catch of 27,821 skins taken in 1919, the total gross revenue for the lot would be $3,922,204.58.

In view of the feverishly advancing prices of all kinds of real fur, the growing scarcity of the supply, and the clamorously insistent demands, both of the rich and the poor, there are good grounds for the belief that very soon we will see good raw fur-seal skins selling at auction at an average price of $250 each. With 110,000,000 people in America demanding "fur," the future of the trade in real fur is remarkably bright—so long as the supply lasts—and Congress may regard the future of the nation's fur seal industry with entire complacency. The saving of the fur seal herds was a good investment.

In the future, when all other bearers of good fur have been utterly exterminated—*as they soon will be*—the protected fur seal herds will produce, by sure-and-certain arithmetical progression, a really vast quantity of the finest fur in the world. It needs no stretch of prophecy to foretell the annual increment to the three nations who now are so sensibly preserving the fur seals of Alaska from killing at sea. When we begin to take, as we formerly did in the days of the fur seal millions, an annual catch of 100,000 skins, the importance of the salvaged fur-seal herd will be realized. If we figure it out on a basis of the sale of February 2, 1920 at St. Louis, the answer is $14,098,000 *per year*, 75 per cent. of which will belong to the United States.

Under the terms of our treaty with England and Japan we are dividing net proceeds with those two partner nations, who now help us to preserve the fur seals when at sea, on the perfectly fair basis of 15 per cent. to Japan, and 10 per cent. to England. During the five-year closed season we annually paid to each of those two nations the sum of $10,000.

In its habits the fur seal—which in reality is not at all a true seal, but a fur-coated sea-lion—is one of the most remarkable of all sea-going mammals. There are writers who still insist that fur seals can be managed by man just as a farmer manages his herds of breeding cattle and horses. As a matter of fact, the fur seal is hopelessly wild and untamable, and the only "management" that man can bestow upon the free animal is in terms of slaughter. He can drive it and kill it by artificial or by natural selection, but that is absolutely all. The fur seal migrates, returns, breeds and feeds solely in accordance with its own erratic and persistent will, and man's so-called "management" lies solely in the use of the seal-killer's club and the skinning-knife.

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SIDE-TO-SIDE VERSUS END-TO-END CONJUGATION OF CHROMOSOMES IN RELATION TO CROSSING OVER

The stonefly, *Perla immarginata* Say, is exceptionally fitted for chromosome studies as it has only five pairs (including the X-Y pair) of chromosomes, each pair of which is structurally differentiated from all others. My observation on this form made in 1917–18 forced me to the conclusion that in the prophase of the first spermatocytic division "homologous chromosomes are connected to each other telosynaptically in the spireme," and later "they bend toward each other at the synaptic point and become reunited parasynaptically before metaphase." These conclusions are in agreement with a limited number of workers but are so opposed to the general contention of the majority of cytologists to-day that it was considered then unprofitable to do anything more than describe the process as observed. This was done in my previous paper in the *Journal of Morphology*,[1] in which no attempt was made at theoretical discussion in relation to certain genetical evidences.

As so convincingly summarized in Morgan's recent book,[2] Mendel's original law—the segre-

[1] Nakahara, W., "A Study on the Chromosomes in the Spermatogenesis of the Stonefly, *Perla immarginata* Say, with Special Reference to the Question of Synapsis, *Jour. Morphol.*, Vol. 32, 1919.

[2] Morgan, T. H., "The Physical Basis of Heredity," 1920.

gation and independent assortment of factors —has been shown to have a close parallelism with the actual behavior of the chromosomes. The situation is quite otherwise, however, as to the mechanism of crossing over. Morgan is right when he states that "while the genetic evidence is favorable in all essentials to the theory of interchange between homologous chromosomes, it must be confessed that the cytological evidence is so far behind the genetic evidence that it is not yet possible to make a direct appeal to the specific mechanism of crossing over on the basis of our cytological knowledge of maturation stage." Morgan, however, assumes the side-to-side conjugation as a fact. His analysis of data on parasynapsis leads him to the conclusion that the early thin thread stage is most favorable for crossing over to take place. End-to-end conjugation, or telosynapsis, according to Morgan, "would have serious consequence for genetics . . . , for while side-to-side union offers an opportunity for interchange between the paternal and maternal members of a pair, no such interchange could be postulated if end-to-end conjugation took place."

It is the purpose of the present note to emphasize that the process of end-to-end conjugation, at least as described by Nothnagel[3] for a botanical object, and by myself[1] for a zoological one, does offer an opportunity for crossing over to take place, contrary to Morgan's statement. End-to-end conjugation simply restricts the stage in which such an opportunity is offered. This can be readily seen from the works of the above-mentioned authors, who describe essentially the following process:

A separate loop or segment of double spireme, whatever the nature of its duality may be, gradually bends and halves of the loop come to lie closely side by side. In the tetrad thus formed there are four longitudinal strands or threads.

It will be seen, then, by telosynapsis, an opportunity is offered for interchange between chromosomes at the thick thread stage, but at this stage only, in the manner originally suggested by Janssen[4] in his chiasma type theory.

It must be remembered that the condition of the chromatin threads at the early stage when the double spireme develops is extremely difficult to study minutely and accurately with the method and apparatus at our command. Under such circumstances, any inclination on the part of the observer will have a considerable influence on the interpretation. If one is so disposed, he may consider the condition of the threads as representing the process of pairing up. Dual threads develop out of reticulum at this stage, and that was all I could be sure of. There was certainly no observable evidence of the process of pairing up of two simple threads at least in the stonefly I studied.

On the contrary, the formation of a tetrad or ring by the bending of a loop of double spireme, which appear in haploid number is a clearly demonstrable fact. It is from this ground that I interpret the haploid as being composed of two homologous chromosomes jointed up end-to-end, and its duality as indicating primary splitting. No one has ever seen two chromosomes actually coming into conjugation, but the subsequent bending, reconjugation in side-to-side position, and the ultimate segregation at metaphase, of the halves of the loop is explicable only under the assumption that two chromosomes were united end-to-end in the loop.

Whether I am right in this interpretation or not will be decided by future studies—perhaps in very near future. Detailed comparison of the premeiotic stage with the prophase of somatic mitosis would throw some light on the situation. Also, a careful re-examination of forms (Orthoptera, for instance), in which parasynapsis is customarily claimed to occur, with special reference to the haploid loops in the thick thread stage would help settle the question. Possibility no doubt exists that the

[3] Nothnagel, M., "Reduction Division in the Pollen Mother Cells of *Allium tricoccum*," *Bot. Gaz.*, Vol. 61, 1916.

[4] Janssen, F. A., "La théorie de la chiasmatypie. Nouvelle interprétation des cinèses de maturation," *La Cellule*, T. 25, 1909.

process may be different in different organisms, but I consider it rather improbable in view of the fact that both para- and telosynapes have been described for different groups of plants and animals, and especially since certain "evidences" involved in the argument are not easily observable.

Summing up: contrary to the general belief, so-called end-to-end conjugation does offer an opportunity for interchange between chromosomes at the late thick thread stage in the prophase of maturation division, but at this stage only. If telosynapsis is a universal phenomenon, it would seem that crossing over must take place at the stage here specified. Of course, no morphological evidence has yet been produced for crossing over, and the most that can be said from the present cytological data is that such an interchange is not impossible at a certain stage in the muturation division.

Waro Nakahara

DESTRUCTION OF ZOOSPORES OF PLANT DISEASE ORGANISMS BY NATURAL ENEMIES

In making some motion-picture photomicrographs of the liberation of zoospores from the sporangia of *Physoderma zeæ maydis* (see Tisdale, *Jr. Agr. Res.*, Vol. 16, p. 137, 1919) the author observed destruction of the zoospores by certain animalcules which are commonly found in decaying vegetable material. No reference has been found regarding the importance of these natural enemies of the plant diseases which are disseminated by zoospores.

The number of zoospores swallowed by one rotifer (*Proales* sp.) is remarkable. When the animalcules are abundant there is a speedy disappearance of the zoospores. One infusorian (*Keronia* sp.) was observed to devour a perfect stream of the zoospores of *Physoderma,* at the same time increasing in size until it became gorged almost beyond recognition.

In active cultures one may see a field in the microscope filled with millions of zoospores swimming about. In a few hours large numbers of these have been devoured by the animalcules, which rapidly increase in numbers. A few hours after this one then sees these same protoplasm constituents swimming about not as zoospores but as animalcules. The process of change is so rapid it makes one wonder if there is always cleavage of the proteins and resynthesis or whether there may not be some shorter method of assimilation especially in the unicellar organisms in which the cytoplasms of the infusorian and the zoospore ingested are in such intimate contact.

In starting from dry material collected from cornstalks infested with *Physoderma,* the animalcules appear first and are on hand for each crop of zoospores.

It would be desirable to determine just how important such animalcules are as natural enemies of those plant diseases which are disseminated by zoospores. Also we should collect data to determine if the destruction of the soil animalcules by excessive liming may not be correlated with epidemics of these diseases.

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THE JOURNAL OF MORPHOLOGY

At its annual meeting in St. Louis, the American Society of Zoologists voted to accept the proposition made by Dr. M. J. Greenman, of the Wistar Institute, that in the future the society should assume control of the scientific policy of the *Journal of Morphology* and elect the editorial board, while the Wistar Institute retained control of the financial management of the journal.

A committee composed of M. M. Metcalf, Caswell Grave and W. E. Castle was appointed to initiate a scientific policy; to nominate an editorial board; to consult with the advisory board of the Wistar Institute and to refer its recommendations for final decision to the executive committee of the society.

This committee on publication and the executive committee and the Wistar Institute have agreed to the following action which accord-