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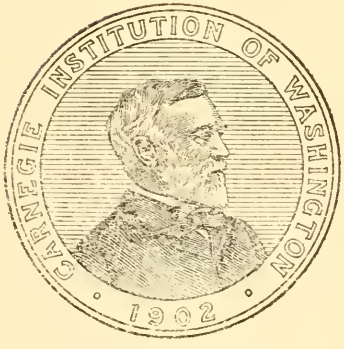

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DEPARTMENT OF EXPERIMENTAL EVOLUTION AND EUGENICS RECORD OFFICE.[1]

C. B. Davenport, Director.

The present seems a fitting time to look back over the past work of the two departments reported upon herewith and to consider the plans for the future.

The Station for Experimental Evolution was started in 1904, not long after the beginning of the new era, which dates from the rediscovery of Mendel's law by De Vries, Correns, and von Tschermak in 1900. That our highest hopes for the Station have all been realized can not be affirmed; but in some respects we builded better than we knew. Thus at this Station was made the first discovery of the variation of chromosomes associated with, and inducing, a corresponding mutation of a species (the evening primrose). This lead has opened up great advances made by Professor Morgan and his colleagues. By discoveries made at this Station we see clearly that there are two types of mutations—the one due to irregularities of assortment of chromosomes and the other to changes in the chromosomes themselves; there are interchromosomal mutations and intrachromosomal mutations.

Again, studies made at this Station on the evolution of the chromosomal complex, especially in the flies, have led to the general conception that evolution has proceeded not primarily by modifications of the series of visible organisms whose evolution is the goal of our researches, but rather evolution has proceeded by changes in the "germ-plasm," the chromosomes, and that these changes have occurred in some cases apparently owing to its intrinsic properties—as radium changes into lead—and sometimes under the influence of intracellular changes, such as are induced by hybridization, and sometimes, perhaps, by extreme conditions external to the germ-cell. However it arises, once a change in the germ-plasm occurs, a corresponding change occurs in the *body* that develops under the control of that changed germ-plasm. The "giant evening primrose" is a giant just because it has, by a sort of accident, gained additional chromosomes. The polydactyl fowl, or man, has this peculiar condition because, in advance, a corresponding change in the "genes"of the chromosomes has occurred. Man is tailless, we may guess, because of a change in the gene that permits or induces a tail to develop. If that change had not occurred, man would doubtless have been a tailed "thinking being." Mankind is what it is in its physical, mental, and temperamental aspects because of the antecedent changes that occurred in the chromosomes of man's ancestors; and even inside of the "human" group, by changes in genes, numerous inheritable subgroups or "biotypes" have arisen with their physical, mental, and tem-

[1]Situated at Cold Spring Harbor, Long Island, New York.

peramental peculiarities. All these conclusions, which arise naturally and inevitably from experiments and observations in which this country has taken a leading part, are bound to revolutionize man's attitude toward himself, toward racial differences, and toward those aberrant individuals who constitute so great a "social problem."

The Eugenics Record Office has played its part in applying some of the genetic studies to man. It has first pointed out the method of inheritance of "feeble-mindedness" (1912), of epilepsy, of temperamental disorders, of eye and hair color, and of inheritance of traits in negro-white crosses.

The work of the two departments at Cold Spring Harbor is fast becoming interlocking. We have studied the distribution of twins in human families and secured an interpretation of that distribution by studies of sheep and pigs. Experimental studies of instincts and temperament in dogs will supplement the pedigree studies made on humans; and so with studies of heredity of cancer and the sex ratio.

The future direction of our work lies plain before us. First, the work of the Station for Experimental Evolution and that of the Eugenics Record Office are so akin and so interdependent that they should obviously be united in one department of Genetics, combining the two sections, each of which will continue to develop its work by the use of methods appropriate to it.

Second, the experimental work at the Station should be largely with mammals because of the probability that genetical results obtained with them will be not only of general genetical interest, but also of especial interest for the heredity of human traits. Thus, we should breed dogs for the light they will throw on heredity of instincts and temperament, rabbits (in so far as we can afford to) for the large number of characters that have already been worked out in them, mice for the light they throw on the inheritance of resistance to malignant growths, upon color factors, and upon problems of mammalian fecundity. At the same time, we should continue to develop to the utmost our genetical studies in plants and insects and, as opportunity offers, in other organisms also, in order to maintain a broad view of genetical phenomena, and because there is some of this work (as on plants) which we are especially well fitted to do.

It is also clear that progress in genetics will be made only as we consider reproduction generally—what sex is, the sex ratio, and how it is modified; differential fertilization and mortality; and the rôle of lethal or absence of vital factors.

For progress in genetics we need the assistance of the cytologist, the anatomist, the biochemist, the biometrician, and the artist. These accessory divisions of our work should not be permitted to develop independently, but only as handmaidens, of genetics. Thus we are not interested in painting or photography as such, or biometry and sta-

tistical analysis, or physiological chemistry or anatomy or microscopy *per se*, but only for the assistance they can give to our main problem.

In a department like ours it is necessary to keep an eye single to our main purpose and avoid diversion of funds to subsidiary matters, however deserving of investigation. It is necessary to plan our work more and more for a common purpose and to apply to all of our investigations the principle of cooperation.

In detail our future plans are as follows: To make a series of preliminary reconnaissance studies on pedigrees of human traits, including instincts and temperament, as particular opportunity arises. Statistical studies on human mate-selection, differential fecundity, and the sex ratio are also planned. On the experimental side it is proposed to push the study of inheritance of instincts (in dogs), of tumor-growth (in mice), of sex ratio, of the meaning of sex and sex intergrades, of fecundity, of sterility, of particular traits in animals and plants, including rabbits, mice, pigeons, bantam fowl, *Portulaca*, and *Datura*. The present status of studies on this material is more fully described in the following pages and in the publications of the department.

The main results of the year's work may now be passed briefly in review. The Director completed his assignment at Washington to the service of measuring 100,000 veterans at demobilization, and secured (1) standard measurements for the use of the Army in making uniforms (and incidentally for clothing manufacturers in general) and (2) a mass of anthropological data concerning the American population comparable to and greater in amount than that secured by Dr. B. A. Gould at the close of the Civil War. In the conduct of the present work leading anthropologists and anatomists of the country were enlisted. A discussion of these measurements, together with those made on 2,000,000 men at mobilization, is now ready for the printer.

An attempt was made by your Director to throw light upon the ancient problem of the meaning of human multiple births. A study of original records showed an inheritable tendency on the maternal side toward double ovulation, but also a nearly equal hereditary tendency toward twin production on the part of the male. Also, the method of inheritance appeared irregular. This led to a study of plural births in pigs and to the discovery (which proved to be only the confirmation of a discovery made by Hammond in 1914) that an important proportion of fetuses fail of full development *in utero*, probably because of lack of vital factors, while another fairly large percentage of eggs ovulated fail (even under favorable conditions) of fertilization. The proportion of these failures will be less, the more active, abundant, and freer from lethal factors the sperm is. Fathers of twins, experience indicates, belong to exceptionally fertile strains. Thus it comes about that fathers of twins are about as apt to belong to twin-producing strains as mothers of twins, and that twins depend on constitutional, hereditary factors on both sides of the house.

A second discovery of importance has recently been made here by Blakeslee and Belling, namely, that some of the irregular breeding behavior of the jimson weed (*Datura*) is due to irregularity of chromosome-division in cell-division, resulting in extra chromosomes in some gametes and a deficiency in others. This behavior is like that discovered by Miss Lutz in the primroses at this Station 13 years ago, which was the starting-point for the great development of our knowledge of the relation between somatic mutations and chromosome variation. The recent discovery establishes that there are two forms of mutations: one due to extrachromosomal changes or, better, changes in number of chromosomes, and one due to intrachromosomal changes, to changes in the genes.

A third capital discovery is that in a strain of mice susceptible to a particular tumor the susceptibility is not only found to be hereditary, but it is shown that the hereditary factors are probably 4, though possibly 3 or 5 in number. This discovery makes much more definite the previously known fact of inheritance of cancer in mice, and gives an explanation of the failure to find a *simple* Mendelian explanation of inheritance of cancer in man. The inheritance is Mendelian, but there are many factors and not merely a single factor involved.

A fourth matter, whose study is now completed, was described in a preliminary way in last year's report. It is the demonstration that the effects of alcoholization of breeding rats show themselves in the grandchildren of such rats. Not, indeed, in the gross fashion described by Stockard in the case of his guinea-pigs, but by a certain stupidity or inability to learn and take advantage of experience. No other experiment of this sort approaches in careful control this series of MacDowell. One may confidently assert, therefore, that the deleterious influence of alcoholism on even remote progeny has been proved.

Again, there has been demonstrated by Riddle a chemical difference between male and female pigeon embryos, inasmuch as relatively more female than male embryos withstand a diminished oxygen pressure, indicating that they have a lower metabolism than male embryos. Thus, the fundamental difference in metabolism of the two sexes has been demonstrated for pigeons in their germinal, embryonic, and adult stages.

Finally, the theory of lethal, or loss of vital, factors has been extended to human heredity by Dr. Little, who finds clear evidence of sex-linked lethal factors in color-blind and hemophilic (bleeding) families, in consequence of which there is a larger proportion of males that are color-blind than would otherwise be expected.

EXPERIMENTAL EVOLUTION.

REPORTS ON INVESTIGATIONS IN PROGRESS.

THE GERM-PLASM AND ITS MODIFICATION.

COMPARATIVE STUDY OF THE CHROMOSOME GROUPS IN DIPTERA.

Dr. C. W. Metz, in association with Dr. J. F. Nonidez and Mrs. Rebecca C. Lancefield, has continued his studies on the chromosomes of *Drosophila* and other Diptera. Dr. Metz reports that, owing to the peculiarities of chromosome behavior (especially paired association) in the flies and the possible bearing of these peculiarities on genetical phenomena, it has seemed desirable to make a detailed study of the maturation processes in this group. Since no one species offers the best technical conditions, and since the cytological processes seem to show differences, there is being made a comparative study of numerous species scattered through the order. A study of spermatogenesis in two species of robber flies (*Asilus*) by Metz and Nonidez has been completed, a similar study of a stratiomyid fly (*Ptecticus trivitattus*) is nearly finished, and studies in other families are partially completed; studies on oögenesis in three families of Diptera are under way.

The studies in *Asilus* yielded a result of great theoretical importance. In the zygote, as is well known, each kind of chromosome is paired, one of each pair coming from the egg and one from the sperm. In a cell-generation before the ripe gametes are formed the "homologous" members of the pairs come together in what is called "synapsis." In the following cell-division the two members of each synaptic pair separate, one going to each daughter-cell, so that the ripe gamete contains only one of each homologous pair. Now Metz has found that in *Asilus* the homologous chromosomes remain closely associated throughout the entire growth-period of the first spermatocyte, with a consequent modification of the synaptic processes, due to the elimination of the leptotene and zygotene stages. True synapsis occurs in the telophase of the last spermatogonial division.

COMPARATIVE GENETICAL STUDIES ON DROSOPHILA.

The studies on oögenesis, Dr. Metz reports, although incomplete, suggest that in the female the processes may be different from those in the male, and that the difference may be responsible for the difference in genetical behavior in the two sexes (crossing-over in the female but not in the male) of *Drosophila*. This study is being extended in the hope that the question may be settled by a combination of cytological and genetical work on favorable material, as follows:

"In conjunction with the cytological studies on *Drosophila*, considerable genetical work has been carried on for the purpose of analyzing the genetical constitution of the chromosomes in different species of *Drosophila* and ascer-

taining, if possible, their genetic relationships. The failure of all attempts at hydridizing species having different chromosome numbers has led to an extension of the intensive study of the selected individual species mentioned in previous reports. The genetical analyses thus obtained are being compared with one another and with that of the well-known *Drosophila melanogaster* (*ampelophila*).

"Studies on *Drosophila virilis* (a species possessing 6 pairs of chromosomes) have resulted in the identification of 5 groups of linked genes, representing, it is believed, the 5 large pairs of chromosomes. Approximately 30 genes are represented in the 5 groups. The sex-linked group of characters in *D. virilis* contains 3 members showing such a resemblance in morphological features and genetical behavior to 3 in the sex-linked group of *Drosophila melanogaster* as to suggest very strongly that they are homologous in the two species and, consequently, that the sex-chromosomes of the two species are similar in constitution. In the non-sex-linked or autosomal groups the evidence is insufficient, as yet, to indicate whether or not similar homologies exist between other chromosomes in the two species.

"In *Drosophila willistoni* more than 40 mutant characters have been studied—mainly by Mrs. Rebecca C. Lancefield. These characters fall into three groups, corresponding to the number of large chromosomes. Particular interest attaches to *Drosophila willistoni* because of the lack of any conspicuous parallelism between its mutant characters and those of any other species thus far studied. No explanation of this fact is apparent at present, and a detailed discussion of the results may, therefore, be postponed until further evidence is secured.

"Our material of *Drosophila obscura* has been transferred to Mr. D. E. Lancefield, of Columbia University, who has undertaken extensive work on this species."

STERILITY IN MUTANT HYBRIDS OF DROSOPHILA VIRILIS.

Dr. Metz and Dr. Weinstein have discovered in *Drosophila virilis* a series of 3 allelomorphic sex-linked mutations affecting the eye:

(1) *Rugose:* Characterized somatically by a slight paling and roughening of the eye, evident in the male only, the female being entirely normal in appearance; in fertility both sexes seem to be fully equal to the wild stock.

(2) *Glazed:* This is more extreme in all respects; the eyes have a glazed appearance in both sexes, though the males are the more affected. Affected females are usually sterile. Of 150 females tested, 3 only were fertile. The males have a reduced fertility.

(3) *Wax:* This is still more extreme. The eyes of both sexes are greatly affected, resembling masses of yellow wax. The females seem to be practically or entirely sterile. The males appear to breed more poorly than do the glazed males; when rugose and glazed are crossed the hybrid females are rugose-like, but are all sterile. When rugose and wax are crossed, the hybrid females are all rugose-like but nearly all are sterile. Hybrids from "rugose," "glazed," or "wax" mated with other mutants are fertile. It appears, then, that the sterility of "glazed" and "wax" does not reappear in hybrids with more fertile mutants except their allelomorph "rugose." This sterility of certain

mutants is a matter of great importance, to the investigation of which this Station is devoting much attention, both in animals and plants.

MODIFIABILITY OF THE GERM-PLASM BY ALCOHOL.

In earlier Year Books have been outlined plans for studying the inheritance, if any, of the effects of alcohol. In making this study some rats of a litter of an inbred (and therefore probably homozygous) strain of rats were subjected to alcohol; others of the same litter were not. It was to be expected that the first generation of offspring (derived from germ-cells in the body of the alcoholized parents) would show the effects of this alcoholism; any defect would then be ascribed to a modification of those germ-cells. But if the grandchildren also are modified, that would indicate that the germ-plasm of the reproductive cells of the treated grandparents had been modified. It can not be denied that the effect might possibly be due to a modification of the cytoplasm of the germ-cells of the treated individuals; but this is, perhaps, less probable.

Maze-behavior of the grandchildren of alcoholized rats: With the cooperation of Miss Vicari and of other members of the staff, Dr. MacDowell has during the year completed the first formal report on the experiments of the hereditary effect of alcohol on rats, started in 1914. This paper has been submitted for publication in the Journal of Experimental Zoology, under the title, "Alcoholism and white rats. I: Influence of alcoholic grandparents upon maze behavior."

In this paper there are compared, with great care, various parallel series of measurements of reactions in the maze of the grandchildren of alcoholized rats and the grandchildren of non-alcoholized rats that were sibs of the alcoholized ones. A comparison was made of the time taken in learning the maze during the entire 24 trials of the original training; also, separately of the first 12 trials, of the second 12 trials, and of all but the first 3 trials. After having learned the maze, the rats rested for a period and were then again tested in the maze to find out how well they retained what they had learned. The two series were compared by giving each rat 12 trials in this retention test. The 24 original learning trials and the 12 retention-test trials were combined for a comparison. Furthermore, the daily record (of 3 trials per day) was compared for the two series.

To the above comparisons of learning and retention ability of descendants of alcoholic and non-alcoholic rats were added the following: The distance covered in running each trial; the speed, or distance per second; the different types of errors made; the number of trials before the first perfect trial, and the time spent in running perfect trials.

Dr. MacDowell reports further:

"Whenever the numbers appeared large enough to warrant the calculation of standard deviations, these have been obtained and the probable errors of the averages and of the differences between the test and control averages

have been given. The chi-square (χ^2) test has been applied to the data on time and distance, to discover how unlikely it is that such differences as are found between the test and control data are due to chance, instead of to the treatment of the grandparents. In making the averages the following groupings of rats have been used (in each of these the tests are always compared with the corresponding controls): strains separately with sexes separately; strains together with sexes separately; strains separately with sexes together; strains together with sexes together. In this way the unsettled questions of the genetic differences between strains and sexes may be eliminated. Briefly, these various groupings of the data show that the general conclusions are independent of whatever strain and sex differences may exist.

"In table 1 are given extracts from the results, with strains and sexes together. The greatest interest lies in the number of times the differences between the averages of the tests and controls exceeds the probable errors of these differences. When the quotient of the error into the difference is 3 or higher the difference may be considered statistically significant, that is, the probability is at most only 1 in 20 that such a difference will be due to chance. Plus differences indicate that the tests took more time or covered more distance than the controls. D/P.E. signifies the quotient of the difference divided by the probable error of the difference.

TABLE 1.—*Time (in seconds) and distance averages of the grandchildren of alcoholized rats (tests, 25 rats) compared with the grandchildren of the non-alcoholized grandparents (controls 25 rats) on each day of the training in the maze.*

Day.	Time elapsed in reaching center of maze.				Distance traversed in reaching center.			
	Tests.	Controls.	Difference.	D/P. E.	Tests.	Controls.	Difference.	D/P. E.
Training:								
1st.....	1862.6	1176.7	+686.0±143.3	4.79	1358.3	999.7	+358.6±73.1	4.9
2d.....	334.2	203.4	+130.8± 39.3	3.33	476.3	382.7	+ 93.6±33.6	2.78
3d.....	163.7	101.5	+ 62.2± 15.77	3.94	356.5	269.7	+ 6.8+27.0	3.21
4th.....	110.2	65.5	+ 44.7± 9.84	4.54	284.4	220.1	+ 64.3±15.4	4.2
5th.....	93.1	78.9	+ 14.2± 11.30	1.24	302.7	252.1	+ 50.6±23.9	2.1
6th.....	97.9	83.6	+ 14.3± 11.02	1.30	295.8	241.8	+ 54.0±19.9	2.7
7th.....	90.3	66.0	+ 24.3± 9.21	2.63	263.6	230.3	+ 33.3±19.3	1.7
8th.....	73.3	63.4	+ 9.9± 8.42	1.17	238.2	223.3	+ 14.9±19.5	0.8
Retention:								
1st.....	87.4	92.6	− 5.2± 8.50	0.61	231.9	242.2	− 10.3±14.9	0.7
2d.....	58.8	55.0	+ 3.8± 8.10	0.47	217.0	185.9	+ 14.8±24.2	0.6
3d.....	56.9	38.6	+ 18.3± 6.2	2.90	199.7	165.4	+ 34.3±15.7	2.2
4th.....	65.3	37.8	+ 27.5± 6.91	3.98	224.3	162.8	+ 61.5±15.8	3.9

"The time, distance, and errors criteria agree in showing the following: In all cases the averages indicate a greater capacity for learning the maze by the controls; in the majority of cases these differences are great enough to be considered statistically significant. The exceptions tend to appear in the last part of training and in the retention test. It is suspected that the maze was simple enough to be learned by all the rats, but that the alcoholism of the grandparents of the tests tended to make them slower in learning it; in the first part of the training, the differences are great enough to be significant, while in the last part many of the rats do not continue to improve and the differences in the averages are reduced, but they still lie in the same direction. However, the results subsequently obtained for the preceding (the filial) generation conflict with this explanation, since there the first days of training do not show any clear inferiority of the tests, but in the latter days the differences are statistically significant, favoring the controls.

"The speed of running (centimeters per second) does not give any clear difference between the tests and controls as to the general rate of movement. The males and females in two strains show the speed of the tests less than that of the controls, but in the third strain the speed of the tests is greater than that of the controls. None of these differences is so great that it may not reasonably be due to chance; in no case is the difference as great as three times its probable error. The conclusions to be drawn, then, are that (1) there is no general difference in the nature of the two series of rats as expressed in the rate of their movements; (2) there is a difference that retards the learning of the test rats.

"Perfect trials, or those made without wrong turns, afford a clean-cut method of comparing the tests and controls. When the number of perfect trials made in the total number of trials by each rat was considered, the following results were found (table 2):

TABLE 2.—*Numbers of perfect trials.*

	Tests.	Controls.	Differences.	D/P. E.
Training	1.48	2.76	+1.28±0.40	3.07
Retention	2.58	4.20	+1.62±0.40	4.05

"It is plain that the tests made fewer perfect trials in the given number and that the difference is great enough to be significant. The second use of the perfect trials is to compare the number of trials before the first perfect one was made. The averages obtained were: Tests, 19.48; controls, 13.82; difference, 5.66±1.66; D/P.E., 3.41.

"Here again the tests are inferior, taking more trials before learning to make the first perfect one. In this case also the difference is too great to be due to chance alone. The third criterion based on perfect trials is the time spent in running them; the final averages (in secs.) are as follows: Tests, 8.48; controls, 7.52; difference, −0.95±0.25; D/P.E., 3.84.

"Although there does not seem to be a general difference in the speed of the tests and controls, there does appear to be a significant difference in the time that the two sets required in running their perfect trials, the controls going faster.

"We believe that the above points show that the tests and controls differ as groups in their behavior in the maze. From the standpoint of learning their way to the center and going there for food, the tests are less successful than the controls. The alcoholic treatment of the grandparents is the only basis upon which the rats have been divided into the groups of tests and controls; therefore the alcoholic treatment of the grandparents seems to be responsible for the inferiority of the tests in running the maze."

"*Maze-behavior of the children of alcoholized rats.*—Since the extensive study of the generation described above has shown that the various criteria for the comparison of the tests and controls give closely similar results, it has seemed necessary to use only part of the available criteria for the other rats. Therefore, for the other generations we decided to employ only time and the three criteria dependent on perfect trials, omitting distance, speed, and errors.

"During the summer Miss Charlotte Gilman has summarized the data on the maze-behavior of the children of the alcoholized rats. The parents of the rats considered above were of this generation. Although these results are still subject to certain further checkings, they are presented at this time for comparison with the other generation. It may be stated at once that the com-

parison of the tests and controls in this generation gives, as a whole, the same sort of results as given by the rats in the next generation, namely, the test rats, on the average, do worse than the controls. But instead of being greater, the difference between the tests and controls (although one generation nearer to the actual alcohol) is less marked.

"There are about 100 rats in this generation, from four strains. Three of these strains show a clear preponderance of averages favoring the controls when the trials on each day of training are taken separately; the fourth strain does not show any difference at all between the two groups. The averages for time obtained, when all strains are put together, are shown in table 3; the differences on each day are plus (the tests taking more time), with the single

TABLE 3.—*Averages of time elapsed in reaching center of maze per day of 48 children (tests) of alcoholic parents compared with 45 controls on each day of training on the maze.*

Day.	Tests.	Controls.	Difference.	D/P. E.
Training:				
1st	1906.5	1901.5	+5.9±140.5	0.03
2d	429.6	399.4	+30.2± 58.1	0.52
3d	145.7	152.3	− 6.6± 24.0	0.27
4th	141.4	76.5	+64.9± 17.7	3.66
5th	90.4	61.7	+28.7± 7.4	3.88
6th	64.8	53.1	+11.7± 5.1	2.29
7th	55.9	46.8	+ 9.1± 3.3	2.76
8th	52.6	40.9	+11.7± 3.6	3.25
Memory:				
1st	125.5	113.2	+12.3± 16.5	0.74
2d	66.5	47.7	+18.8± 6.0	3.13
3d	47.3	37.6	+ 9.7± 4.1	1.62
4th	49.5	40.7	+ 8.8± 3.4	2.59

exception of the third day. However, on none of the first three days are the differences great enough to have any significance. For the most part, the differences on the other days do show that the tests are significantly slower in running the maze than are the controls. The criterion of perfect trials gives similar results. The controls made more perfect trials; they required fewer trials before making the first one; and they spent less time in running them.

TABLE 4.—*Comparison of children in respect to perfect trials.*

	Tests.	Controls.	Difference.	D/P. E.
Perfect trials	4.00	7.08	+3.08±0.56	5.50
Trials before first perfect trial	19.40	15.80	+3.60±1.39	2.59
Time spent on perfect trials	7.7	7.1	+0.60±0.17	3.52

It is too soon to make final comparisons between the generations, but the results so far seem to show that two of the three strains that are represented in both generations bear the same relative positions in each generation. Strain C is a fast one in both generations; strain L a slower one; strain A, however, is different in the two generations, being slower in the later generation. This difference in strain A may very possibly be associated with the appearance of a sort of inherited hydrocephalous (*pig-headed*) condition in the later generation.

"A more striking result of the comparison of the two generations is that the differences between the tests and controls are greater in the generation farther away from the alcohol. This is especially the case in the first three days of the training, where the differences are great in the second generation, but in the first generation are practically non-existent. In the later days the actual differences between the tests and controls in the two generations are not very unlike. However, when these differences in the later days are considered, it appears that they are not significant in the second generation, while in the first, due in part to the larger numbers, they are fully significant. Thus there are significant differences in the first part of the training for one generation and in the second part of the training for the other generation, although in both generations all the significant differences are in favor of the controls."

The above indicates that the effect of alcohol was certainly not purely somatic, due to alcohol in the blood of the mother entering the vessels of the fetus, for if this were the case, the grandchildren should show *less* rather than *more* difference between the tests and controls. There is some effect handed down; whether it originated in the germ-cells of the embryos of the offspring of the treated mothers, or in the germ-cells of the parents themselves, can not be decided from the evidence at hand. The only data on this point appear to show that the germ-cells of the fathers alone were not sufficiently modified to affect the offspring. A small group of 5 rats, whose father was treated and whose mother was normal, compared with 7 normals from the same pair of grandparents, do not show any effect of the alcohol treatment; on some days the averages of the test rats are higher, on some days the averages of the control rats are higher, with no preponderance either way. If this result has any significance it may be due to two things: either that the amount of alcohol taken by one parent was not enough to modify the offspring, or else the susceptible age is while the individual is *in utero*. Unfortunately, no further generation was raised from these rats. It appears to be entirely possible that the difference in first days of training in the two generations studied may be due to two different effects of the alcohol treatment; there may be working both (1) a direct somatic effect that does not modify the first part of training, but which tends to make the rats move more slowly in the last part, after the habit is well learned, as well as (2) a germinal effect that does not make itself manifest till the next generation, and then modifies the nervous system rather than the muscular mechanism and hinders the learning process that goes on in the early trials.

"*Maze-behavior of rats from parents and grandparents treated with alcohol.*—Miss Vicari has made summaries of the training records of a set of rats that came after two alcoholized generations. Of these there were 9 tests and 9 controls. When the time data were averaged for each day of training separately, the differences in every case in the original training were plus, that is, the tests took more time. In the trials of the retention test given a month later, the controls took more time on the first and third days; the tests took more time on the second and fourth days. Since the number of rats is so

small, the probable errors are naturally very large; indeed the differences, large as they seem, are more than three times their probable errors on only a few days. These days are at the end of the training period (sixth, seventh, and eighth days) and the last (fourth) day of the retention test. The differences between the averages on the earlier days are much greater, but the variability at this time is so much greater that the significance is much reduced (see table 5).

"The criterion of perfect trials fully supports the findings of the time data. The tests made fewer perfect trials, required more trials before making a perfect trial, and the average of the time spent by the tests in running perfect trials is greater than the average for the controls, yet this difference is not great enough to be significant. Plus signs indicate that the tests took more time.

TABLE 5.—*Averages per day of time elapsed in reaching center of maze by 9 rats (tests) whose parents and grandparents were alcoholized, compared with 9 controls.*

	Tests.	Controls.	Difference.	D/P. E.
Perfect trials	1.16	7.00	+ 5.84± 0.98	5.96
Trials before first perfect trial	33.44	17.0	+ 16.44± 2.03	8.10
Time spent on perfect trials	8.97	7.74	+ 1.23± 1.06	1.16
Training:				
1st day	2000.53	1977.36	+ 23.17±301.50	0.08
2d day	948.64	827.25	+121.39±271.20	0.45
3d day	591.14	367.21	+223.93±148.80	1.50
4th day	449.80	215.97	+233.83± 87.16	2.41
5th day	267.82	128.60	+139.22± 69.13	2.01
6th day	166.27*	68.20*	+ 98.07± 25.91	3.78
7th day	156.04	78.88	+ 77.16± 24.44	3.16
8th day	139.84	62.44	+ 77.40± 25.77	3.00
Memory:				
1st day	121.13	105.94	+ 15.19± 25.31	0.60
2d day	71.53	47.74	+ 13.28± 16.56	0.80
3d day	34.16	39.11	+ 4.05± 4.93	0.82
4th day	130.53	35.22	+ 95.31± 16.97	5.62

*Excluding one abnormal rat.

"If the hypothesis suggested as an explanation of the different results given by the two generations compared above is at all correct, one would expect to find in this case that both the first and later parts of the training gave real differences, since there would be opportunity for both sorts of effect to be manifest—the immediate somatic effect from the mother, and the germinal effect from the grandparents upon the germ-cells of the parents. And the table shows that the differences in the first days are greater than those differences shown above (table 2) in the early days of the training of the first generation, but the significance of the differences resembles the findings in this first generation rather than those in the following generation, namely, the last part of the training gives significant differences and the first part does not. With such a small number of animals this result has no decisive significance, but as far as it indicates anything it goes in the direction that the suggested hypothesis demands."

"*Results of the multiple choice training.*—The apparatus used in this training consisted of a series of nine compartments, each of which had front and back doors, operated at a distance by the observer; different sets of these front doors were opened at different trials and the rat was given its reward of

food when it entered the correct compartment (at the extreme right or left, as the problem might be—the 'end-compartment'). The steps in the training were these: Preliminary training, when the regular series of doors was opened in successive trials, but the rat was fed upon entering any compartment (2 days, 20 trials); right-hand problem, when the rat was fed only when it entered the open compartment at the extreme right (10 days, 100 trials); the same problem with a different series of doors open (2 days, 20 trials); left-hand problem, when food could be obtained only upon entering the open compartment at the extreme left (10 days, 100 trials); same problem with a different series of doors open (2 days, 20 trials); after a month, the same problem, with the same series of doors open as in the last step, to test retention (4 days 40 trials). Previously (Year Book 1919, 126) only the numbers of correct first choices and the number of wrong choices for the different parts of training were summarized, involving no comparison with the preliminary training, and no comparison was made of the different days in the same step of training to show the progress of learning. For the consideration of the progress of learning the trials have been grouped by twenties; that is, two days are taken at a time. The end-compartments seem to be especially attractive, irrespective of training, since about 70 per cent of the first choices are end-compartments through all the training, although only 57 per cent of the open compartments are end-compartments. The new summaries are based on the numbers of left end-compartments, and the numbers of right end-compartments that were chosen first in every 20 trials of the whole training; besides these, there have been studied the numbers of wrong choices, including end-compartments in the same sets of trials. There were 21 test rats used and also 21 controls.

"Figure 1 shows the percentage of the trials on which right and left end-compartments were respectively chosen first, for each of the sets of 20 trials in the whole training. The percentage for the test rats is shown by the broken lines, for the control rats by the solid lines. Trials on which the right end-compartment was chosen first are indicated by the heavy lines (both broken and solid) and trials on which the left end-compartment was chosen first are shown by the light lines.

"The right-hand first choices (heavy lines in figure 1) will first be considered. Both the tests and controls increased the percentage of right end-compartments in the right-hand problem. For the 5 points in the curve showing the regular right-hand training the tests have fewer right-hand choices, but when the different series of open compartments was given on the eleventh and twelfth days, the tests made more right-hand first choices than the controls. The general superiority of the control percentages disappears when it is observed that the tests started out in their preliminary training with fewer right-hand choices than the controls. If the slant of the curves is compared, it appears that the tests made more rapid and greater progress. On the other hand, it may not rightly be claimed that the tests were actually better than the controls, since the higher initial right-hand tendency of the controls may have in itself limited the opportunity for rapid improvement. The highest proportion of correct choices reached during the 120 trials of this problem is little over half of the trials. In the left-hand problem the number of right-hand first choices is immediately reduced in the first 20 trials. The tests and controls are both lower than in their preliminary trials. In the following sets of 20 trials there is no further reduction in the percentage of right-hand choices; the controls remain about the same, but the tests chose more right-hand doors than the controls, although the initial tendency of the tests was to choose fewer right-hand doors.

"The left-hand first choices (light lines in figure 1) will be considered. In the right-hand problem the tests and controls started with about equal left-end choices, and reduced this number at about the same rate. In the first 20 trials of the left-hand problem, when the number of right-hand end-compartments chosen first was suddenly reduced, the number of left-hand end-compartments chosen first was suddenly increased. But after this original increase, instead of showing still further improvement, the following set of 20 trials actually lowered the proportion of left-hand first choices; that is, the continued training seems to have the opposite from the expected effect. The tests (broken line) made fewer left-hand choices than the controls, but the same tendency to go against the direction of the training is obvious in both sets of rats.

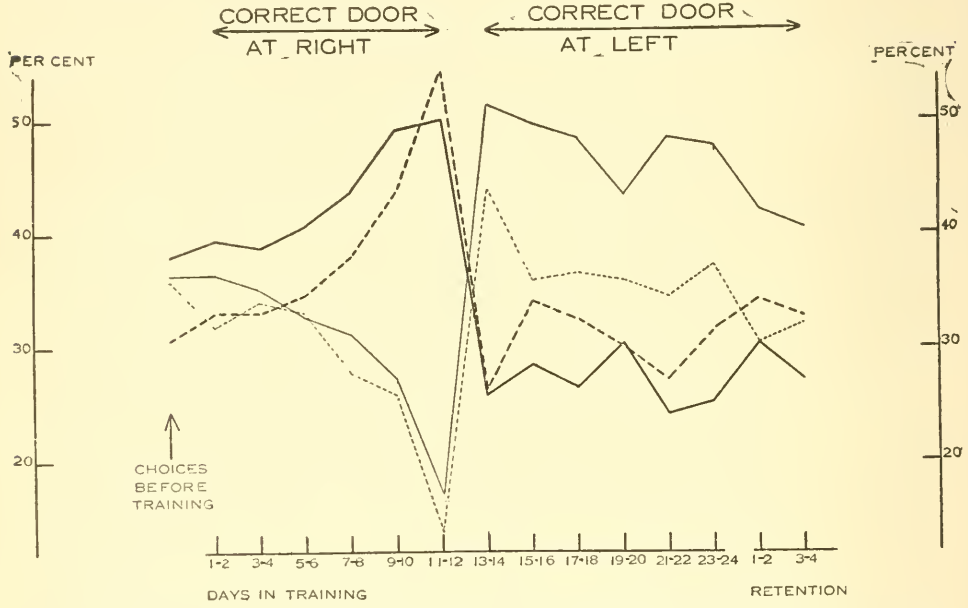

FIG. 1.—Showing the percentage of right-hand and left-hand end-compartments that were chosen first in each successive group of twenty trials per rat. The heavy lines show right-hand end-compartments chosen first, the light lines show left-hand compartments chosen first; solid lines are the controls, broken lines the tests. After the twelfth day of training the correct door was made the one on the left end, instead of the one on the right end.

"The retention trials show the tests making about as many right-hand and left-hand choices as in their preliminary trials, although the number of right-hand choices was larger in spite of the 120 trials immediately preceding, when food was given only after a left-hand compartment was chosen. The controls, on the other hand, show very plainly the retention of the tendency to go to the left end-compartment more frequently than the right end-compartment.

"When the curves like the above were drawn for each rat separately, 5 controls and 8 tests were found that did not seem to learn at all. If these rats are omitted from the summaries, the curves present the same general situations, but an exception appears in that the tests in the left-hand problem make fewer right-hand choices than the controls, instead of more, and there is less difference in this problem between the numbers of left doors chosen by the tests and controls. In other words, the elimination of these rats reduces

whatever differences previously existed between the tests and controls. More test rats failed to show signs of learning, but those that did tend to profit by experience appeared to have no handicap as compared with the controls.

"The curves for the third criterion, the numbers of wrong choices made, offer the following observations: The test and the control rats continuously reduced the numbers of wrong choices in the right-hand problem, but the controls made fewer wrong choices. Considering the stronger right-hand tendency with which the controls started out, this superiority can not be held to prove greater ability in learning or in adaptability. The rate of elimination of wrong compartments is much the same for the tests and controls.

"The tests and controls both made more errors in the first 20 trials of the left-hand training than they did in the beginning of the first problem; this is evidence that there was real learning in the first problem which interfered with the learning of the opposite problem. The second set of 20 trials in the left-hand problem shows an abrupt reduction in the number of errors, but in the following sets of trials errors are eliminated much more slowly. During this time the controls have made more progress than the tests. In this problem the fewer errors made by the controls can not be explained by their original tendency, nor can it be due to any differential effect of the right-hand training, since both tests and controls were doing about the same at the end of that training.

"Although the numbers of right and left hand end-compartments that were chosen first in the left-hand problem do not indicate any improvement after the first set of 20 trials, the number of wrong choices does seem to be reduced. There are fewer correct choices, but also fewer wrong choices. This is mainly due to the elimination of repeated choices of the same door that had been a correct door in the right-hand problem.

"For the general failure of the training in the second problem, there does not appear to be any obvious explanation. Learning of this type is possible; it is demonstrated in the right-hand problem. The failure is not due to the chance effect of combining rats which individually show very different results, for only 4 rats showed anything that might be considered signs of learning the left-hand problem.

"The observations just made may be summarized as follows: Considering the end-compartments that were chosen first, it appeared that (1) both tests and controls increased the numbers of right-hand choices and decreased the numbers of left-hand choices when food was given only in the right end-compartment, whichever one that happened to be; (2) that both tests and controls reversed the preponderance of their choices from right end-doors to left end-doors in the first 20 trials after food was given only in the left end-compartment; but after the first 20 trials there was no further improvement in the direction of training, but rather a tendency for the numbers of left-hand choices to fall off and the numbers of right-hand choices to remain the same; (3) that in the first problem the test may have a little advantage, as far as there is any difference at all; in the second problem the controls appear to show a greater degree of adaptability. Comparing the preliminary training with the retention tests, the controls appear to have been more lastingly modified than the tests by the training; but when certain rats that did not appear to show any signs of learning at all were eliminated (5 controls and 8 tests), the advantage in favor of the controls in their greater adaptability in the left-hand problem no longer is found.

"There remains one valuable source of information that has hardly been touched, namely, the study of the methods employed by individual rats in meeting the situations presented, quite apart from their success as tested by

the numbers of doors of different kinds that they entered. The data, as tabulated, entirely obscure these reaction tendencies, yet these would give a different and possibly fairer test of the rats than is afforded by their relation to an arbitrarily established standard. The graphic records of each individual trial provide this information; from the record sheets it will be possible to classify the successful types of reactions on an absolute basis; how consistently the different types of reaction were shown; how sensitive the animal to extraneous circumstances, to the operation of the apparatus; thus, a study of the general motor tendencies and many other exceedingly interesting and important side-lights will be afforded.

"That the maze and the multiple-choice apparatus do not give the same results when the tests and controls are compared does not weaken the significance of the conclusions in either case. The problems are of a different nature and require different mental processes for their solution; the rats solved the one with ease, the other was not solved in the number of trials allowed. Longer training would have given greater success and probably mastery of at least one of the multiple-choice problems. It is entirely possible that the final perfecting of the solution would bring out differences between the tests and controls, however much alike their rate of learning in the beginning. On the other hand, if the performance in the early part of the learning process should be a true sample of the whole process, it is equally simple to suppose that the alcohol may have modified the nervous mechanism involved in learning the maze and have had no influence at all upon the processes that are involved in the solution of the multiple-choice problem. So the conclusion stands that the maze brings out differences between the tests and controls, and the multiple-choice apparatus, as far as the training went, did not bring out these or other differences."

SIGNIFICANCE AND CONTROL OF SEX.

COMPARATIVE METABOLISM OF SEXES IN PIGEONS.

Dr. Riddle has extended his studies on the metabolic differences between the eggs that give rise to the two sexes to a study of the differences in the metabolism of male and female embryos. He found it difficult to devise a satisfactory method of measuring these differences but finally adopted the following: He undertook to subject, during an entire year, all, or practically all, of the embryos produced by the ring-doves and common pigeons of our collection to reduced and to increased concentrations of oxygen, or to expose them to protracted periods of cold, and to observe the relation of sex to survival under these conditions.

Theoretically, if female embryos have a lower metabolism than male, the female embryos should withstand diminished pressures of oxygen better than male embryos. Similarly, since it had been earlier learned that high pressures of oxygen result in the death of some embryos, the male embryos should be somewhat better able than female embryos to withstand an increased concentration of oxygen. Again, if males have a higher metabolism than females, the reduced metabolism induced by cooling should prove more harmful to the male embryos.

Embryos aged 3 minutes (after laying) to 12 days were used; and most frequently the age was between 1 hour and 4 days. Increased

concentrations of oxygen varying from 26.8 per cent to 96.6 per cent and decreased concentrations varying from 18.3 per cent to 0.15 per cent have been used. The time during which embryos were subjected to the altered pressures of oxygen has varied from 1 to 5 days. For 0.15 per cent O_2 the time was 15 minutes to 8 hours.

During treatment the embryos were kept in a sealed chamber (a modified desiccator) into which prepared washed gas of known concentration was continually fed and then led away. Two to five analyses were made daily of gas samples drawn from the chamber. Before and after treatment in the chamber (kept at 103.0° F. in a Freas oven) the embryos were incubated by doves, either by the parents or by generic hybrid doves maintained for this purpose.

The age of the embryo has been found the most important factor in survival under alteration of the gaseous environment. Older embryos are most affected by reduced pressures of O_2; younger embryos most affected by increased pressures of O_2. It is probable, but not now certain, that the adequacy of thickness of the shell is also a factor in such survival. Probably the egg-shell normally acts as a buffer against the oxygen of the air. This adequacy of the shell has been painstakingly measured by Dr. Riddle in all of the treated embryos. A small number of embryos has been simultaneously treated with increased pressures of oxygen and carbon dioxide. From 8.0 per cent to 46.0 per cent of CO_2 have been employed. In the embryo these two substances doubtless have in part antagonistic effects. Table 4 presents the chief data obtained concerning sex. It will be understood that the sex of many embryos which were killed, and even of some which survived treatment but died later, could not be ascertained. The results thus far obtained, with the year three-fourths completed, indicate that sex is also a factor in survival.

TABLE 6.—*Comparison of effect of variations of percentage of oxygen in the atmosphere on male and female embyro pigeons.*

Nature of treatment.	No. of embryos treated.	Embryos.		Sexes.		
		Killed.	Survived treatment.	Killed.	Survived.	Total.
				♂ : ♀	♂ : ♀	♂ : ♀
Increased O_2	596	247	345	8 : 11	173 : 121	181 : 132
Decreased O_2	536	264	258	35 : 19	100 : 127	135 : 146
Increased O_2+CO_2	146	109	35	1 : 3	17 : 13	18 : 16
Cooling	297	155	139	30 : 18	62 : 62	92 : 80

The tabulated data show that fewer males than females were killed by increased pressures of O_2, and that more males survived this treatment. More males were killed by decreased pressures of O_2, and

fewer survived this treatment; also, when subjected to cooling, more males were killed and fewer (in proportion to total) survived. In all these respects Dr. Riddle is convinced that the metabolic theory of sex is supported. The pigeons, therefore, have now supplied cogent evidence of fundamental, metabolic sexual difference in their germinal, embryonic, and adult stages.

MODIFICATION OF THE SEX-RATIO IN MAN.

The standard sex-ratio may be considered to be 100 males to 100 females. In some species where the ratio deviates far from 100, a special explanation is demanded and has sometimes been received. The sex-ratio in man is usually over 100—not far from 105—and this deviation is doubtless due either to the fact that male-producing sperm have a better chance of fertilizing the egg than female-producing sperm, or else that the male embryo (zygote) is more viable, on the average, than the female embryo (zygote). Dr. Little is paying special attention to the human sex-ratio, and his work was reported on in the Year Book for 1919 (pp. 135–137). It was there pointed out that the sex-ratio is greater when the parents belong to different European races than when they belong to the same, as 122 is to 106. It now appears that when both parents are whites born in the United States the ratio is high (118), which we might expect in view of the hybrid nature of our white population. Studies on the sex-ratio in the colored population yield some new and unexpected facts. The sex-ratio of the offspring of colored parents born in the United States is exceptionally low (96). Of offspring of colored parents, born in the British West Indies, and probably *less* hybrid than the progeny of colored persons born in the United States, the sex-ratio is 108. If we may regard the West Indians as less hybrid, then on the basis of the findings in Europeans we should expect a smaller ratio than in offspring of colored Americans. While in white primapara the sex-ratio is higher in first births than in offspring of subsequent births as 115.5 ± 1.5 is to 97.3 ± 1.2, in the colored population the sex-ratio is the lower in first births as 103.6 ± 2.8 is to 112.0 ± 2.8.

SEX-LINKED LETHAL FACTORS IN MICE.

One important cause of disturbance of the sex-ratio is the presence of some lethal factor in, or the absence of some vital factor from, the sex-chromosome. Dr. Little has found the sex-ratio of inbred, non-waltzing mice to be 103.1 ± 2.8, giving the usual slight excess of males. The sex-ratio of litters from a closely inbred race of Japanese waltzing mice is 53.2 ± 5.7. The difference between the two sex-ratios is 7.9 times its probable error, so it is certainly significant. Reciprocal crosses of animals from this particular strain of inbred waltzing mice with non-waltzing races give extremely interesting and distinct results.

Thus Japanese waltzing females crossed with non-waltzing males give a sex-ratio of 44.0 ± 7.4, while Japanese waltzing males crossed with non-waltzing females give a sex-ratio of 118.2 ± 3.8. The latter result is commonly obtained in hybrid combinations, but the former represents a departure from the normal type requiring explanation.

Dr. Little advances the following hypothesis as that which best fits all of the observed experimental facts. In some of the females of the inbred Japanese waltzing race there is a recessive lethal factor which is sex-linked. Such females would transmit the lethal factor to one-half their male progeny. Males of this sort, having the lethal in an unbalanced condition, would not survive, thus producing a sex-ratio of 1 male to 2 females or 50.0 in the progeny of such females. All surviving males would, by hypothesis, lack the lethal, and therefore could not transmit any peculiarity of the sex-ratio to their progeny. The result of the cross (Japanese waltzing male by non-waltzing female) is thus explained. One-half of the female progeny of the lethal-bearing females should theoretically be homozygous normals and one-half should transmit the lethal. In more advanced hybrid generations descended through Japanese waltzing females an excess of females should thus be produced. The exact ratio would depend upon the proportion of lethal-bearing to homozygous normal females in the population. Actually such an excess of females has been obtained. The sex-ratio of advanced hybrid progeny descended through the Japanese waltzing female line is 78.7 ± 2.9.

The size of litter also affords supporting evidence for the presence of a lethal. Frequency polygons for litter-size in Japanese waltzing and in non-waltzing females have been compared by a χ^2 test. The 58 litters from Japanese waltzing females average 3.38 young per litter, while those from non-waltzing females (100) average 5.93. The odds are greater than 1 in 100,000 against the distribution curves being the same. Since Japanese waltzing females should be of two general types, (*a*) those producing small litters, due to the lethal, and (*b*) those producing larger litters when free from the lethals, the result falls in line with the sex-ratio evidence.

It seems clear, therefore, that certain females of the closely inbred Japanese waltzing-mouse race are transmitting a recessive, sex-linked, lethal factor. This is, it is believed, the first case of a sex-linked lethal in mammals and the first case of sex-linkage in rodents.

DIFFERENCES IN RESISTANCE OF SPERM OF DIFFERENT SPECIES TO ACID SOLUTIONS OF VARIOUS STRENGTHS, IN RELATION TO THE SEX-RATIO.

In the annual report of the Institution for 1919 the hypothesis was stated that certain peculiarities in the sex-ratio might be due to variations in the quality of the internal secretions of the female reproductive

tract at different times. Also, it was suggested that these secretions may act in a different degree upon the male-forming and upon the female-forming sperm. The female-forming sperm with its larger amount of functional chromatin offers more opportunity to produce physiological incompatibilities and therefore to be eliminated than the smaller male-forming sperm.

It has long been known that the hyperacidity of the vaginal secretions is a powerful factor in producing sterility. Evidence also exists that before pregnancy the os of the uterus is so small that there is little, if any, opportunity for the slightly alkaline secretions of the uterus to mingle with and neutralize the acid vaginal secretions. After the first birth the os is often enlarged or torn and more chance for admixture of the secretions exists. Since acidity is known to be harmful to sperm, and since the selective and eliminating power of the vaginal secretions might, by alterations of its acidity, be changed, it seemed of interest to determine what was the behavior of sperm in various concentrations of acid. For this purpose glacial acetic acid was chosen. The work was planned and carried out by Dr. Little with the assistance of Miss Marion Gibbons.

The results are tabulated below. Control drops were used on the same microscope slides with the treated drops. Each test was repeated three times. The sperm was kept in warm Ringer's solution. The different reactions of the various sperms are striking.

TABLE 7.—*Reactions of spermatozoa of various mammals to acid solutions.*

Ratio of glacial acetic acid to water.	Type of sperm used.	Results.
1 : 6,400	Mouse....	Kills all sperm immediately.
1 : 11,200	Mouse....	Greater part of sperm move normally; few stop.
1 : 12,800	Mouse....	Sperm unaffected.
1 : 9,600	Rabbit....	Kills all sperm immediately.
1 : 12,000	Rabbit....	Few sperm stop; greater part are unaffected.
1 : 12,800	Rabbit....	Apparently all sperm live.
1 : 24,000	Rat.....	All sperm stop immediately.
1 : 66,000	Rat.....	Over 50 per cent of the sperm stop.
1 : 70,000	Rat.....	Very few sperm stop; greater part move normally.
1 : 40,000	Dog.....	All stop immediately.
1 : 210,000	Dog.....	Slow down greatly and eventually most stop.
1 : 300,000	Dog.....	Some stop, but the majority go on normally.

Since the mouse and rat sperm-cells are easily distinguishable in size, a mixture of the two was made and treated with solutions of acetic acid as follows:

TABLE 8.—*Reactions of mixtures of mouse and rat sperm-cells to acid solutions.*

Ratio of glacial acetic acid to water.	Type of sperm used.	Results.
1 : 11,200	Mixture of mouse and rat	All motion of rat sperm stops; very few mouse sperm stop; greater part move normally.
1 : 12,800	.....Do.....	All rat sperm stop immediately; mouse sperm unaffected.
1 : 68,000	.....Do.....	Very few of rat sperm stop; greater part of rat sperm and *all* mouse sperm move normally.

From the above data it may be concluded that there is differential mortality in the sperm-cells of different species when subjected to weak solutions of acetic acid.

SELECTION OF SEX INTERGRADES IN DAPHNIA.

As noted in last year's report, Dr. Banta has undertaken to increase or decrease the amount of a sex-intergrade condition that he discovered in the water-flea, *Daphnia*, by means of selection based on somatic differences. At that time it was thought that it could be concluded that strains like No. I, selected for increased intergradeness, would probably not become more intergrade; but strains like Nos. III and V, selected to produce normal females, could tend to become so. All mothers received identical treatment so far as practicable—culture water was taken from the same jar and the temperature was maintained in each at the same level. By means of a somewhat arbitrary scale, running from 0 to 80, the degree of intergradeness may be expressed quantitatively. It is to be recalled that all strains descended parthenogenetically from the same progenitor.

The procedure and the results with intergrade strains I and III were as follows: Strain I was maintained as a "high" intergrade strain. Its level fluctuated from 10 to 33, averaging about 22 on the scale of intergradeness. Strain III was selected as a "low" strain. Selection was effective in the seventh generation, the level falling to about 3. After five further generations of selection the level fell to about 1. In the nineteenth generation there was an abrupt rise in this low strain to 20, after which, during four generations of selection, it fell to 0, producing none or very few slight intergrades for the next five generations. Later, it again went to somewhat higher levels. However, in spite of all the fluctuations in both the high (I) and the low (III) strains after the sixth generation a pronounced divergence between the two strains was maintained (fig. 2).

In the nineteenth generation a return selection was begun in the low strain (III) to produce a high strain. Unfortunately this was begun when Strain III was at a rather high level, but the result was the same in Strain V, another low strain, which was at a low level when return

selection was begun. The effect was pronounced. In two generations the new high strain (X) attained even a higher level than the original high strain and maintained a level as high as that for Strain I.

In the twenty-ninth generation a low selection was begun in Strain I. The result was the immediate production of a low strain (XII, see fig. 2). At the same time a low selection was begun in Strain X, the high strain, which was itself a return selection from Strain III. A low strain (XIV) was obtained in the second generation of selection. These selection experiments are represented diagrammatically in the figure, the abscissas representing generations of descent and the ordinates degrees of intergradeness.

Seven other intergrade strains of *Daphnia longispina* used in similar selection experiments gave similar results, so that one seems warranted in concluding that in this sex-intergrade stock selection and return selection are equally effective and that a strain may be raised or lowered in the scale of intergradeness at will by means of "selection."

FIG. 2.—Curves showing diagrammatically some of the selection experiments with sex intergrade strains of *Daphnia longispina*—Strains I, III, X, XII, and XIV. The abscissas represent generations of selection, the ordinates degrees of intergradedness. Roman numerals indicate the strain numbers while the courses of the curves indicate which were selected as high and which as low strains.

The interpretation of the results of "selection" in *Entomostraca* is not entirely clear, since there are numerous elements of the problem that are unknown. To these principles we hold fast: Selection in a proper sense selects only what is given; it does not create. Also, any inherited consequences of the selection are carried in the constitution of the gametes, either chromosomes or cytoplasm. If, then, "through selection," a parthenogenetic race has been genetically modified, it is because genetic idiosyncrasies in the desired direction have been afforded. If a rapid change has been possible through selection it is because individuals showing somatic variations carry corresponding determiners for such in their germ-cells. Thus, in making selection of visible, somatic hereditary peculiarities we are, at the same time, selecting corresponding, but invisible, gametic determiners for such hereditary peculiarities.

Just what the nature of the hereditary change in the gametes is, that the selector takes advantage of in his successful selections, is uncertain.

It may be a cytological modification, but in the great majority of cases it is chromosomal. It has been urged that, when chromosomal, it may be due to a change in the nature of a gene without a change in its identity. But a change in a gene is, for the geneticist, a new gene, whether it occupies the place of a pre-existing gene or not. It troubles some experimentalists that, through selection, a change in the race can be so soon effective; but the change had already occurred before they began their effective selection; they were merely working with a germinal change that had occurred before the phenotypic change which they were "selecting." In a species or biotype that is invariable we may be surprised if we make progress in a given direction; but in a biotype that is already showing mutations we may expect further mutations in every generation. Of course, size of the phenotypic change induced in the mutation is never to the point; sometimes two or more changes in genes induce no visible, phenotypic change.

SEX IN MUCORS.

Work on the sexuality of the mucors has been continued by Dr. Blakeslee. He has accumulated a considerable body of new data on the results of testing the sexual interactions of dicœcious species. In 15 different species, representing 9 different genera, over 1,600 individual races have been tested in pairs for the production of zygospores. Nearly 10,000 combinations have thus been made, with the result that each race tested has been shown by the presence or absence of zygospore formation to belong to the plus (+) or minus (−) sex or to show no sexual reaction in any combination. No sex intergrades have been discovered. Different races have different strengths of sexual vigor, as shown by the intensities of zygospore formation. The "neutral" races, which have not as yet shown zygospores in any combination, may be the low extremes of a graded series into which the sexually active races can be arranged, based on the abundance of zygospores which they form in different combinations. We have just completed a series of tests of sexual reactions between races of different species and have thus made a total of over 2,000 combinations. "Imperfect hybridization" has taken place only when the races grown in contact belonged to the opposite sexes, minus (−) and plus (+). The "imperfect hybridization" work confirms the conclusion drawn from the tests between races of the same species, viz, that a stricter sexual dimorphism is present in the dicœcious mucors than in dicœcious species of higher plants. It is suggested that this difference may be connected with the fact that in the mucors we are dealing with gametophytes, while in higher plants we are dealing with sporophytes.

Dr. Blakeslee has pointed out, in his recent vice-presidential address before Section G of the American Association for the Advancement of Science, that as a criterion of sex the relative size of the two uniting gametes (the larger being called female and the smaller male) does not

lay hold of the fundamental differences between the two sexes. He concludes that the "plus" races of mucors may be homologous in some cases with what has been called male and in others with what has been called female.

INHERITANCE OF GERMINAL PECULIARITIES.

FLOWERING PLANTS.

Portulaca.—In the breeding of this species Dr. Blakeslee has obtained additional data as to the factors responsible for its color types. One of the most interesting properties of the portulacas is their ability to undergo vegetative mutations. Dr. Blakeslee has a paper in press describing the dominant vegetative mutations to normal habit of growth that occur in recessive dwarfs, previously described in the Year Book. Color characters also arise as dominant vegetative mutations. Last year, in a line which for several generations had borne white flowers and which had never shown red pigment in any of its parts, a single plant was discovered one branch of which was slightly red and bore white flowers with pink filaments. The two types of flowers were selfed individually. Seeds from the normal white flowers have this year given uniform pedigrees with white flowers, while seeds from the flowers with pink stamens have given pedigrees with yellows and whites in a 3 : 1 ratio. The pink-stamened white flowers therefore bred like a heterozygous yellow.

In another white-flowered pedigree, a plant was found which, in addition to white flowers, bore some purple flowers and some flowers the petals of which showed a centrally located "fan" of pale purple. Seeds from the white flowers of this plant have bred like whites of this line; seeds from the purple flowers have segregated for purple and white, while seeds from the "fans" have bred like the heterozygous purples.

In a third line there have been found certain purple flowers breeding like whites. The genetics of these three cases becomes intelligible, Dr. Blakeslee concludes, if we consider them examples of periclinal chimeras. Pigment in the petals is located almost exclusively in the epidermal layers, while it is the subepidermal layers which take part in the formation of the germinal tissue. In the first example, a white epidermis covered a subepidermal tissue which carried factors for yellow. In the second example, in which the purple flowers and white flowers with pale purple "fans" bred alike, the "fan" flowers had a white epidermis with a subepidermal layer carrying the factors for purple, and therefore bred like a purple. In the third example, where a purple flower bred like a white, the epidermal layer alone apparently carried pigment and the subepidermal layers were devoid of factors for purple pigmentation. The histological evidence so far obtained is in accord with this conception. "Fan" flowers are devoid of purple in the epidermis, but contain purple pigment in the cells sheathing the bundles. Moreover, some purple flowers have been found with purple

in both epidermal and subepidermal layers, while in other purples the color seems confined to the epidermis. The occurrence of sectorial and periclinal chimeras and mutative stripes and spotting render the portulacas a favorable species for the study of vegetative mutations.

Datura.—In the jimson-weed, work has been brought to a close on the graft-infectious disease "quercina," which causes profound morphological changes in the plants affected; and the results of the investigations are embodied in a paper now in press. It is suggested that the non-Mendelian behavior of "rogues" in culinary peas may be due to a similar type of disease.

The investigation of the mutants obtained from the jimson weed has been continued by Dr. Blakeslee, who reports as follows:

"Most intensive work has been carried on with the 'Globe' mutant, since this mutant can readily be recognized in the seed pans, but the same mode of inheritance seems to be characteristic of all the mutants of this type. The mutant character is transmitted to about one-quarter of its offspring, whether the female parent is fertilized by its own or by foreign pollen. The mutant character is transmitted not at all or but slightly through the pollen. Tests made last fall and winter showed that all the normal races have good pollen, with less than 5 per cent of the grains defective, while all the mutants of the globe type have a relatively high percentage of bad pollen-grains. The mutant 'New Species,' which differs from other mutants in breeding true and being largely sterile with other lines, resembles normals in that its pollen is relatively good. This mutant is one in which abnormal color ratios had been observed. It was believed that chromosome relations might furnish a clue to the abnormal behavior of the mutants in this species. Mr. John Belling has cooperated in a cytological study of our mutant variants. The work is still in progress, but some definite results have already been obtained.

"The 'New Species' turns out to be tetraploid, having 24 in contrast to 12 pairs of chromosomes, characteristic of normal races. Sufficient breeding work with this form has already been carried out to settle certain questions in regard to the behavior of the tetraploid chromosomes at the formation of gametes. Independent assortment of the chromosomes is demanded by the breeding results. Starting with a tetraploid plant that is heterozygous for a factor A and represented by the formula AA′aa′, we expect its gametes to be AA′, Aa, Aa′, A′a, A′a′, aa′, if the chromosomes assort independently. If we disregard the primes and sum the types we have 1 AA + 4 Aa + 1 aa as the formula for both the male and female gametes produced by a tetraploid plant of the formula AA′aa′. Selfing such a plant, one should obtain the following: 1 AAAA + 8 AAAa + 18 AAaa + 8 Aaaa + 1 aaaa, or a ratio of dominants to recessives of 35 : 1. The AAAA plants should always breed true; the AAAa plants when selfed or back-crossed to recessives should give only dominants, but in later generations should give 35 : 1 ratios; the AAaa plants when selfed should again give 35 : 1 ratios and when back-crossed to recessives should give 5 : 1 ratios; the Aaaa plants when selfed should give 3 : 1 ratios and 1 : 1 ratios when back-crossed; and in later generations some 35 : 1 ratios should be expected from selfing individual plants of a 3 : 1 pedigree. By using the purple color of stem, which is dominant to green stem, we have been able to take records from the seed pans and in consequence have obtained relatively large numbers in our pedigrees. The results show that the inheritance of the purple color of stem in our tetraploid jimson is in accord with the theoretical expectation already described for independent assortment of chromosomes. Last year a tetraploid plant arose

in a pedigree heterozygous for spines and node number as well as for color. Although detailed field records have not yet been taken, the segregation of these other factors appears to conform to the expectation made out for the purple stem-color. Triple recessives have not appeared, but the theoretical chances of their occurrence are only 1 in 46,456 individuals in this generation. They should be easily obtained another year.

"So far as has been determined, the *Datura* mutants of the 'Globe' type seem to be due to the presence of an extra chromosome giving 25 instead of 24 chromosomes in the somatic cells. The gametes therefore would have 12 and 13 chromosomes. There appears to be some evidence that the extra chromosome is a specific one in each case. Apparently we have been able to identify in *Poinsettia* the mutant which has as an extra chromosome—the one carrying the determiners for purple and green stem-color. Five *Poinsettia* plants, heterozygous for purple and green, have, when selfed, produced pedigrees of which all the *Poinsettia* mutants are purple and the normals show purple and white in a ratio closely approximating 8 : 1. If these parent *Poinsettia* plants be considered to have the formula PPp, their female gametes should be P+Pp+P+Pp+p+PP or 2P+2Pp+p+PP. The male gametes represented by the pollen-grains should be the same, but, since the mutant character generally fails to be carried by the pollen, the effective male gametes may be considered to be 2P+p. Selfing *Poinsettia* plants of the formula PPp should therefore give all the *Poinsettia* offspring purple and 8 purples to 1 green among the normals, a result that was actually obtained. The matter is being tested further with this particular mutant and an effort is being made to discover which of the remaining mutants have as their extra chromosomes those carrying factors for known Mendelian characters. It seems inadvisable to report further than in a very tentative way on the Globe type of mutations in *Datura*, since the present stage of the work would not warrant any definite conclusions. It is obvious, however, that if the facts turn out to be what the present findings indicate, they may furnish the clue to much which has been unintelligible in this and certain other species."

Rudbeckia.—The breeding work on *Rudbeckia* has been discontinued by Dr. Blakeslee on account of the difficulties of technique involved in crossing them, and their decrease in vigor when inbred. The work on the inheritance of the two yellow cones in this species and their identification by chemical means has been written up and is now in press.

HEREDITY OF SUSCEPTIBILITY TO CANCER.

The importance to mankind of a knowledge of the hereditary factors in cancer can hardly be overestimated, and the theoretical interest of an analysis of the behavior of these factors is no less great. Dr. Little reports on his results in this field as follows:

"*Genetical factors involved in susceptibility.*—In 1916, Little, in collaboration with Tyzzer, reported on the inheritance of susceptibility to a transplantable carcinoma (J. w. A.) of the Japanese waltzing mouse. This tumor grew in 100 per cent of the Japanese waltzing mice inoculated, and in 0 per cent of the common non-waltzing mice. When these two races were crossed, the F_1 generation hybrid showed 61 out of 62 mice to be susceptible. The F_2 generation gave a very interesting result—only 3 out of 183 mice grew the tumor. At that time the results were explained on the basis of multiple Mendelizing

factors whose number was estimated at from 12 to 14. *Simultaneous presence* of these factors, themselves introduced by the Japanese waltzing race, was considered necessary for progressive growth of the tumor.

"Later, while working with a transplantable sarcoma (J. w. B.) of the Japanese waltzing mouse, results were obtained which showed what seemed to be a somewhat simpler quantitative condition of the same process. In this case the parent races and F_1 hybrids behaved as before, but the F_2 hybrids gave a total of 23 susceptible to 66 non-susceptible animals. It was previously estimated that from 5 to 7 factors were involved. In order to determine more closely the number of factors, new experiments were devised, as follows: F_1 hybrid mice, themselves susceptible, were crossed back with the non-susceptible parent race. This has during the past year given a back-cross generation whose susceptibility would depend upon the factors introduced through the gametes received from their F_1 parent. If one factor was involved, the ratio of gametes containing it formed by the F_1 animal to those lacking it would be 1:1; if two factors, 1:3; if three factors, 1:7; if four factors, 1:15; if five factors, 1:31; if six factors, 1:63; and if seven factors, 1:127. Susceptible to non-susceptible *individuals* would occur in the back-cross generation in similar proportions.

"The actual numbers obtained were 21 susceptible to 208 non-susceptible. This result may be compared with expectations on three, four, five, and seven factor hypotheses, as shown in table 9.

TABLE 9.—*Comparison of observed and various theoretical ratios of susceptibility and non-susceptibility in back-cross.*

	Susceptible.	Non-susceptible.	Ratio.
Expected 3-factor	28	201	1:7
Observed	21	208	1:99
Expected 4-factor	14	215	1:15
Expected 5-factor	7	222	1:31
Expected 7-factor	1.8	227.2	1:127

"The observed figures fall between the three and four factor hypothesis. The numbers are not large enough to give a definite test, but the F_2 generation already mentioned is interesting as a supporting line of evidence. If we compare this with the expectation, we find that the observed figures lie between the four and five factor hypotheses (table 10).

TABLE 10.—*Comparison of observed and various theoretical ratios in F_2.*

	Susceptible.	Non-susceptible.	Ratio.
Expected 3-factor	39	50	1:1.3
Expected 4-factor	29	60	1:2.1
Observed	23	66	1:2.8
Expected 5-factor	21	68	1:3.2

"In both cases the four-factor hypothesis figures are close and the three and five factor hypotheses are to be still considered as possibilities, though not probabilities. The six and seven factor hypotheses appear to be definitely eliminated.

"The sex chromosome has been eliminated as a probable carrier of any of the four factors, as follows: If mice, like other mammals, have the female XX and the male XY in formula, the use of susceptible Japanese waltzing males to form the F_1 animals used gives daughters carrying his X and sons his Y chromosomes. If, now, his *sons* only are used to produce the back-cross generation by mating with common non-susceptible females, all the chromosomes in the resulting animals will be derived from common non-susceptible mice and the male offspring would be all non-susceptible; but such is not the case. Unless, therefore, crossing-over between the X and Y chromosomes occurs frequently, any susceptibility factor borne in the X chromosome of the original Japanese waltzing males used has been eliminated.

"While further investigations are in progress, we may conclude provisionally that:

"(1) From three to five factors, probably four, are involved in determining susceptibility to the mouse sarcoma J. w. B.; (2) that for susceptibility the simultaneous presence of these factors is necessary; (3) that none of these factors is carried in the sex (X) chromosome; and (4) that these factors mendelize independently of one another.

"*Factors underlying growth of a transplantable mouse sarcoma (J. w. B.)*—A series of experiments was undertaken by Little to determine factors that underlie sarcoma growth. The 675 mice used in these experiments were of two races: (1) common non-waltzing animals of albino (a) and dilute brown (dbr) stocks, and (2) hybrids produced by crossing these common races with Japanese waltzing mice and then back-crossing the first-generation hybrid with the common non-waltzing parent race.

"The two races are very different biologically. Series N includes the common-stock mice unrelated to Japanese waltzing mice (the race in which the tumor originated and in which it grows freely). Common mice rarely, if ever, have shown progressive uninterrupted growth of the Japanese tumor J. w. B., although as Tyzzer and Little have shown there may be temporary growth of the tumor followed by its regression and eventual disappearance. Their behavior in the present series of experiments is very similar to that in the earlier series referred to.

"Animals of series B. C. (back-cross), on the other hand, have one of their grandparents a Japanese waltzing mouse of the same inbred race which gave rise to the tumor, and one parent a first-generation hybrid between the Japanese waltzing and common races. These first-generation hybrids will, as shown by Tyzzer and Little, grow the tumor as well as, if not better than, animals of the pure Japanese waltzing race. If, as seems certain, hereditary factors favoring growth of the tumor are introduced by the Japanese waltzing race, the B. C. generation has a direct opportunity to receive them, while the common race has not.

"Beginning with a date two weeks after inoculation, weekly observations were made upon all inoculated mice. The mice were examined individually by palpation, and the presence or absence of a mass noted. If a mass is present it is described, and if it is larger than a pinhead a sketch is made of it on the record sheet of the mouse.

"Thus weekly observations are made in the case of all animals up to and including the sixth week after inoculation. From that time on, observations are made upon only those animals showing a mass. In this way a diagrammatic representation is obtained of the gradual growth of the tumor as well as a record of its diminution and eventual disappearance, should this take place. The absolute size of the mass can not, of course, be very accurately determined, and is not to be considered at present. The Misses B. W. Johnson, E. E. Jones, and D. M. Newman have been of the greatest assistance in the tedious work of inoculation and observation of the mice.

"Mice were inoculated in ten age-groups: 2, 4, 6, 8, 10, 12, 14, 16, 18, and 20 or over days old, respectively. In calculating the amount of growth observed, a single observation is the unit employed.

"A short table will show the interesting difference between the albino stock mice (N) and those of the back-cross (B. C.) generation. The sign + denoted an observation showing a mass, the sign − one which is negative (table 11).

TABLE 11.—*Comparison of stock and back-cross mice.*

	+	−	Per cent.
Stock mice (N)	233	1,862	11.12±0.46
Back-cross mice (B. C.)	170	799	17.54± .83
Difference	.....	.....	6.42± .95

"The difference between the races is certainly significant, being almost seven times its probable error.

"If the mice in each series are divided into a younger (2 to 10 days old) and an older (12 to 20+ days old) group, according to their ages at inoculation, an interesting fact is brought out (table 12).

TABLE 12.—*Comparison of younger and older groups.*

	Per cent of observations showing growth.		
	Young group.	Old group.	Difference.
Stock mice (N)	12.87±0.6	9.49±0.6	3.38±0.85
Back-cross mice (B. C.)	13.77±1.05	21.58±1.28	7.81±1.65

"The differences between the two age-groups are in the opposite direction in the two series. When the data are analyzed according to sex, another point is brought out, namely, the fact that the females present certainly significant differences between the age-groups, while the males do not (table 13).

TABLE 13.—*Comparison of sexes and age-groups.*

	Per cent of observations showing growth.			Remarks.
	Young group	Old group.	Difference.	
Stock mice (N):				
Males	15.70±1.64	10.30±1.2	5.4 ±2.6	Probably not significant.
Females	19.46±1.31	9.39± .87	10.7 ±1.51	Significant.
Back-cross mice (B. C.)				
Males	14.51±1.51	15.38±1.55	0.87±2.46	Not significant
Females	12.12±1.76	25.74±2.07	13.62±2.71	Significant.

"The suggested explanation is that the females of the series in the older age-group reach sexual maturity earlier than the males, and in many cases at least do so during the period in which they are under observation. That the ovary increases its activity as an organ of internal secretion at the onset of sexual maturity is well known and affords an assurance that a more detailed differentiation of the tissues is possible. The true physiological nature of the individual expresses itself and the genetic factors, determining in the stock mice elimination of the tumor and in certain of the back-cross mice its growth come into full activity.

"It may be concluded that the onset of ovarian activity following the attainment of sexual maturity acts in mice as an important agent in determining the fact of successful implants of a mouse sarcoma. Thus in a race of a genetic constitution known to be unfavorable to growth of this tumor, the most stringent and efficient elimination is found in females old enough to be sexually mature. On the other hand, in a hybrid (back-cross) race, the females of sufficient age to be sexually mature show the highest percentage of tumor growth.

"Both these cases are explicable on the supposition that the internal secretions arising at the onset of sexual maturity are important agents in determining physiological differentiation of the tissues of the animal. This differentiation is merely the expression of its full hereditary physiological make-up. This in the non-susceptible race eliminates the tumor, and in the back-cross race, where certain animals have some or all of the factors determining susceptibility, encourages tumor growth."

"*Origin and propagation of adenocarcinoma dBrA.*—In March 1920 an adult female dilute brown mouse of a closely inbred strain of this color, which has been under observation for eleven years, showed a tumor nodule of spontaneous (*i. e.*, not inoculated) origin. Examination of sections of this nodule showed it to be an adenocarcinoma. Upon subcutaneous implantation, bits of this tumor dBrA have grown in all the animals (30) of the inbred race which were inoculated. Some two weeks after the tumor dBrA was observed, another adult female dilute brown mouse of the same inbred strain showed a spontaneous tumor. This upon section showed a structure indistinguishable from dBrA. It also was classified as an adenocarcinoma, dBrB. This has also grown in 100 per cent of the animals (20) of the closely inbred dilute brown race inoculated. It is to be expected that all the mice of the dilute brown race should after eleven years of inbreeding have a strikingly similar genetic constitution. This should find expression in the similarity of the nature and physiology of tissues as well as in other characteristics. It is, therefore, to be expected that bits of the transplanted tumor should persist in different individuals of this race, since, by inbreeding and the resulting approach to homozygosity, the conditions met with in homoplastic implants become more nearly identical with those characteristic of autoplastic implants.

"That the factors underlying growth of implants of one of these tumors (dBrA) were not generally distributed in all common mice was shown by its inoculation in 10 animals of an inbred yellow-and-black agouti race, unrelated to the dilute brown race in which the tumor originated. These 10 mice were negative and failed to grow the tumor.

"Another group of mice was available for inoculation. These were back-cross animals resulting from a pure dilute brown mouse crossed with an F_1 hybrid between a dilute brown and a Japanese waltzing mouse. All such mice therefore possessed one dilute brown parent which by hypothesis should be forming gametes of which a considerable preponderance had the factors necessary for susceptibility. Actually, of 24 mice inoculated, 20 showed progressive growth of the tumor dBrA, while two others showed temporary growth with regression and eventual disappearance. In its behavior, therefore, the tumor dBrA gives genetic results explicable on a similar basis to that employed in the case of the Japanese tumor. It is interesting to find supporting evidence from a race of common non-waltzing mice showing that the principles involved are not confined to any one race or group of animals.

"*Origin and propagation of adenocarcinoma dBrB.*—The tumor above described as dBrB grew in all dilute brown mice in which it was inoculated, even if dBrA was inoculated at the same time and in the same mouse.

"During the summer of 1920, Mr. L. C. Strong conducted experiments on the reaction of wild mice to implants of these two tumors. Each mouse was inoculated on the same day with dBrA on the right side and with dBrB on the left. The mice have been observed at weekly intervals, beginning at a date two weeks after inoculation. Those showing a palpable mass are designated +; those negative, −."

TABLE 14.—*Comparison of reaction of mice to tumors in dBrA and dBrB.*

Observation, post-inoculation.	Total mice.	dBrA.			dBrB.		
		+	−	*p. ct.* +	+	−	*p. ct.* +
2 weeks.....	114	7	107	6.1	20	94	17.5
3 weeks.....	74	4	70	5.4	12	62	16.2
4 weeks.....	54	3	51	5.5	7	47	13.0
5 weeks.....	42	1	41	2.3	3	39	7.1
6 weeks.....	26	0	26	0.0	1	25	3.8
Total.....	.....	15	295	4.68±0.8	43	267	13.43±1.29

The difference between the percentages of observations showing indications in the two tumors is 8.75 ± 1.51 or 5.7 times its probable error. This demonstrates that even in the small number of observations recorded we have in the reaction to implants of the tumors a physiological test more delicate in its discrimination than is the histological test, which failed to show notable differences between the two tumors. The experiment indicates that reaction to tissue implants may be used as a comparative method which gives an extremely delicate test of physiological differences. Mr. Strong is also studying the effects of gonadectomy and splenectomy on growth of implants of these tumors and will continue the work during the ensuing year.

HEREDITY OF MENTAL AND PHYSICAL TRAITS IN DOGS.

A study has been undertaken on the method of inheritance of instincts and other traits in dogs. This work is under the immediate charge of Dr. E. C. MacDowell. On September 1, 1920, the laboratory possessed 7 male and 6 female adult Dachshunds and 14 pups. Four litters have been born, with the loss of only one animal, which died at birth. There were also 1 male and 3 young female English setters. All of the breeding animals are of standard-bred stock. Already much work has been done in testing instincts of the individual breeding animals, and some special methods of measuring strength of instincts have been worked out.

HEREDITY IN SHEEP, RABBITS, AND POULTRY.

The experiments on heredity of twinning and multinipples in sheep were continued. A new and young ram, born 1919, himself one of triplets, was used as a sire for the lambs born in 1920. There were 28 lambs born from 21 mothers, being a proportion of 1.3 lambs per

mother, as contrasted with 1.8 for 1919, 1.6 for 1917, and 2.2 for 1916. It is thought that this falling-off in percentage of twin births is due, in part, to the youth of the ram and, in part, to the poor nutritive conditions in which the ewes were found at tupping time. The cooperative sheep experiment with the New Hampshire Experiment Station, which experiment is primarily under the direction of Mr. E. G. Ritzman, is being continued. A joint paper by Ritzman and Davenport, "A comparison of some traits of conformation of Southdown and Rambouillet sheep and of their F_1 hybrids," was published during the year.

During the year about 80 chicks of the Silky and rumpless strains were hatched. Dr. George B. Jenkins made further studies on rumplessness and abnormal plumage of these strains.

Professor H. D. Fish, of Denison University, Research Associate at the Station, has made use of the facilities of the Station during the year to continue his remarkable series of rabbits which are being bred, largely, to get at the factors for spotting and coat color.

HEREDITY IN MAN.

Studies on this topic by members of the station are considered under the Eugenics Record Office.

SPECIES HYBRIDS.

More and more, in both plants and animals, the subject of sterility and the importance of lethal factors, factors that kill, is assuming great prominence. This leads to the subject of sterility between distinct species. As a guest of the Station, Mr. John Belling has been paying special attention to the nature of species hybrids in plants and the mode of inheritance of their characters, especially of partial sterility. Mr. Belling is working especially on the genus *Canna*, which contains numerous species that are propagated as clones. It appears that most of the *Canna* clones are heterozygous and much affected by imperfect pollen-grains, which can be readily distinguished from those that are perfect. The topic has important theoretical bearings on the origin of species, since distinct species are usually characterized by some degree of sterility.

GERMINAL AND SOMATIC VARIATIONS.

MUTATIONS IN MUCOR.

A report on mutations by Dr. Blakeslee in vegetatively pure lines of mucors is in press. In addition to a number of mutants of various types already reported, one mutant from a hermaphrodite has been cultivated in pure vegetative lines since 1913, but has consistently failed to show sexual spores since its first discovery. That it is still a hermaphrodite is obvious by its sexual reactions with both test plus (+)

and minus (−) races of dicœcious species and by the fact that it has given rise to another distinct mutant which does produce zygospores, although in scanty amount.

THE VASCULAR ANATOMY OF VARIANT BEAN SEEDLINGS.

Dr. Harris has, for some years, been breeding strains of beans with remarkable abnormalities that appear even in the seedling stage. Since 1917 a detailed study of the vascular morphology of these variant bean seedlings (in comparison with the normal) has been under way in cooperation with Professor E. W. Sinnott, of the Connecticut Agricultural College, and with the assistance of Dr. John Y. Pennypacker and Mr. G. B. Durham. A first paper, covering the problem of number and variability of bundles in the different regions of dimerous and trimerous seedlings is now in press. The results of this study show that external differentiation, such as that which characterizes dimerous and trimerous seedlings of *Phaseolus vulgaris*, is accompanied by profound differences in internal structure. They show further that anatomical characters, which by morphologists in general have been regarded as relatively stable, may be highly variable, even in series of individuals which are genetically highly homogeneous. Furthermore, the results show that variability in morphological characters is not a constant for the plant as a whole, but may differ from region to region or from organ to organ. In the seedling types investigated, for example, hypocotyl and epicotyl differ widely in the variability of bundle-number. Furthermore, differences in variability from organ to organ or from region to region are not constant, but may be conditioned by other morphological features. Thus, the variability of bundle-number of normal seedlings is higher in the hypocotyl than in the epicotyl. In seedlings with three cotyledons and three primordial leaves just the reverse is true. Other phases of the problem will be discussed in papers nearly ready for publication.

THE ORIGIN OF PIEBALD SPOTTING IN DOGS.

During the past year Dr. Little has published a note concerning the occurrence of piebald mutants in thoroughbred Scottish and Airedale terriers. These two breeds have been selected for generations for absence of white spotting and are unknown in piebald forms. The sudden appearance of spotted mutants with a considerable amount of white demonstrates that in some forms at least clearly piebald forms can arise without gradual selection from minute beginnings.

ALTERATION OF THE QUALITY OF A POPULATION BY SOMATIC SELECTION.

Dr. Banta has completed, ready for press, work upon selection for reactiveness to light of *Entomostraca* of the order Cladocera. The study has a special interest in being, apparently, the first extensive investigation of selection of a purely physiological character. As

stated in last year's report (p. 132), one line (line 757) showed a marked divergence in the strains selected for greater and for less reactiveness respectively. Further analysis has brought out additional points: (1) Environmental influences, which much affected the mean reaction-time in all the lines studied, in the line in which an unmistakable selective effect was obtained, merely served to cut down the divergence temporarily; (2) the result, divergence in reactiveness, is due to changes in the reactiveness of both the high and low strains of the line affected; (3) the form of the reaction-time curves indicates that the effect was cumulative and that the divergence was still increasing when selection was discontinued; (4) the result appears to be due to many small genetic changes; (5) the two strains of the line are indistinguishable, except in their differences in behavior to directive light stimulation; (6) the divergence is permanent, or at least persisted 112 generations (32 months) after selection was discontinued.

EFFECT OF CAVE CONDITIONS.

For some years Dr. Banta has been breeding animals under cave conditions to ascertain the change in color, or possibly form, that may result, in the hope of throwing some light upon the extraordinary characters of cave animals. Amphipods are breeding freely under our cave conditions. Additions to our cave fauna are being made each spring.

MUTATIVE COLOR CHANGES IN FLOUNDERS.

An observation which has interested Dr. Banta concerns the occurrence of pigment on both sides of the common flounder in Cold Spring Harbor. During this summer apparently about 20 per cent of the fish, which are presumably of this year's (early spring) hatch, possess more or less pigment on the side upon which the fish rests and which is normally entirely without pigment. Fishermen agree that they had never commonly seen "flatfish with black on both sides" until the present season, though they claim to have seen them occasionally before. In an examination of all the larger flatfish available (only a few, however) which were presumably of a previous year's hatch, only one with any trace of pigment on the under side was found. The pigment in this case consisted of a mere trace on the caudal fin and in the lateral-line region of the tail. The pigment on the under side in the smaller fish varied all the way from a mere trace caudally to complete pigmentation of the under side, except for the head and a small portion immediately posterior to it. All specimens were fully pigmented on the upper side, which bears both of the eyes.

PHYSIOLOGY OF REPRODUCTION AND DEVELOPMENT.

EFFECT OF VARIATIONS OF OXYGEN-SUPPLY ON AVIAN DEVELOPMENT.

In connection with his sex studies, Dr. Riddle has found it necessary to undertake or to continue the examination of certain factors or con-

ditions of avian development and reproduction. The more important of his results are the following:

It has been found that the failure of some birds' eggs to hatch is caused by a hitherto unsuspected and quite unapparent inadequacy of the egg-shells. Shells which do not break may, nevertheless, permit a too rapid ingress of oxygen and egress of water, and this certainly results in the death of many embryos. Undoubtedly this discovery, which is of much interest in our sex and fertility studies, will become of real importance in the poultry industry, where probably millions of incubated eggs with failing embryos are annually lost from this source.

Two attempts have been made to learn a treatment for birds which produce eggs with defective or inadequate shells. These attempts have been essentially unsuccessful in the main purpose, but have developed some useful facts. In collaboration with Mr. Martin C. Hanke, it was found that the feeding of additional soluble calcium salts—calcium lactate and calcium phosphate—affects but very slightly the amount of calcium which laying doves utilize in the formation of the egg-shell. It is concluded that the inadequate shells are probably not primarily due to a lack of soluble-calcium compounds in the food of the bird.

In view of the above result, it was thought advisable to investigate the mechanism of control of the oviducal secretions of the bird. With the assistance of Mr. Cecil V. King, the effects of atropine, nicotine, and cocaine upon the production of albumen and shell-material was extensively studied. The results lend no hope to a possible stimulation of excess secretion by means of drugs. An effect (somewhat reduced secretion) could usually be obtained with appropriate dosage of cocaine and nicotine, but Dr. Riddle concludes that either the oviducal secretions of the bird are largely independent of the sympathetic nerves or that the drugs which act most pronouncedly on these nerves of the mammal do not have an essentially comparable action in doves.

Finally, in connection with the study of the effects of increased and decreased pressures of oxygen on embryos of the two sexes, Dr. Riddle has completed an investigation of the necessary gaseous environment of avian embryos. In general, it is found that for oxygen the lower limit of life and development, for 24 to 48 hour periods followed by return to normal air, is about 10 per cent and the upper limit about 96 per cent. The upper limit for carbon dioxide, in connection with increased pressures of oxygen, is not far from 10 per cent, which is more than 300 times the amount normally present in air. The age of the embryo is an important modifying factor. It has also been found that the higher pressures of oxygen result in the complete disappearance of hemoglobin from the blood of embryos of about 2 days of development, and that abnormalities and "monsters" are often produced in embryos of less than 1.5 days. A further study of the conditions and nature of these changes is being made.

PHYSIOLOGY OF FECUNDITY IN THE DOMESTIC FOWL.

Biometric investigations of the physiology of egg production and the inheritance of egg production in the domestic fowl have been continued by Dr. Harris in cooperation with three of the agricultural experiment stations. These studies have to do with the prediction of future egg production from the record of short antecedent periods, with the relationship between first and second years' production, and with the correlation between the record of mother and daughter. Manuscripts giving the results of some of this work are in an advanced state and will be ready for publication shortly.

OTHER INVESTIGATIONS.

STUDIES ON THE PHYSICO-CHEMICAL PROPERTIES OF VEGETABLE SAPS.

These investigations, which have been under way for the past several years in cooperation with the Department of Botanical Research, have been continued and considerably expanded during the year by Dr. Harris and his assistants. He reports progress along the following lines:

"(1) *Osmotic concentration and electrical conductivity of sap properties in relation to growth-form.*—A first paper on the problem of the electrical conductivity of the leaf-tissue fluids of the Cold Spring Harbor region, to be published with Professor R. A. Gortner, of the University of Minnesota, and Mr. John V. Lawrence, of the University of Chicago, is now ready for press. The results of this investigation, in connection with others carried out earlier, are of considerable interest in relation to the much-discussed problem of the evolution of woody and herbaceous growth-forms. Earlier studies in the Jamaican montane rain forest, in the Arizona deserts, and in the mesophytic habitats of the north shore of Long Island have shown that the osmotic concentration, as measured by the cryoscopic method, is far higher in the leaf-tissue fluids of ligneous than of herbaceous species. As a result of these studies a definite physico-chemical difference in the leaf-tissue fluids was shown to be associated with the differentiation in growth-habit. In the present paper it is shown, on the basis of a large series of determinations in the various non-halophytic habitats of the north shore of Long Island, that the specific electrical conductivity of the expressed leaf-tissue fluids of ligneous species is lower than that of herbaceous species. Thus, while the concentration of total solutes is higher in the tissue fluids of ligneous species, the reverse is true for ionized electrolytes. An investigation of the correlation between freezing-point lowering, Δ, and specific electrical conductivity, k, shows that in a series of species there is practically no relationship between these two properties of the expressed tissue fluids.

"(2) *The chloride content of the tissue fluids of halophytic plants.*—The osmotic concentration of the tissue fluids of halophytic plants is generally higher than that of comparable growth-forms in non-saline habitats. This may be due either to the direct absorption of salts from the soil or to the synthesis of larger quantities of organic substances. The solution of this problem is of considerable importance in relation to the more general problem of adjustment and adaptation. Considerable attention has, therefore, been given to the determination of the chloride content of plant saps in relation to osmotic concentration. With the cooperation of Mr. John V. Lawrence, of the Department of Physiological Chemistry of the University of Chicago, satis-

factory methods for the collection and analysis of large series of samples have been worked out. The analyses of the extensive series of samples collected with the assistance of Messrs. Lawrence and Hanke in Dr. Harris's work along the Atlantic Coast in 1919 (Year Book Carnegie Inst. Wash., 1919, 143) has now been completed and the results will be presented shortly.

"(3) *Sap properties of the vegetation of the Lake Bonneville Basin.*—The most extreme concentrations of the soil solution to which plants are exposed in their struggle for existence are found in the basins of ancient lakes. In such regions striking peculiarities of structure bear witness to the evolutionary changes from the plant structures typical of mesophytic regions which have been necessitated by the special environmental conditions. Of the available regions, the basin of ancient Lake Bonneville, of which Great Salt Lake and Sevier Lake are small remnants, affords the best opportunities for investigation, both because of the wide range of environmental conditions and the relative accessibility of the habitats. Furthermore, the classic geological studies by Gilbert and the pioneer work on the indicator plants of alkaline regions by Kearney, Briggs, and Shantz and their coworkers makes coordination with other investigations highly desirable. The Biophysical Laboratory and the Office of Drought and Alkali Resistant Plant Investigations of the Department of Agriculture asked Dr. Harris to cooperate in the investigation of the physico-chemical properties of the vegetation of this region, and he was absent from Cold Spring Harbor from May 20 to September 7, 1920, while engaged in this and associated projects. Mr. Andrus T. Valentine, of Cold Spring Harbor, served efficiently as field assistant during the entire period. Dr. R. A. Gortner, professor and chief of the Division of Agricultural Biochemistry of the University of Minnesota, and Mr. Walter B. Hofmann, instructor in agricultural biochemistry at the University of Minnesota, joined in the field operations until August 1. Operations were then transferred to the Cooperation Testing Station at Sacaton, Arizona, where facilities were placed at Dr. Harris's disposal.

"In the Great Salt Lake region about 750 determinations of osmotic concentration and specific electrical conductivity were made. Samples were also preserved for chloride determination. These was chiefly on native vegetation, but a considerable number was based on cereal varieties under investigation on the dry-farming substation of the Utah Agricultural College and the Office of Cereal Investigations of the Federal Department of Agriculture at Nephi, Utah. Thanks are due to C. R. Ball, of the Department of Agriculture, and to F. S. Harris, Director of the Utah Agricultural Experiment Station, for permission to obtain material from this valuable series of cultures; also to Mr. Bracken, the superintendent of the Nephi substation, for assistance in obtaining materials. At Sacaton it was possible to secure valuable determinations on the halophytic vegetation of the Gila River Valley, and also to supplement our series of determinations on loranthaceous parasites by determinations of electrical conductivity as well as osmotic concentration. Dr. T. H. Kearney also placed his culture of upland and Pima Egyptian cotton at our disposal, and it was possible to compare the sap properties of these two types and of their F_1 hybrids under as nearly as possible identical conditions. Incidentally it may be noted that Pima Egyptian cotton is probably the plant mutation of the greatest economic importance, a $20,000,000 crop having been grown in 1919. A crop of 200,000 acres, the progeny of an original mutant individual, is now in the field. The results of Dr. Harris's determinations in the field and in accessible cultures will be worked up as rapidly as the computing and clerical force available will permit."

STATISTICAL THEORY OF PLOT TESTS.

Studies of field heterogeneity in relation to the problem of the accuracy of plot tests of new varieties have been under way at this Station for the past several years. Some time ago the Office of Western Irrigation Agriculture of the United States Department of Agriculture asked our cooperation on this problem and work has since been conducted largely on the data and with the support of the Department of Agriculture. A first paper, in which substratum heterogeneity was shown to be a universal factor of significance in plot tests, has been published by Dr. Harris (Jour. Agr. Res., vol. XIX, 279–314, July 1, 1919), and another, in which the permanence of the differences in the small plots of an apparently uniform field and the influence of preceding upon subsequent crops are discussed, is now ready for publication.

COOPERATIVE WORK ON HUMAN NUTRITION.

Studies of variation and correlation in the basal metabolism of the individual subject have been under way by Dr. Harris, in cooperation with the Director of the Nutrition Laboratory of the Institution. The results are practically ready for publication.

EUGENICS RECORD OFFICE.

STAFF.

During the year ending September 1, 1920, the Eugenics Record Office has gradually resumed its former activities, though with diminished personnel, because of limited funds. Dr. H. H. Laughlin, who has served effectively as superintendent of the Office from the beginning, was given leave of absence for one year from June 1 and Dr. Banker has served in his stead. Dr. Arthur H. Estabrook was honorably discharged from the service of the government in April and took up again his investigation of the Ishmaelites, with his headquarters at Indianapolis. Dr. Banker has continued his investigations into heredity of aristogenic families. Dr. Wilhelmine E. Key returned to the Office for a few months in the autumn and winter of 1919–20, but is now located with the Battle Creek Sanitarium. Miss Nelson has served continuously as archivist.

HEREDITY IN ARISTOGENIC FAMILIES.

Dr. Banker's work proceeds somewhat slowly, because he is without clerical assistance and because of the difficulty in filling all the gaps in the biographical records. He has been confronted with the difficulty of expressing quantitatively the mental grade of individuals. In the case of college graduates of the last three or four generations it is possible to obtain an approximate relative grade of intellectual attainment, but for non-college people there is no basis for comparison. From the data available one forms an impression of greater and less ability and may arrange these crudely under an arbitrary scale, but it is impossible to draw any definite lines to the scale and equally impossible to group individuals into any definitely graded categories.

Also, we are far from being in a position to make a Mendelian analysis of the hereditary elements of the genes involved in an eminent author, poet, or popular orator. Psychology has not progressed far enough to aid in this matter. Indeed, it seems probable that the genetical analysis will aid the psychologist.

Nevertheless, Dr. Banker has obtained some significant facts from the study of an extended network of closely interrelated families. The network consists of a portion of the descendants of one couple through seven or eight generations, together with the blood relatives of these descendants for usually three or four generations. Lines in which there were evidently no college graduates have been dropped. The material so far accumulated contains more or less complete records of 3,538 individuals, of whom 518 were college men, or 14.6 per cent. This is, of course, too high a proportion for the whole network, because of the dropping of those lines which contain no college-bred persons. However, it gives a true percentage for those lines in which there are

one or more college men, and may be used as a basis of comparison within that range. The base, 3,538, includes all recorded individuals, a very large proportion of whom could not possibly have had a college record. Such are practically all the females, since in former years college courses were not open to women, and those males who died before the age of, say, 20 years. A more correct working-base would, therefore, be found by confining ourselves to the eligibles, that is, all men who have attained the age of 20. At present it is possible only to approximate the number of eligibles, which appears to be about 1,500, of whom, therefore, 34.5 per cent were college graduates. Among these, however, some lines show a much higher incidence of college men than others. Thus, in one branch, which we may designate as the W branch, there are more than 180 related individuals, of whom there appear to be not more than 12 who are college men, making less than $6\frac{2}{3}$ per cent, and in reality probably less than 1 per cent, since a large portion of this branch has been dropped because entirely lacking in college graduates. Of these 180, about 65 were eligible for college, hence something less than 18.5 per cent of those who could have gone to college actually went.

In the closely related G branch, from which scarcely any were dropped because of lack of college men, we have about 615 individuals, of whom 306 were eligible for college and about 121 actually attended, making thus 19.6 per cent of the whole branch, or 39.5 per cent of those eligible, that actually went to college. Other branches would show similar wide variation.

Moreover, comparing the W and G branches as to eminence, we find that, of the 12 W's who went to college, one is in Who's Who in America (vol. 10) and one in National Cyclopedia of American Biography, making $16\frac{2}{3}$ per cent of the college men of this branch who attained to sufficient distinction to be included in one of these publications. Of the 121 in the G's who actually entered college, 25 are mentioned either in the National Cyclopedia of American Biography or in Who's Who in America (vol. 10), making 20.6 per cent of the college men in this branch who attained to this degree of distinction. These facts appear to indicate that in the G branch we are dealing with a group of higher average intellectual ability than in the W branch.

PLURAL BIRTHS.

The Director attempted to secure light on the matter of plural births, especially twinning, in man. This is a matter of considerable interest, since about 1 per cent of all human births are twin births. These twin births are of two types, namely, those derived from two eggs ovulated simultaneously or nearly so, and those derived from a single egg which has formed two embryos. In the former case, each embryo is enveloped in its own chorion; in the latter, both embryos are enveloped

in one and the same chorion. Frequently a mother will have more than one pair of twins, a condition that may be called repeating. This phenomenon indicates a structural or physiological condition of the ovary which readily permits double ovulation. It is easy to understand how such an idiosyncrasy of the mother would tend to reappear in her daughter and thus the tendency to twinning show itself as a hereditary trait.

A statistical study of the close relatives of twin-repeating mothers, combined as a population, shows that in this population the ratio of twin production rises to 4.5 per cent, which indicates that such mothers belong to strains in which the factors for twinning are four times as effective as in the population at large. If, on the other hand, one considers as a population the close relatives of fathers of twins, then one still finds that the incidence of twins in this selected population is much above that in the population at large, namely, 4.2 per cent, a ratio nearly as high as that found for the relatives of the mothers. These proportions, calculated from the extensive records of the Eugenics Record Office, lead to the inquiry: How is it possible that there shall be a paternal inheritance in twin production that is as real, and nearly as potent, as the maternal. Also, the tendency for twin production is even stronger in identical than in two-egg twins, since the rate of twin production in families that produce identical twins is about 13 per cent.

The frequently denied possibility of inheritance of twinning through the father's side of the house depends on a tacit assumption which seems never to have been challenged by students of twin inheritance. This assumption is that the determining, essential fact in twin production as contrasted with single-birth production is the constant double ovulation in the first case and the constant single ovulation in the second. Whenever two eggs are simultaneously ovulated at a period when fertilization occurs, there will be twins. Under such an hypothesis it will be difficult to understand how the results are influenced by any tendency toward twin production from the father's side of the house.

Where the above hypothesis, however, fails is that about 8 per cent of ovulations are double, according to counts made by Leopold and other gynecologists. Thus the proportion of twins actually born is less than one-fifth, probably only one-seventh, as great as the proportion of double ovulations. To secure light on the question of what has happened to reduce the proportion of twin births so far below that of twin ovulations, a comparison has been made in the uteri of pregnant swine between the number of embryos in the course of development and the number of recent corpora lutea, each one of which indicates one ovulation. A preliminary study made during the winter revealed that there was a regular deficiency of from one to seven advanced embryos, as compared with the number of corpora lutea. During the

summer Dr. George W. Corner, jr., of the Johns Hopkins Medical School, assisted by Mr. Clyde E. Keeler, of Denison University, extended this statistical study into several hundred pregnant uteri. It appeared from all of these counts that nearly 98 per cent of the eggs ovulated enter the Fallopian tubes. Only about 80 per cent, however, are developing during the second month, and only about 70 per cent develop to the last third of pregnancy. Probably not more than two-thirds of the eggs ovulated result in pigs born alive. Just what happens during the first month of pregnancy which should cause the failure of so many eggs to develop is yet uncertain. Probably a certain proportion of the eggs are not fertilized. It is certain that of the fertilized eggs a certain proportion, which may lie between 10 and 20 per cent, proceed along their development to different points and then die. Similar blighted fetuses have been commonly found by obstetricians, and scores of cases of blighted twin fetuses are recorded in the literature. Also, as is well known, miscarriages and stillbirths are fairly common among humans, of which an important cause is apparently sheer inability to continue development because of the internal weakness of the embryo. Children who are born are already a selected group from among those whose development has been initiated. Such incapacity for development has been observed by geneticists in a large number of cases, and the category of lethal factors which inevitably prevent further development is now well recognized. Such lethal factors probably correspond to gross variation in essential visceral organs and run parallel to such gross variations of external organs as cleft palate, microphthalmia, and the absence of appendages. Now, such lethal factors may be brought into the zygote by the egg alone, by the sperm alone, or by both. They are not found in all germ-cells; it may be only in a small proportion of them. When they occur in the gametes of both consorts, small families, with some feeble children, may be expected; but when absent in the germ-cells of both parents, then, in a good environment, the fertilized egg will develop vigorously, with good prospects of reaching maturity. Now, it is in such families that any tendency toward double ovulation will be expressed in the production of healthy twins. This accounts for the long-known statistical fact that the proportion of twins is greater in highly fecund families than in those that produce few offspring. Also, among humans there is probably a frequent failure of fertilization of both eggs, resulting in a development of only one of a potential pair. The preceding considerations make it clear where the male factor enters in twin production; for the father, as much as the mother, determines whether both of a pair of simultaneously ovulated eggs shall be fertilized, and whether or not they shall receive lethal factors.

The statistical studies made on the relation between number of corpora lutea and number of embryos have been greatly extended by

Dr. Corner during the summer, and he has been able to establish the fact that there is in hogs an internal migration of ova from one horn of the uterus to another. He finds also a strong tendency, through this mechanism, to establish an approximate equality of embryos in the two horns, even when the disparity in the number of corpora lutea in the ovaries is very great.

SEX-LINKED LETHAL FACTORS IN MAN.

Dr. C. C. Little, with the cooperation of Miss Marion Gibbons, has prepared for press a paper on the statistical evidence of the occurrence of sex-linked lethal factors in man.

Sex-linked factors, other than lethals, have long been known in man. Among these, two genes which have been especially well studied are those for hemophilia and for color-blindness. The genes for these characters and their normal allelomorphs are carried in the X or sex chromosome. Since sex-linked lethals if they exist will also be carried in the sex-chromosome, certain of them may be closely linked with either of these genes or with their normal allelomorphs, according to the nature of the gamete in which they originate. If one of them occurs in a chromosome in which the gene for hemophilia is carried, it will, if it be closely linked with that gene, eliminate all the males, which would otherwise be hemophilic except in case a cross-over occurred. The same is true for color-blindness. Families in which such a condition was found might continue for several generations without giving a hemophilic or color-blind individual because of rareness of cross-overs. Such families would, therefore, not be recognized and would not be included among those selected to show the method of inheritance of these traits.

If, however, the lethal factor were linked with the normal allelomorph of these genes, normal sons would appear only when crossing-over occurred. These families would show a striking excess of hemophilic or color-blind males, as the case might be, and would, of course, be included in any study of the inheritance of these traits which might be made with other families in which no sex-linked lethals occurred. If a sufficient number of such families were, by chance, included in any mass of statistics on the inheritance of these traits, they should produce in the data a significant excess of hemophilic or color-blind males above the expected 1 : 1 ratio.

To test this matter, data at the Eugenics Record Office, as well as in Bulloch and Fildes's work on hemophilia and in Nettleship's work on color-blindness, have been tabulated and analyzed. Dr. Sewall Wright had previously tabulated the data in Bulloch and Fildes, and his data, which have very kindly been turned over to Dr. Little, are included in his figures. He has developed also a formula for calculating the number of abnormals (bleeders or color-blind) to be expected on

a 1:1 ratio basis in families of different sizes. This formula has been used in the calculations herewith appended (tables 15 and 16).

TABLE 15.—*Hemophilia.*

No. in family.	No. of families.	No. of males.	No. of bleeders.	No. of bleeders expected.	One certain bleeder extracted from each family.		
					No. of males.	No. of bleeders.	No. of bleeders. expected.
2	82	164	128	82	82	46	27.3
3	124	372	250	186	248	126	88.5
4	75	300	169	150	225	94	85.0
5	42	210	136	105	168	94	66.3
6	26	156	81	78	130	55	53.3
7	27	189	100	94.5	162	73	68.2
8	7	56	31	28	49	24	21.1
9	3	27	13	13.5	24	10	10.5
10	1	10	10	5	9	9	4.0
11	2	22	5	11	20	3	9.0
12	3	36	17	18	33	14	15.0
13	1	13	4	6.5	12	3	5.5
Total....	393	1555	944	777.5	1162	[1]551±11.05	543.7

The difference between the expected and observed numbers of bleeders is an excess of "observed," which is 8.8 times its probable error.

The data from pedigrees of color-blindness are much more meager, but are interesting in support of the figures previously quoted in the case of hemophilia.

TABLE 16.—*Color-blindness.*

No. in family.	No. of families.	No. of males.	No. of color-blind.	No. of color-blind expected.	One certain color-blind extracted from each family.		
					No. of males.	No. of color-blind.	No. of color-blind expected.
2	34	68	50	34	34	16	11.32
3	32	96	64	48	64	32	24.84
4	19	76	44	38	57	23	21.54
5	3	15	9	7.5	23	6	3.74
6	9	54	26	27	45	17	18.4
7	4	28	16	14	24	12	10.1
Total....	101	337	207	168.5	236	[1]106±5.15	89.94

The difference in this case is 3.1 times its probable error and is probably significant.

Another indication of the presence of sex-linked lethal factors is to be expected when the sex-ratio of families in which all the males are either hemophilic or color-blind are contrasted with families in which

[1]The ± value given represents the fluctuation in the actual number of hemophilic males, due solely to chance. On the basis of this value the actual number observed may be compared with the theoretical expectation as indicated.

part of the male offspring are normal. If the excess of hemophilic and of color-blind males is due to sex-linked lethal factors, families in which all the males are hemophilic or color-blind should include among them a considerable number in which this condition is due to linkage of the sex-linked lethal with the normal allelomorphs of hemophilia and of color-blindness. This should be accompanied by a relative excess of females in which no sex-linked lethals are present. Table 17 shows the totals for the four categories of families in question:

TABLE 17.—*Sex-ratio.*

	Males.	Females.	Ratio males to 100 females.	Difference.
All males hemophilic.....	413	337	122.55±2.73	35.26±3.39 (10.4×P.E.)
Some males hemophilic.....	1070	678	157.81±2.02	
All males color-blind.....	114	100	114.0 ±4.4	30.62±6.52 (4.6×P.E.)
Some males color-blind.....	184	119	154.62±4.83	

In both cases, although the data are gathered from very different sources, there is a significantly lower sex-ratio in the matings where it is to be expected.

We may, therefore, conclude that statistical analysis of human pedigrees of hemophilia and color-blindness indicate strongly that sex-linked lethal factors are present in man. Only detailed long-time investigations covering several generations of unusually prolific families can furnish additional proof.

HEREDITY OF HAIR, EYE, AND SKIN COLOR, AND HAIR FORM.

The Director has spent some time in securing additional and improved data on the family recurrence of peculiarities in pigmentation and hair form. Acknowledgment is made of the courtesies of the superintendent and principals of the Huntington schools and of the principals and instructors in biology at Girls' High School and Erasmus Hall, Brooklyn, and Evander Childs High School, the Bronx, for co-operation in this study.

MUSICAL FAMILIES.

Some months ago Professor C. E. Seashore, of the State University of Iowa, invented a method of expressing quantitatively variations in sense of pitch, intensity, time consonance, and tonal memory. Tests of these capacities were made by the use of phonograph records prepared in his laboratory. After conferences with Professor Seashore in 1919, it was decided to ask the Institution for an appropriation to enable his student, Miss Hazel M. Stanton, to make tests on musical families in New York, Boston, and vicinity, and this was granted. She

accordingly made such studies from February to June 1920, and has deposited the results with the Office. Of the M family group a pedigree chart including 78 individuals was prepared, and of these 11 were given the standard test. Of the L family there are 38 individuals recorded, of whom 6 were tested. Of the B family 77 individuals are recorded and 12 tested. Of the K family 78 individuals are recorded and 8 tested. Of the S family 113 individuals are recorded and of these 21 tested. The results are being analyzed.

THE FAMILY OF ISHMAEL.

Dr. Arthur H. Estabrook is continuing his studies on this group, continuing the work of Mr. MacCulloch of a generation ago. The more important subfamilies of the group have been completely studied and written up for final use. Many smaller families, closely connected with these, have been studied; they are now being worked up to final form. Pauperism seems to be the most important characteristic of this group; and the feeble-mindedness is, in general, of the higher grades. The other anti-social traits are closely allied to the feeble-mindedness. In connection with this pauperism, the report will include a study of the social forces at work here in the past. Approximately 10,000 individuals are under study.

OTHER FIELD WORK.

Dr. Elizabeth B. Muncey has been carrying on field-work on twin-producing families and, incidentally, on other families occurring in the locality in which she has been working.

LIBRARY WORK.

Miss Mabel L. Earle has continued to furnish extracts of the literature on special subjects, as required for the use of the Office. She spent three months at the Surgeon General's Library in Washington, abstracting literature which was not available in the Boston and New York libraries. Miss Earle spent some time at Cold Spring Harbor in work upon the library here. She has also sent in several hundred sheets of typewritten abstracts on various topics, especially on those relating to human reproduction and to twinning. Dr. Muncey has also made abstracts of the literature on twins.

INBRED COMMUNITIES.

A further analysis of the data obtained on the populations of certain island communities has been made by Miss Mary M. Sturges. It is hoped that during the autumn of 1920 some additional field-work to clear up certain remaining points will be made.

ANTHROPOLOGICAL STUDIES IN THE ARMY.

As indicated in the last report, a "First study of the records on drafted men and the defects found in them" was published by the

Surgeon General's Office in June 1919, in collaboration with Lieutenant-Colonel A. G. Love. In November 1919 there was issued, in collaboration with Lieutenant-Colonel Love, "Defects found in drafted men." The first edition consisted of 359 pages of text and did not include the extensive appendix. In the summer of 1920 the printing of the full book, including about 1,600 pages with the tables, figures, and plates, was authorized, and it has since been issued from the press. This work dealt with the defects found in 500,000 men who were rejected by draft boards on physical grounds and about 2,000,000 recruits who were examined also at military camps. It is impossible to summarize a statistical work here. It may merely be stated that the incidence of each of the principal groups of diseases in the military population was analyzed; that the incidence of these diseases in the different states was considered; and the relation of these diseases to classification in the military service was discussed. The whole country was divided into 155 geographical sections, partly based upon the nature of the population and certain physiographic features. The distribution of defects in each of these different sections is analyzed. Similar sections are then consolidated into larger groups, such as agricultural, manufacturing, mining, desert, mountain, maritime, largely Scandinavian, largely German and Austrian, largely of Scottish origin, and so on. Also, the comparative distribution of defects is given for urban and rural districts. The selected cities showed about 15 per cent more of defect than did the rural districts. This excess of urban defects is largely determined by the excess of flat feet in urban districts. There is also in the cities an excess of underweight, inflammation of the middle ear, errors of refraction, goiter, pulmonary tuberculosis, defective teeth, and syphilis. Defects of the rural districts are largely influenced by the racial fact that the negro population is more prevailingly rural than the white population. The rural districts exceed the urban in hereditary congenital defects, partly due to the differential migration away from rural districts of those without such defects and partly to the more frequent consanguineous matings in a rural population with such defects. The rural districts, also, show an excess of defects arising from accidental injuries.

In summary, the northeastern part of the country appears to be characterized by congenital defects and those of city life. The Northwest is characterized by deformities due to accidents, by goiter, and by flat-foot. The Southeast is characterized by venereal diseases, hookworm, and similar other complications, including blindness of one eye, arthritis and ankylosis, underweight, mental defect, emotional disturbances, pellagra, hernia, loss of upper extremity, and bullet or other wounds. The Southwest is characterized by tuberculosis, drug addiction, hypertrophied tonsils, and hernia. The northern central area is contrasted with the southern central by having more goiter,

less tuberculosis, much less venereal disease, more variocele and more varicose veins, more valvular disease of the heart and cardiac hypertrophy and dilation, more deficient teeth, more psychasthenia and constitutional psychopathic states. It is characterized by more otitis media, errors of refraction, diabetes, curvature of the spine, defects of genitalia, and weak feet, but less epilepsy, blindness of one eye, pellagra, loss of upper extremity, bullet and other recent wounds, underweight, and deficient chest measurement.

STERILIZATION LAWS.

Dr. H. H. Laughlin, superintendent of the Office, completed in April a manuscript comprising 1,300 pages on "Eugenical sterilization in the United States," and this is now awaiting publication. This work treats the historical, legislative, legal, administrative, surgical, physiological, and eugenical phases of the subject. It contains a complete record and a careful analysis of all legislation and litigation relative to the matter; it provides, also, a complete statistical and historical, and, so far as possible, a physiological record of all cases of eugenical sterilization under the several statutes.

STATISTICAL STUDIES OF STATE INSTITUTIONS.

The statistical study of State institutions for the defective, dependent, and delinquent classes which was planned by Dr. H. H. Laughlin, who, as special agent of the Bureau of the Census, had charge of the collection of the data, appeared in the year under review. This directory is important for the work of the Office, since we have extensive correspondence with State institutions and much of our field-work has hitherto been done in connection with them. The number of persons cared for in State institutions has a eugenical importance, since the basis of the defect which has made such State institutions necessary is largely hereditary. The total number of inmates of such State institutions in January 1916 was approximately 400,000, of whom half were in institutions for the insane, one-fourth in State institutions for criminalistic, about an eighth in State institutions for dependents, about 5 per cent in State institutions for the feeble-minded, and the rest in institutions for epileptics, tuberculosis, blind and deaf, and still other minor causes. The total expenditures by the States for maintenance and operation of these institutions was, in 1918, $81,000,000. The directory includes a number of summary tables for the United States, and then takes up for each State the general statistics for the State which bear upon its ability to care for defectives, and gives a list of the institutions in the State, together with a map showing the location of each. This is followed by a detailed statement for each individual institution, concerning its controlling body, its chief executive officer, the number of employees, income and expenditures, character of persons provided for, and the number of inmates.

STATISTICS OF ANCESTRAL INFLUENCE.

Work on mechanical devices for demonstrating the mathematical formulæ of heredity was continued by Dr. Laughlin. A machine was perfected for aiding the formulation of the mathematical aspects of the segregation and recombination of chromosomes in passing from generation to generation. Twenty-eight formulæ which present general mathematical pictures of hereditary processes were developed; these were described in a paper entitled "Calculating ancestral influence in man," read before the National Academy of Sciences at New Haven, Connecticut, November 10, 1919. Work on a series of calculations for formulating the mathematical aspects of the "Pure-sire system of breeding" and the "Measure of consanguinity" were begun and are progressing satisfactorily.

ARCHIVES.

The care of the archives has remained in the hands of Miss Louise Nelson. The following is a summary of material added to the archives, September 1, 1919, to September 1, 1920:

I. Index cards:	
1. Main index	68,167
2. Persons index	3,910
II. Manuscript material:	
1. Field reports	5,951
2. Miscellaneous	751
III. Special schedules:	
1. Record of family traits	351
2. Family distribution of personal traits	3
3. Twin	128
4. Weight	32
5. Eye, hair, and skin color	17
6. Other, including polydactylism, left-handedness, tuberculosis, deafness, etc.	59
IV. Miscellaneous material:	
1. Individual analysis cards	246
2. Genealogical data cards	157
3. Biographical data cards	150
4. Newspaper clippings:	
a. Biographical	27,048
b. Genealogical	7,600
V. Books:	
1. Biography	130
2. Collective biography	32
3. Genealogy	10
4. Town history	9
5. Miscellaneous pamphlets	76

Up to the present time there is a total of 752,231 cards in the Sextuple Index and 4,500 in the Persons Index. And since each card affords space for 40 entries, and, as some contain this number and many others as many as from 4 to 10, it is probable that there are more than 2,000,000 entries on file. Of the Record of Family Traits schedules there are over 3,500.

From the books received, 10,058 pages have been indexed, but because of lack of filers it has not been possible to add this material to the main card-index. In addition to this, 4,272 sheets containing 461,376 entries made from manuscript material have also been accumulated.

During the summer we were able to make use of the assistance of a number of college students, some of whom were doing other work at Cold Spring Harbor, in the preparation of material for the archives and in the analysis of the records. Mr. William Kraus assisted in the analy-

sis of weight. Miss Laura Craytor, Miss Ruth H. Twining, and Mr. Clyde E. Keeler, all of Denison University, assisted in the work of filing clippings of biographies, genealogies, and special traits.

TRAINING COURSE.

The 1920 training course for field-workers in eugenics was in session from June 30 to August 10. There were 13 students in the course. This brings the total number who have been trained by this Office for field-workers up to 205. Clinical instruction was received at the State hospitals for the insane at King's Park, Central Islip, and Ward's Island; also at Letchworth Village, Randall's Island, and at Brunswick Home (for feeble-minded) at Amityville. A clinic was also held by Dr. W. B. Weidler at the Manhattan Eye, Ear, and Throat Hospital, and another at the Hospital of the New York Society for the Relief of the Ruptured and Crippled. Opportunity was also offered for observing the physical examination of immigrants at Ellis Island.

JOINT-BASIS FIELD WORKERS.

During the year the Office has continued its practice of supplying custodial institutions with eugenical field-workers, training the worker and paying her salary, while the collaborating institution provides maintenance and expenses of travel. During the year Miss Virginia Rohde continued at the State Hospital at Bangor, Maine. Miss Cornelia Augenstein resigned from her appointment at the Girl's Training School at Gainesville, Texas, and Miss Mae C. Graham was appointed to succeed her. Our contract with the State hospital at Central Islip was completed by Miss Dorothy Aldridge. In view of limitations of income, it seems probable that we shall have to restrict for the present the number of joint-basis field-workers.

VOLUNTEER COLLABORATORS.

The archives of the Eugenics Record Office are being increasingly utilized by other institutions, as well as individual eugenicists, as a depository of records having eugenical significance. Many are institutions which became acquainted with the work of the office through the employment of trained field-workers, and have for some years contributed largely to the records of this office. Among these may be especially mentioned the State Village for Epileptics at Skillman, New Jersey (Dr. David F. Weeks, superintendent); Whittier State School, Whittier, California (Dr. Fred C. Nelles, superintendent); Minnesota School for Feeble-minded at Faribault, Minnesota (through Dr. F. Kuhlman, Director of Research); State Hospital at Middletown, Connecticut (Dr. Floyd C. Haviland, superintendent); Letchworth Village for Epileptics at Thiells, New York (Dr. Charles S. Little, superintendent).

Many teachers in our colleges and other educational institutions are introducing the study of family histories into their courses in genetics, sociology, biology, and psychology, and some of them contribute the results of the work of their students to the archives of this office. Among those who have especially made this Office the repository of such data may be mentioned: Professor Will S. Monroe, State Normal School, Montclair, New Jersey; Dr. Louis W. Rapeer, Washington, D. C.; Norman R. Stoll, Central High School and Junior College, Detroit, Michigan; H. R. Hubbard, Plainfield High School; Miss Gertrude Sevin, Adelphi College, Brooklyn, New York; Professor Donald W. K. Davis, College of William and Mary, Williamsburg, Virginia; Professor A. J. Goldfarb, College of the City of New York; Professor Robert A. Budington, Oberlin College, Oberlin, Ohio; Miss Elizabeth Whittaker, Elmira College; Professor Charles W. Hargitt, Syracuse University; and many others.

The office has also received from G. L. Meylan, Director of Physical Training of Columbia University, about 2,000 negatives showing the physical build of students of that institution.

Among personal workers who have contributed to the office the results of their studies are Dr. F. L. Reichert, of Johns Hopkins Hospital; Dr. Edward D. Churchill, of Faulkner Hospital, Jamaica Plain, Boston, Massachusetts; and Edward L. Caum, Honolulu, Hawaii, all members of former training classes.

THE SECOND INTERNATIONAL CONGRESS OF EUGENICS AND THE EUGENICS RESEARCH ASSOCIATION.

On March 20, 1920, a committee of the National Research Council on the Second Eugenics Congress met at Washington, under the chairmanship of your Director, and voted to hold such a congress in New York City, September 22 to 28 (inclusive), 1921. A meeting of the general committee on the congress was held at the American Museum of Natural History, New York City, April 10, 1920, Dr. R. S. Woodward presiding, and further details were elaborated. It is believed that this congress will have a very beneficial effect on the public attitude toward and interest in research in eugenics.

On January 9, 1920, the executive committee on the Eugenics Research Association, under the presidency of Dr. Stewart Paton, approved the personnel of the eugenics committee selected by the National Research Council. The secretary of the association appeared at a hearing of the Congressional Committee on Immigration, to present the eugenical aspects of immigration. The association also made arrangements by which it acquired control of the "Eugenical News."

The eighth annual meeting of the association was held at Cold Spring Harbor on June 25, 1920, under the presidency of Dr. Stewart Paton.