shown particularly by the experiments of deVries. But peloria nectaria also occurs as a rare mutation in Linaria and other genera. What more natural than that a one-spurred ancestor of Aquilegia should undergo a peloric mutation, producing a flower with the five petals all bearing spurs? Indeed, it is very difficult to imagine how a flower with one spurred petal could give rise to one with five spurred petals by a series of mutational steps, in each of which a balanced type of flower with survival value was produced.

I am therefore a confirmed believer in the evolutionary importance of large mutations. The changes in basic chromosome number which we frequently find in passing from one genus to another appears to represent a condensed series of events resulting shortly in a new balance of the chromosomes. This serves as the stable starting point for a new genus, in which other mutational processes of various kinds will then proceed to produce a series of new species or even a whole sub-family, as in the Pomoideae. Amphidiploidy, now known in so many wild species of plants, is a method by which a new species arises by two marked steps, one of which is a mutation.

From such considerations it seems impossible to separate macro- from micro-evolution. Both must have played their part, since gene mutations occur in the same line of descent in which chromosome rearrangements have taken place. Evolution is a very multiform process. Mutations may be of any size, from minute differences only demonstrable by statistical methods to large mutations and chromatin rearrangements and finally to "hopeful monsters." Each group of organisms has moreover its own evolutionary problems and principles, depending on its ecology and its reproductive methods.

My views differ from those of the neo-Darwinians in assuming that wide jumps occur as well as the small mutations of recent Mendelian work. I differ from the macro-evolutionists in assuming that the small mutations as well as the large ones have evolutionary significance. This brings my position in some respects much nearer to that of deVries in the original statement of his mutation theory.

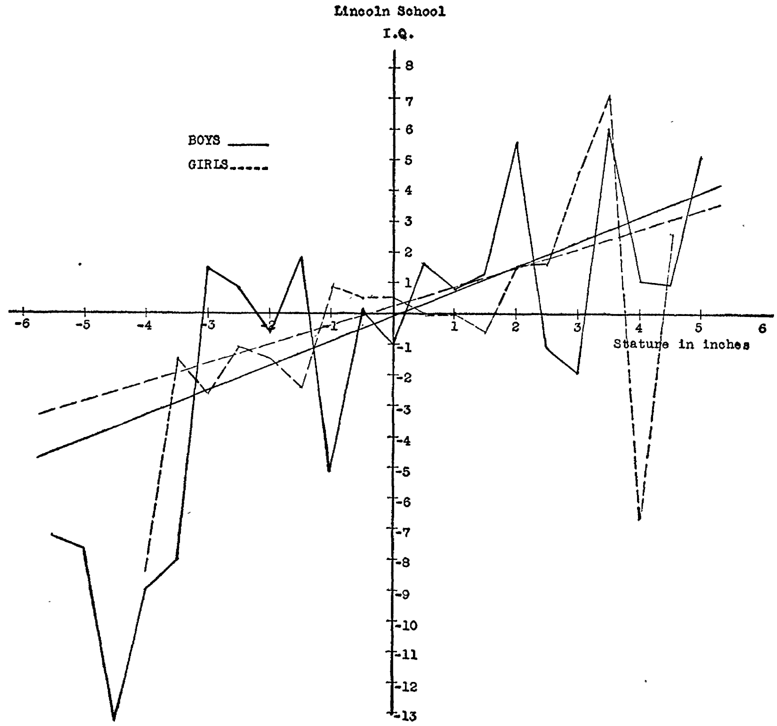
THE RELATION BETWEEN PHYSICAL AND MENTAL DEVELOPMENT

By Professor FRANZ BOAS
COLUMBIA UNIVERSITY

UNTIL recent years anthropological investigation has proceeded on the assumption that populations are stable. The significance of the increase of stature during the past century, which has been known for a long time, has been assumed to be the effect of bettered social conditions without leading to the question in how far other bodily features may be affected. Selection has been considered as leading to differentiation of social or local groups, but actual physiologically determined changes have hardly been considered at all. In other words, the form of the human organism has been considered as entirely determined by hereditary causes. In 1910 I was able to show that the proportions of the body are not stable, but that, taking a population as a whole, children differ in bodily form from their parents. These data have been vigorously contested, although a simple consideration shows that every organism must bear the impress of its environment and that an absolute form without environment does not exist. I believe this view is now fairly generally accepted, particularly after Dr. Shapiro's corroborative studies on the differences between Japanese born in Japan and their descendants born in Hawaii, and Bowle's studies of Harvard students and their descendants.

The observed changes in proportion of the body have led to the problem of the interrelation between these changes. Every change of proportion indicates that the rates of change of the measures involved are unequal. It is not possible at the preesnt time to study the reaction of a given anatomical type to various environmental conditions, and there is little hope that the problem can be solved by this method. It seems more promising to study the interrelation between tempo and rates of growth and development of various parts of the body, because these will determine the ultimate proportions of the adult. The investigations of constitution and of the effects of internal secretion upon growth indicate where we have to look for modification of bodily proportions. Anthropometric investigations relating to the tempo of development will throw light upon our problem. It has been known for a long time that during adolescence there is a rapid increase in the rate of growth. This occurs in both sexes, in girls nearly two years before it takes place in boys. So far as we know the whole skeleton participates in this change of tempo of growth, but not in equal amounts. The extremities grow more quickly than the trunk until adolescence, while later on the ratio is reversed. The size of the head has its most rapid growth in very early years. The development of teeth follows different laws. The age at which

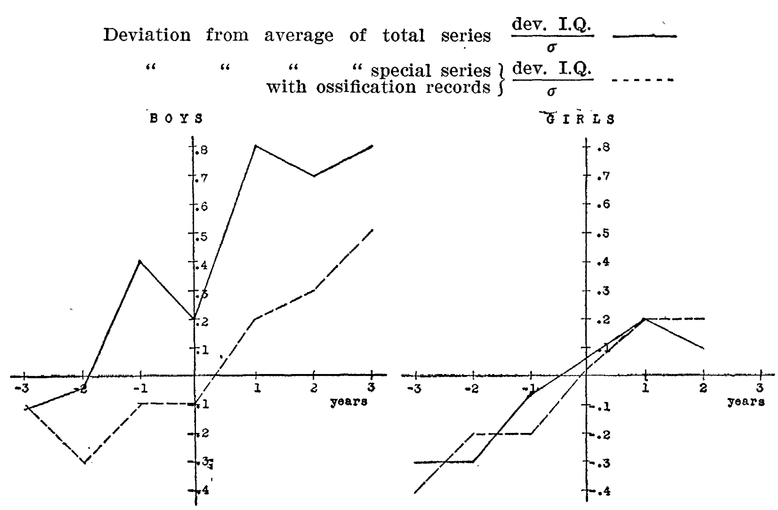
Relation between stature and I.Q. in terms of the deviation from average I.Q. determined by deviation of stature.



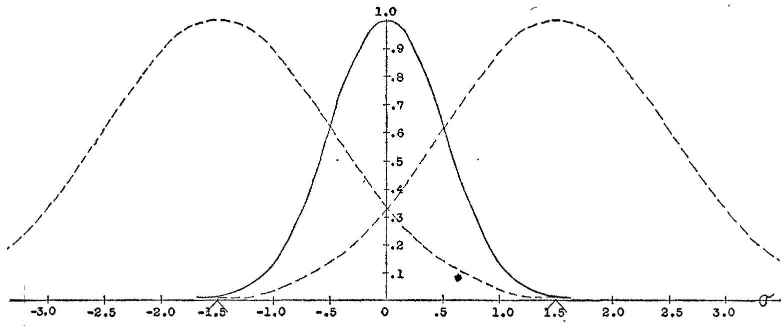
the same stage of tooth development is reached by boys and girls differs not by two years, as does the skeletal growth, but by a few months only. I showed also in 1912 that the accelerated development of girls which brings it about that the rapidly growing parts of their skeleton equal or exceed the corresponding measures of boys hides the sexual differences in size which exist from birth on. These appear clearly in all slow-growing parts of the skeleton.

The general problem can not be solved by a study of size. We have to extend it so as to include the relations of physiological functioning to anatomical structure since the form depends largely upon physiological conditions. As a further step it should be correlated with psychological observations. Crampton studied in 1908 the time of appearance of pubic hair. In 1912 I investigated the tempo of physiological development by observations on the time of appearance of a number of features, such as ossification of epiphyses, time of minimum and maximum rate of growth,

Relation between I.Q. and ossification of radius in terms of the deviation from average I.Q. determined by deviation of x-ray age.



Variability of family lines and fraternities
Lincoln School



eruption of teeth, menarche and menopause and found a very rapid increase of the variability of the tempo of development. During the last few years it has also been shown repeatedly that the tempo of physiological development has increased. From material kindly placed at my disposal by Dr. Milo Hellman, it appears that the eruption of teeth has been accelerated by five months from 1915 to 1925. Dr. Michelson has found an acceleration of four months from 1930 to 1938 in the menarche. Throughout, these investigations have proved that the tempo of physiological development is closely related to anatomical growth, so much so that it has been suggested that physiological age should be substituted for chronological age. This, however, is not justifiable, because the average child, let us say of twelve years, is not equal to the ten year old with an acceleration of two years. Furthermore, the degree of acceleration in regard to one feature, for instance sexual maturity, does not influence skeletal development as strongly as the time of maximum rate of growth; and the same is true in regard to eruption of teeth and skeletal growth.

Data in regard to the relation between bodily and mental development are very scanty. Most suggestive are the observations of Shuttleworth of 1938 on the relation between mental age and period of maximum growth. During the past year I studied under a grant from the Carnegie Corporation the relation between skeletal growth and development and the intelligence quotient. Although I am fully conscious of the limited significance of the intelligence quotient, it may be said that it is an expression of maturity and of intelligent use of experience, and in this sense significant in a group that has essentially the same range of experience. The material was obtained from Lincoln School with the generous help of Dr. Gertrude Hildreth; measurements and x-ray films of hands and wrists were made by Dr. N. Michelson and Dr. W. Nussbaum, the calculations were made by Dr. D. Trachtenberg. A study of the data collected shows an unexpectedly high relation between intelligence quotient and stature. Children short for their age had an intelligence quotient markedly under the norm, those tall for their age, one above the norm. The groups short and tall contain a hereditary element that is probably less pronounced in the tempo of ossification. The latter shows the same type of relation to the intelligence quotient. Retarded children have a lower intelligence quotient than those accelerated. This corroborates the inference drawn by me from Porter's early observation. He found that children of the same age are the taller the higher the grade they had attained in school. In other words, their physical and mental development went hand in hand. The close correlation between anatomical and psychological traits in childhood must be interpreted as due to the influence of the tempo of physiological development over the body and its functions. The important question remains open whether in the further course of life, when the variations in tempo become greater and greater, similar relations may be found. Unfortunately we have no data on this subject. The available observations certainly do not prove that normal bodily traits that are not affected by acceleration or retardation have any effect upon mental status.

I have also investigated the question in how far heredity may be expressed in the results of our investigations. I have repeatedly pointed out that the coefficient of correlation which is often used to express hereditary resemblance is merely an expression of the heterogeneity of a population. It is readily seen that when in a large population all families were of the same character it would be impossible to identify a child by its resemblance with its own parents, because all the parents would be alike. The correlation would be zero. If on the other hand all the families are very diverse, the similarity between children and parents will be apparent. The more diverse the families, the greater will be the correlation between siblings. It follows from this that the greater the inbreeding in a population, the lower the correlation between members of the family. I have demonstrated this by studies of city populations and small inbred communities.

We may consider every individual as a member of the fraternity to which he belongs. The fraternity represents a family line. The whole population consists of many distinct family lines and every family of a group of siblings. The variability of siblings is determined by heredity, that of the family lines by the degree of inbreeding in the population and the diversity of the constituent lines. My investigations of New York populations from this point of view gives for all the traits investigated, tempo of development, head form, and mental development as expressed by the intelligence quotient very nearly the same relation between the variability of family lines and that of fraternities, the latter being very nearly one half of the former. This value has been found not only in the selected New York groups, such as Horace Mann School, Lincoln School, Hebrew Orphan Asylum, etc., but also for the so-called European races. To realize the importance of this result I have indicated in a diagram the distribution of family lines and overlaid it with the variability of the fraternities for those family lines that stand on points one and one half times the standard deviation of the family lines below and above the norm. It will be seen that very few of the individuals belonging to the two sets of fraternities are equal. This is one of the strongest proofs showing that it is an utter error to ascribe the same qualities to a whole population—too often called a race. The genetic lines composing a race are so varied that the assumption that all members are by heredity endowed with the same physiological and mental characteristics is as absurd as to claim that they are all physically alike.

OBITUARY

SAMUEL HENSHAW

SAMUEL HENSHAW was born in Boston on January 29, 1852, and died in Cambridge on February 5, 1941. Of an old Boston family, he was the son of Joseph Lyman Henshaw and Jane Paine Henshaw. He had a sister, Elizabeth Lyman Henshaw, who died on October 24, 1926, and a brother, Joseph Putnam Bradlee Henshaw, who died on October 11, 1930. Samuel Henshaw married Miss Annie Stanwood on April 28, 1886. She died on March 12, 1900. Her death was a desperate blow that radically changed all his subsequent life.

Henshaw went to the Chauncy Hall School and the Boston Latin School. He did not go to college, but received an honorary A.M. from Harvard in 1903.

Henshaw was short in stature; but very strong. He had wonderful eyesight so that he could read the finest print and he never wore glasses, even in his old age. He had a very keen and critical mind in affairs and in his relations with men. This gave his judgment great weight with his associates. The keynotes of his character were his sensitiveness, his sense of fun, his personal integrity and his devotion to his work and his friends.

Primarily an entomologist, he had wide knowledge in other groups of animals, and his knowledge of scientific literature was both very great and very accurate. This helped him greatly to his editorial work, which was very extensive throughout his active years. He was gifted with a singleness of purpose and an intense application to work that are rarely equalled.

All his life interested in natural history, he became a member of the Boston Society of Natural History in 1871. In 1876 I first find record of his working on the collection of insects in the society. From that time on, until 1891, Henshaw was working at the Boston Society as assistant to Professor Alpheus Hyatt. While insects were his primary interest, he worked on various groups of invertebrates and vertebrates as well, and assisted Professor Hyatt greatly in preparing material for his courses in the Teacher's School of Science, a department of the Lowell Institute.

In 1892, Henshaw became secretary and librarian of the Boston Society of Natural History, which position he held until 1901, when he was succeeded by C. F. Batchelder, and a year later by Glover M. Allen.

In 1891–1898, Henshaw was part-time assistant in entomology at the Museum of Comparative Zoology, succeeding Dr. H. A. Hagen, who had retired on account of illness. From 1898–1903 he was assistant in entomology and librarian of the Museum of Comparative Zoology. From 1903–1911 he was curator of that museum, when his title was changed to director, and this position he held until November, 1927, when he was succeeded by Dr. Thomas Barbour. From 1927 his title has been director of the University Museums and of the Museum of Comparative Zoology Emeritus.

Henshaw was a fellow of the American Academy of Arts and Sciences, member of the American Society of Naturalists and American Society of Zoologists. He was one of the founders of the Cambridge Entomological Club, which started publishing *Psyche* in 1874, and for a time he edited that journal.

While secretary of the Boston Society of Natural History, he edited their publications and during at least a large part of his connection with the Museum of Comparative Zoology, he edited the publications of