It is impossible to review in the space available the wealth of detail which makes up this second volume. Much of great interest must be passed over. The correspondence with De Candolle and Darwin on heredity includes the difficulty between Galton and Darwin about the experimental work on pangenesis, which Pearson handles with fine diplomacy and tact but still in such a way as to permit the reader to form a sound judgment for himself as to what actually occurred. The psychological investigations can only be mentioned. The chapter on "Photographic researches and portraiture" must be more particularly noticed, because in composite photography was one of Galton's most ingenious ideas. It was technically so difficult that it was never followed up thoroughly. But its theoretical bearings and potentialities are so great that it seems certain that some day some one will build the proper superstructure on the solid foundation Galton laid. Pearson sketches these bearings in the following passage:

> To grasp fully Galton's photographic activities at this time we must bear in mind two important facts. He was still searching for some physical features which should have high association with the mental characters. This attitude was perfectly reasonable at that date, because not only no correlations between such characters had been determined, but the methods of measuring correlations were of the crudest kind. Further Galton was a traveller, and every traveller is accustomed as he passes along to notice that the racial mentality changes with the change of the physical characters. The conception therefore naturally arises that physique and mentality are highly correlated. The American Indian, the Negro, or the Arab has each his individual physique, and each also his individual mentality. But this appearance of high correlation may be most deceptive; it does not follow that there is any organic linkage between the physical and psychical characters. If a race be started from a pair of individuals both possessing a physique of type A and a mentality of type A′, we may find in later generations an apparent linkage of A and A′ in all the members; but this is not a true correlation, and a cross-breeding may show that A and A′ have no organic relation, and can be at once separated. In the second place Galton did, like most men of his generation and probably like most of us to-day, consciously or unconsciously, give weight to physiognomy. So impressed by physiognomy is mankind in ordinary every-day life, that we hardly realize how much confidence we place in it. We say a person is good or bad, is intelligent or stupid, is slack or energetic, on what is too often only a rapid physiognomic judgment. The custom is so universal as a rough guide to conduct, that we are almost compelled to believe that there is in human beings an intuitive or instinctive appreciation of mental character from facial expression. Galton differed only from the mass of us in desiring to ascertain on what physiognomic appreciation is based. He belonged to a generation in which the influence of Lavater and the belief in some form of phrenology were still appreciable. He accordingly sought to isolate types and to measure deviation from facial type, in order to determine whether facial variations were correlated with mental variations. He was really attempting to make a true science out of the study of physiognomy.

The final chapter deals with Galton's early statistical investigations, which were mainly along anthropometric lines, but also included many studies in the theory of statistics. From a technical point of view this is one of the most valuable chapters in the book, because of Pearson's penetrating comments on Galton's work. The following quotation from the lecture on "Generic Images" published in the Proceedings of the Royal Institution in 1879 epitomizes the point of view from which Galton approached the problems in which he was interested—and the range of his interests knew no bounds—perhaps better than anything he ever wrote.

> General impressions are never to be trusted. Unfortunately when they are of long standing they become fixed rules of life, and assume a prescriptive right not to be questioned. Consequently, those who are not accustomed to original inquiry entertain a hatred and a horror of statistics. They can not endure the idea of submitting their sacred impressions to cold-blooded verification. But it is the triumph of scientific men to rise superior to such superstitions, to devise tests by which the value of beliefs may be ascertained, and to feel sufficiently masters of themselves to discard contemptuously whatever may be found untrue. (p. 168.)

It is unfortunate that the manner in which this biography has had to be prepared precludes the possibility of an index until the whole work is finished. This lack is especially felt in the second volume. But even the mildest of critics of any point whatever must be disarmed in the face of the extraordinary excellence of this biography. It is a wholly worthy memorial of a very great man.

Raymond Pearl
The Johns Hopkins University

SPECIAL ARTICLES

CHROMOSOMES AND SEX IN SCIARA

A study of the chromosomes of *Sciara similans* Joh. (manuscript)[1] has disclosed a condition which, so far as known to the writer, has not been observed in any other organisms. Both sexes of this fly are diploid. The chromosome group of the female consists of four symmetrical pairs of chromosomes,

[1] The writer is greatly indebted to Professor O. A. Johannsen for identifying this as a new species and for giving it a manuscript name, which is used here. His taxonomic description will appear shortly.

two of which are rod-like and two V-shaped, as shown in Fig. 1. That of the male consists of four pairs like those of the female, and in addition two larger and individually distinct chromosomes, one V-shaped and the other J-shaped (Fig. 2).

Thus the male possesses two more chromosomes than the female. These two chromosomes are confined to the male line, passing from father to son in each generation. In the latter respect they resemble an ordinary Y-chromosome, but they differ from the Y-chromosome in other features. The Y-chromosome is a member of a pair and segregates at maturation. It has a mate, X, which is derived from the female parent and which passes into the female offspring. The two large chromosomes in *Sciara,* on the other hand, are unlike all the other chromosomes and have no mate or mates among them; they do not pair with or segregate from any of the others. It is not known, as yet, whether there is a true XY pair in this species or not.

Peculiarities are also observed in the maturation divisions in the male. The two large chromosomes apparently do not undergo synapsis—at least they are separate elements in the primary spermatocyte figures (Fig. 3). Consequently the first spermatocyte group consists of six chromosomes—the four pairs being represented by bivalents. Both of the large (univalent) chromosomes divide longitudinally at this division and their daughter halves go to opposite poles (Fig. 4). But one of the small V-shaped bivalents goes precociously to one pole undivided (Fig. 5). The chromosomes at this pole remain in the spermatocyte, while those at the opposite pole are cast off in a small bud, suggesting the polar body of an egg (Fig. 6).

Since the second spermatocytes are not appreciably smaller than the first (and for other reasons) it has not been possible to distinguish positively between first and second maturation divisions—or, indeed, to be certain that a second division occurs. Several indications of such a distinction have been observed, however, and it is believed that both divisions do occur. If so, the second is essentially like the first, with all the chromosomes dividing except the precocious V-shaped bivalent (not one of the two large chromosomes). Again the pole receiving the precocious chromosome remains in the spermatocyte and the other is cast off in a bud. The significance of the behavior of the precocious chromosome is unknown, partly because oogenesis has not yet been studied to ascertain what the oocyte contributes to the fertilized egg.

Whether both divisions occur in the male, or only one, the end result should be essentially the same, *i.e.,* each spermatozoan should receive both of the large chromosomes, both members of the precocious V-shaped pair and one member from each of the other three pairs. If these observations are correct (and the figures are clear and easily analyzed in most cases) all the spermatozoa should be male determining and all the fertilized eggs should give males. Females, therefore, should arise by parthenogenesis, unless a chromosome elimination occurs at some later stage.

But simple parthenogenesis apparently does not occur, for all virgin females tested (thirty in one experiment and many more subsequently) have failed to give offspring. Possibly the female-producing eggs need only the stimulus of the sperm to start development, as in *Rhabditis.*[2] This seems improbable, but it can be tested genetically, and experiments are under way for that purpose.

Sex ratios from single wild females or laboratory pairs in this species range from all females to all males, with various intermediate proportions. Of 27 wild females tested, 12 gave only females, 10 gave only males and 5 gave mixed broods. Similar results are given by laboratory pairs. It seems probable that this behavior is associated with the cytological features described above, although further genetic study, with egg counts, etc., is prerequisite to an explanation.

No detailed discussion of the above problems will be attempted at this time, but a word may be said as to the nature of the evidence presented. The cytological observations are based on an examination of over a hundred specimens. Spermatogonial or spermatocyte figures have been studied in specimens from twenty-four different parentages, including seven wild sources. Ovarian figures have been obtained in material from thirty different parentages, including eleven wild sources. In four cases brothers and sisters were examined. Spermatocyte divisions

[2] Kruger, *Zool. Anz.*, 40, 1912.

have been obtained in males from both bisexual and all-male broods (from single mothers) and ovarian figures have been obtained in females from bisexual and all-female broods. This makes it seem certain that the difference between the chromosomes of the two sexes represents a normal condition, not peculiar to any one stock or race and not due to hybridization of different species. It also tends to indicate that the maturation phenomena are the same in all males, regardless of their source, although this point can hardly be established until further genetic study has been completed.

Stock cultures of this species have been kept in the laboratory for a year (over 18 generations) and others have been kept for three or four generations. Many intercrosses have been made. Both unisexual and bisexual broods have been obtained from these, but no sterile combinations have been found—which tends to indicate that we are dealing with only one species. If not, and the aberrant sex-ratios are due to hybridization of true species, these species must be very similar and the crossing must occur freely in nature.

Similar chromosomal relations of the sexes and a similar type of spermatogenesis have been observed in a closely related species, *S. pauciseta* Felt, and in a more distantly related species of the same genus. It seems possible, therefore, that the chromosomal relations described above are characteristic of a number of species—perhaps of the genus.

ADDENDUM: Since the above was sent to press evidence has been obtained which indicates that probably all the spermatocyte divisions described here are from secondary spermatocytes and that the first division is very different—involving no division of the two largest chromosomes. The account of spermatogenesis should be corrected accordingly, although the main feature (the passage of both large chromosomes into all the spermatids) is not altered.

CHAS. W. METZ
DEPARTMENT OF GENETICS
CARNEGIE INSTITUTION OF WASHINGTON

THE EFFECT OF LIGHT ON THE PERMEABILITY OF LECITHIN

BECKING and Gregersen have recently contributed an interesting paper[1] under the title given above. They use an adaptation of the method devised by the writer,[2] in which the electrolytic resistance of the more dilute of two solutions separated by a more or less permeable diaphragm is measured from time to time and the permeability of the diaphragm deduced from the change in resistance. By this method Becking and Gregersen claim to have shown that the permeability of membranes consisting of lecithin and collodion in equal proportions was increased by illumination. Their paper unfortunately leaves the matter somewhat in doubt, for two reasons which are here considered.

The first doubt of the validity arises by reason of the fact that as far as one can judge from the data given, equilibrium is attained when the distilled water in one compartment has been entered by only enough salt from the other compartment to reduce its resistance from 15,000 to 30,000 ohms to about 2,000 ohms. The upper compartment originally contained 0.2N KCl, and the lower one distilled water; since they appear to have been of nearly equal volume, they should both have contained approximately 0.1N KCl at the end of the experiment. But the observed change in resistance was nowhere nearly adequate to account for an increase in KCl concentration from practically none (distilled water) to 0.1N, and the final conductance was so small that one almost unavoidably concludes that diffusion through the membrane stopped while there was still a difference in concentration between the solutions separated by the membrane, and while there was still a concentration gradient through it. In this case there must have been factors, other than light, which controlled the permeability, if in fact the membrane was permeable to KCl at all. This can not be assumed unless, as in the writer's experiments on *Laminaria*[2] it can be shown that: (1) no change in resistance occurs when both compartments are filled with the more dilute solution, so that no diffusion gradient through the membrane exists: (2) the increase in resistance in the more concentrated solution keeps pace with the decrease in resistance in the more dilute solution.

In this connection it should be noted that under the conditions of the experiment the rate of change of conductance in the lower cell should theoretically be nearly

$$\frac{dx}{dt} = K(a-2x),$$

where x = total change in conductance of the more dilute solution at the time t, and a = original difference in conductance of the two solutions. Integrating, we obtain

$$Kt = \log \frac{a}{a-2x}$$

from which we may, if a is known, calculate values of K from the observed data. No data being given as to the value of a, it may still be of interest to assume a probable value, namely $a = 50 \times 10^{-4}$ ohms^{-1}, and to calculate the values of K from the data given by Becking and Gregersen.

[1] Becking, L. B., and Gregersen, M. I. *Proc. Soc. Exp. Biol. Med.* (1924), XXII, 130.

[2] Brooks, S. C., *Bot. Gaz.* (1917), LXIV, 306.