utmost confusion. There is no agreement as to the limits even of the genera, and the inconstancy of the characters that have been used for the definition of species is plain to any one who studies a large collection.

This cry is not an unusual one. We hear it expressed by specialists in all branches of natural history. Usually it means that the individual thus afflicted has for want of time or inclination failed to go to the bottom of things in his study, and expresses his weariness by saying that things are in "utmost confusion." I have yet to find a species that can not readily be placed in the proper subgeneric group in the classification that I submitted in Bulletin 122, or my subsequent paper, "The Shipworms of the Philippine Islands," Bulletin 100, Volume 2, Part 5, U. S. National Museum, 1927, excepting such cases where new groups that were unknown at the time of the publication of these papers are involved. While I do not claim infallibility, I nevertheless believe that this classification is based upon a sound foundation—shell characters.

The shell, in spite of what some soft anatomists would preach, is the soundest single element that one can use in the classification of mollusca. It is comparable, as far as its value for classificatory purposes is concerned, to the skeleton of mammals, birds, reptiles, batrachians and fishes, that is, the vertebrates. I believe that no one will challenge the use of the skeleton of vertebrates for that purpose, and in mollusks this use is even less assailable, for in the molluscan skeleton, unlike that of the vertebrate, we have the story of the entire ontogeny of the animal engraved upon its skeleton. We are therefore able to see, on a perfect specimen of a shell, the sculptural characters that were impressed upon the parts that develop while the animal was still in the egg or the uterus of the parent, as well as the subsequent additions of characters acquired during the various phases intervening between the egg and senescence. In no other group that I am acquainted with—vertebrate or invertebrate—do we find such a perfect complete story of the life history engraved upon any part of the anatomy of an animal. In the other groups each life stage has its features which are modified or eliminated by subsequent development, while in the molluscan shell we have simply a series of additions with perfect preservation of the preceding stage or stages. I maintain, therefore, that the shell is of prime importance in the classification of mollusca. I may here also add that the geological record is based upon this element.

Likewise do I wish to call attention to another very interesting fact brought out by my Cerion breeding experiments, where we found that crossing *Cerion viaregis* with *Cerion incanum* produced an endless number of mutations in the F^2 generation. The shells of these animals, while they present innumerable changes in sculpture and coloration, would nevertheless be recognized as Cerions by any amateur. Not so the anatomy! The dissection of one hundred of these hybrids has brought to light such changes in the organization of the soft parts that had we soft parts only and no shells to check against, different family or even higher rank might have to be assigned to some of these mutations. The same story was expressed by the dissection of one hundred individuals representing a native wild cross of *Cerion peracuta* and *Cerion tridentata*. These facts will be fully presented in a paper which is almost completed. It is sufficient to simply state here that the facts adduced from this Cerion breeding point strongly to the conclusion that the soft anatomy of mollusks is less stable than the shell. This, combined with the fact that the soft anatomy of animals presents at any one time only the particular age or functional stage of that animal, while the shell has engraved upon it all its history up to the time of its demise. A study of the cytology, embryology metamorphosis, as well as that of the adult characters of a shipworm should, and I hope will, give confirmatory evidence for what I claim as facts presented by the shell.

The classification offered in Bulletin 122 was the first attempt in trying to bring up to date the classification of a group of mollusks that had for some time been seriously neglected. New groups since discovered will require its expansion. Here, as in every

other group we must look for further modifications, as our knowledge of the subject increases, but I am convinced that the basis upon which it is founded is sound.

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THE RUSSIAN ACADEMY OF SCIENCES

Homo hominis lupus est.—Old proverb.

The President of the Russian Academy of Sciences, Dr. A. P. Karpinsky, the distinguished geologist, is leaving his post at the academy. This decision is the outcome of his unsuccessful protests against the recent forced decision of the academy to deprive of its membership four academicians, including such historians as S. F. Platonov and E. V. Tarle, whose scientific views have been pronounced by the authorities to be incompatible with their presence in the academy of a communistic state. It is noteworthy that at the same meeting of the academy several foreign scientific workers were elected as foreign members. It appears clear, in the circumstances, that the acceptance of membership of the Academy of U. S. S. R. must involve silent agreement with the basic principle underlying the attitude of the Soviet authorities toward science.—*Nature, March 7, 1931, p. 346.*

I VISITED Russia, Siberia and Russian Turkestan (Usbekistan) in 1927, and gave, in *Nature* of November 19 of that year, a brief account of the biological work as I observed it in the U. S. S. R. I was greatly impressed by the volume and variety of the work done, and the fact that all the scientific men I met were industriously cooperating to increase knowledge and education throughout the country. Even at that time it was declared that the professors holding over from pre-revolutionary times would be replaced by "Red" professors as soon as practicable; but although this appeared ominous, I hoped that the actual results would not be definitely unfavorable to science. My more or less optimistic view resulted from acquaintance with a number of young men and women in course of training in the universities, and presumably destined to do the research and academic teaching of the not distant future. They appeared to be on the whole sensible, enthusiastic young people, whose contacts had been broad enough to free them from excessive political dogmatism. I hoped that they would continue to be governed by the true spirit of science, and saw in them the best hope for the Russia of to-morrow.

The Academy of Sciences at Leningrad, combining the functions of the Royal Society and the British Museum, has been the great intellectual center of the country. In its museum are preserved innumerable scientific treasures, excellently arranged. During the early days of the revolution it took all the efforts of Dr. Karpinsky and his daughter to prevent irreparable damage. As it was, a few bullets came through the windows, but no serious injury was done. When I was there, the academy appeared full of energetic and capable workers, who were glad to exhibit some of the latest results of their investigations. Dr. Karpinsky was presiding over a committee to consider the geological and physical aspects of the proposed railway between Usbekistan and Siberia (Turksib railway), which has since been successfully completed. I did not hear anything to suggest that the scientific men were not doing their very best to aid the country and develop its culture. The venerable Karpinsky, over eighty years of age, was as active as a young man.

Yet, in the midst of all this happy and fruitful activity, there was a note of alarm. It was as though one lived in a country of earthquakes, never knowing what the next hour might bring forth. Every one knew that it was possible to be arrested, usually in the small hours of the morning, and carried off to some place not designated. The brother of one of my best friends had disappeared in this fashion, and although the family found out what had become of him, they could only guess at the cause of his arrest. I believe he has now regained his liberty. I had in my pocket a little note-book, crammed with scientific information, and including addresses of people I had met and a sketch-map of the streets of Irkutsk. An official (not of the academy) who happened to see it was greatly alarmed. What would they do if they found that? Yet it contained nothing whatever of a political nature, and as a matter of fact no one asked to see what was in my pockets. At Tashkent we were asked to meet a lady who had been born in California, but had married a Russian and lived for many years in Turkestan. My wife being an old Californian, they wished to talk over old times. But when we sat down to the meal where this lady should have appeared, a note was brought, stating that she could not come, for reasons she would explain later. When we returned through Russia, we learned that we should cross the Volga about midnight. So my wife and I remained awake, and when we came to the great river got up and looked out of the window. We were about a third of the way across when a soldier with a gun appeared, and ordered us back to bed. This was done as a matter of routine; he did not know who we were.

I cite these various occurrences as typical of the existing state of mind. The government is afraid of the people; the people are afraid of the government. I spoke of this to an intelligent Russian. Yes, he