soluble potash salts which are in ordinary use. After these experiments were concluded the attention of the writer was called by Dr. F. K. Cameron to the fact that the value of ground orthoclase as a fertilizer had been pointed out by several investigators in the past, notably by Magnus in Germany (1850), Aitken in Scotland (1887) and by the Maine State Experiment Station and the Colorado Experiment Station (1889 and 1901) in our own country.

A paper is being prepared to be published in due time which will present all the information so far obtained upon this subject. This country is at present dependent upon foreign sources of supply for all the potash used annually for fertilizer by our farmers and growers, and in case of foreign wars, embargoes or reprisals, we should be cut off from a steady source of supply. The great stimulus that has been given by our growing cement industry to the art and economics of the milling of rocks to almost flour-fineness makes it possible to-day to consider the feasibility of grinding, not only some of our feldspar deposits, but even our richer potash-bearing feldspathic rocks, like some of the granites which we possess in unlimited quantities. To the proper solution of a problem of this kind it is necessary to enlist the interest and attention of men familiar with the economics of rock grinding and the handling and transportation of material in bulk, as well as of growers and experimental agriculturists. The object of this communication is to call attention to the interest and importance of the problem, and to open the field to all who are desirous of experimenting or of making actual use of ground rock for fertilizing purposes.

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SPECIAL ARTICLES.

ZIEGLER'S THEORY OF SEX DETERMINATION, AND AN ALTERNATIVE POINT OF VIEW.

IN his recent pamphlet entitled 'Die Vererbungslehre in der Biologie' Professor H. E. Ziegler proposes a new theory of sex determination. It was said even at the time of Drelincourt that no less than 262 groundless theories of sex had been suggested; and it may be added that since that time there has been no falling off of interest in the sex question if the number of new theories proposed is a criterion.

Ziegler attempts to bring the question of sex determination under the prevailing view of specific chromosomal action. In recent years cytological speculation has largely rested on the assumption that the chromosomes are the sole bearers of the hereditary qualities. Hence all questions of inheritance have been referred to them, and in consequence their changes in the cell have attracted extraordinary attention. Many theories of heredity have been based on the shifting changes in the chromosomes alone. Their capacity for stains has greatly facilitated their study, while the rest of the cell that does not show much differentiation in staining capacity has been often ignored. Only in the case of the egg has the cytoplasm received anything like adequate treatment. The experimental work of Driesch, and of Wilson, in particular, has shown the important rôle that the cytoplasm plays in development.

Ziegler's primary assumption is that the chromosomes that arise from a female individual have a greater tendency to produce a female; and those that originate from a male individual have a greater tendency to produce a male. Since the child gets as many chromosomes from the father as from the mother, the parental chromosomes as such can not determine the sex. But it is to be recalled that amongst the parental chromosomes some have come from the grandfather and some from the grandmother. The relative number of chromosomes from the maternal and paternal lines will be variable in number on the current assumption that at the reduction division it is merely a question of chance which member of a pair of homologous chromosomes goes to one pole of the spindle, and which to the other. If the chromosomes of the grandfather predominate in the offspring it will be a male; if the grandmother chromosomes predominate a female develops.

To take an example. If the somatic num-

ber of chromosomes for the human species be assumed to be 24; the child gets 12 from the father and 12 from the mother. If amongst the former there are 8 grandmother chromosomes and amongst the latter 7 grandmother chromosomes the child will be a girl, for there are at least 15 of the 24 derived from the grandmothers' side.

Ziegler admits that his grandmother theory of sex will not apply to all cases. The 'peculiar' methods of reproduction of the honey bee, the gall wasps, the daphnias, rotifers, and *Dinophilus* can not, be says, be explained in this way. This admission is in itself a serious objection to the theory, for any satisfactory theory of sex must be prepared to account for this class of cases, that can not be put aside by calling them 'peculiar.' But there are other and more serious objections to be urged against Ziegler's view.

In the first place, Ziegler's theory is only a special application of the *differential* chromosome theory, which Sutton first suggested might account for the Mendelian ratio. Boveri has recently followed Sutton's interpretation, and Ziegler also, it appears now, adopts this point of view. Let us look for a moment more closely at this hypothesis, since it has an important bearing on Ziegler's assumption in regard to sex. Two views, both purely hypothetical, may be held as to the way in which the chromosomes represent the heredity qualities. Either, each chromosome contains only one set of characters, *i. e.*, the chromosomes are all different in regard to their hereditary material, or, they are all alike in this respect. Mendel's law is sometimes worked out on the former supposition, and appears to give a satisfactory explanation of how the *assumed* purity of the germ cells of hybrids may arise. On the other supposition, viz., that the chromosomes are all alike, it is difficult to explain the supposed purity of the germ cells of hybrids. In fact, on this supposition it can rarely happen that the germ-cells are pure in respect to any one character. If we reject the assumed purity of the germ-cells in Mendelian cases, and still attempt to explain the Mendelian ratio on our second assumption, viz., that the maternal or the paternal chromosomes are all alike, we can give a formal solution for some cases provided we assume that the reduced number of chromosomes is an unequal one; for, the results may then depend on whether more of the grandfather chromosomes or more of the grandmother chromosomes happen to get into a particular cell. But if we examine the list of cases given by Ziegler himself we find that the reduced number of the chromosomes is an even one in 29 species, and odd only in 10.

On Ziegler's theory of sex it is evident that whenever the reduced number of chromosomes is even there must often occur an exact balance of grandmother and grandfather chromosomes, hence the child can have no sex at all! For a small number of chromosomes this would often occur.

There is also a serious difficulty in the case of the other assumption that chromosomes are individually different. The peculiar inheritance of the Mendelian extracted recessives is difficult to understand from this point of view. For example, if a white mouse is bred to a gray mouse gray offspring will be obtained. If these gray offspring are inbred they give some gray and some white according to the Mendelian ratio. These white mice (extracted) are assumed to have been formed by pure white-bearing germ cells meeting white-bearing germ cells, but that this explanation will not account for their origin is shown by crossing these extracted white mice with black mice. The offspring will be gray according to Cuénot. This must mean that the extracted whites must contain gray in a latent condition, and moreover in sufficient amount to dominate the black color of the black mouse. Cuénot who has discovered this and similar facts offers an hypothesis to account for them, but it is an hypothesis far removed from the chromosome theory as applied to Mendelian cases, at least in the form maintained by Ziegler. Since neither assumption in regard to the chromosomes is capable of explaining certain results of the Mendelian cases the most obvious conclusion is that the germ cells are not 'pure,' and that the Mendelian ratio is not due to this sort of purity but to dominance and recessiveness of contrasted charac-

ters that depend on some other relation in the germ cells than that brought about by the shifting of the chromosomes in the reduction division to produce 'pure' gametes.

Ziegler's failure to give a satisfactory account of sex determination on the *differential* chromosome basis raises the wider question as to whether at the present time we are really obliged to look in this direction for a solution of the question. The known facts in regard to sex indicate that we have to deal with two sharply contrasted, yet interchangeable states. Furthermore, the facts seem to indicate that some internal mechanism exists that gives with great precision the one or the other condition. We lack completely at present the necessary knowledge of the chemistry of the cell on which alone we can hope to establish a real theory of sex determination. It might be possible indeed to invent a purely fictitious, *quasi* chemical, hypothesis, such, for instance, as assuming that the female and the male represent two contrasted conditions of the same protoplasm, one state being a combined (the female) and the other a separated (male) condition of the aggregate bodies (molecules) of which the protoplasm is composed. While we might, were it worth while, work out this or some similar idea into a more or less consistent hypothesis, the only value that such a conception might have at present would be to indicate that sex determination may not be the result of differential *nuclear* divisions that locate sex determining chromosomes in different cells, but that the process is chemical rather than morphological.

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THE SARGASSO FISH NOT A NEST-MAKER.

EVER since 1872 the sargasso fish (*Pterophryne histrio*) has been famous as the builder of a remarkable globular nest made of the sargasso weed, in the midst of which it finds a congenial home. Professor Louis Agassiz first described such nests observed by him in December, 1871, during a voyage to Brazil and attributed them to the Antennariid. No one has since doubted the accuracy of the identification, and in innumerable works it has been accepted as well established. A few weeks ago, however, Dr. Hugh Smith, deputy fish commissioner, informed me that he had obtained eggs laid by the sargasso fish and, on a visit to his office, he showed me some under a microscope, and I was surprised to find that they were quite different from those found in connection with the nests and which had been elaborately described by Vaillant and Möbius. Later I received a letter from Professor E. W. Gudger, of the State Normal College of North Carolina, containing an account of the pterophryne's oviposition. This corresponds remarkably with that practised by the fish's distant relative, the angler (*Lophius piscatorius*). The elaborate provision thus made specially for the eggs, as well as the absence of polar filaments, negatives the attribution of such eggs to the nest-maker of the sargasso sea and leaves the question of the real maker an unsolved problem. Similar eggs were found free on the surface of the sea off the African coast and noticed by Cunningham (1887) but not identified. Can such be the product of a flying-fish?

The fish, whatever it may be, is probably not a direct maker of the nest but the filaments of the eggs may, perhaps, become mechanically entangled with the fronds as well as with each other and the contraction into a subglobular mass may be the result.

Professor Gudger's communication is herewith submitted.

THEO. GILL.

A NOTE ON THE EGGS AND EGG-LAYING OF PTEROPHRYNE HISTRIO, THE GULFWEED FISH.

SPECIMENS of the gulfweed fish occasionally drift with the *Sargassum* into the harbor of Beaufort, N. C., and are picked up along the beach by boys and brought to the laboratory of the United States Bureau of Fisheries.

When I reached the laboratory about the middle of June, 1903, there were two of these interesting fishes confined in an aquarium of running salt water. These were put in my care and on one of them and its eggs the following observations were made. The two fishes were of unequal size and were contin-