in 1931 will be held in Lincoln and the 1932 meeting in Omaha.

THE New York State Geological Association held its sixth annual field meet at Union College, Schenectady, N. Y., on May 16 and 17, under the presidency of Professor Edward S. C. Smith. The first day was spent north of Schenectady near Saratoga and Schuylerville. The Precambrian, Cambrian and Ordovician rocks were studied. The Cryptozoon ledge, diabase dike cutting Grenville sediments, the overthrust at Bald Mountain and Stark's knob at Schuylerville were of special interest. That evening at the Mohawk Golf Club, President Day and Professor Emeritus Stoller, of Union College, addressed the geologists. Professor Stoller spoke of the Pleistocene and Post-Pleistocene drainage changes in the immediate region. On Saturday, the Helderberg escarpment and the Schoharie district were visited, a region of particular interest to paleontologists. Fossils were collected from the Devonian and Silurian outcrops. In the evening the members of the New York State Museum were hosts to the geologists in the museum in Albany. Two hundred students and their instructors, representing twenty-four educational institutions, attended the field meet. Next year the association plans to meet in the Mineville-Port Henry district of the Eastern Adirondacks. Professor Harry N. Eaton, of Elmira College, was elected president, and Professor O. D. von Engeln, of Cornell University, secretary.

THE annual field excursion of the section of geology of the Ohio Academy of Science was conducted in eastern Ohio on May 30, 31 and June 1. Special attention was given to the physiographic features of Guernsey, Belmont, Monroe and Noble Counties. Studies were also made of the stratigraphy of the Pennsylvanian and Permian systems, and the structure of the Cambridge anticline and the Parkersburg-Lorraine syncline. Fifty-seven people participated in the excursion, which was conducted by Wilber Stout, state geologist, and Paris B. Stockdale, vice-president of the section of geology of the Ohio Academy of Science.

DISCUSSION

RACE MIXTURE AND PHYSICAL DISHARMONIES

PROFESSOR H. S. JENNINGS in his recent book on "The Biological Basis of Human Nature" devotes a chapter to the subject of race mixture and its consequences. Considering first the purely physical results, he mentions both advantages and disadvantages resulting from wide racial crosses. As an advantage he reckons hybrid vigor and the covering up in the immediate offspring of any recessive defects which may be present in either parent race. As a disadvantage he mentions possible disharmony in details of structure. It is to this latter point that I wish to give brief consideration, as it is a matter of considerable biological importance apart from its human interest.

Jennings says, on page 280:

> Working probably to the disadvantage of some race mixtures in man is the fact that certain human races differ in such ways that union of their characteristics may yield combinations that are in details inharmonious. In the mixture of races found in the United States, as Davenport[1] has pointed out, some of the stocks differ greatly in physique from others. Some are smaller, having organs that go with a small body—small heart, small kidneys, small jaws, small teeth; such on the whole are the races that come from the Mediterranean region of Europe. Others have large bodies, with large kidneys, heart, jaws, teeth, and other organs.
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> Judging from what occurs in other organisms, when such diverse races are crossed, the offspring, receiving genes from both sides, may well develop combinations of parts that lack complete harmony. If a large body is combined with small kidneys, the latter may be insufficient for the needs of the individual. Or a large body might be combined with a small heart that would not keep the blood properly circulated. Large teeth, resulting from the genes of one parent, may be crowded in a small jaw that results from the genes of the other parent. In consequence the teeth decay. Partly to it, Davenport (by whom the examples given above are suggested) ascribes the prevalence of defective teeth in the United States. According to him, crowded and defective teeth are less common in nations with races less mixed.
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> It is difficult to measure with certainty lack of harmony between body size and size of kidney or heart, so that direct proof that the possible inharmonious combinations mentioned above actually occur in man as a result of mixture of races is not available. But the occurrence of inharmonious combinations of certain bodily parts as a result of race crossing has been observed both in man and in other organisms. A striking case of this kind in the dog—comic rather than tragic in its consequences—is described by Lang.[2] A great St. Bernard dog was crossed with a dachshund. Some of the progeny had the large heavy body of the St. Bernard, resting on the short crooked legs of the dachshund. The result (figure 49) was neither beautiful nor efficient.
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> The occurrence of inharmonious combinations, in human race crosses, has been shown with respect to parts of the body that are measurable, in the recent study

[1] C. B. Davenport, "The Effects of Race Intermingling," *Proc. Am. Phil. Soc.*, 56: 364–368, 1917.

[2] A. Lang, "Experimentelle Vererbungslehre," Vol. I, p. 727, 1914.

made by Davenport and Steggerda[3] on the whites and blacks in Jamaica, and on the browns resulting from crosses between them. The whites have relatively short legs and long bodies, while the blacks have relatively long legs and short bodies. "Some of the mulattoes have an unexpected combination of long legs and long body and others of short legs and short body" (Davenport). Again, in the blacks, arms and legs are both long; in the whites, both are shorter. Some of the crosses have "the long legs of the Negro and the short arms of the white which would put them at a disadvantage in picking up things from the ground."

Since inharmonious combinations of physical characteristics that are thus open to precise study are shown to occur as a result of race crossing, it appears probable that similarly inharmonious combinations of a more serious character may likewise occur, giving rise to insufficiency of heart or kidneys, or to crowding of teeth, as suggested in a former paragraph.

(1) As to the large-sized dog with short crooked legs and of St. Bernard-dachshund ancestry, what is the evidence that its lack of beauty and efficiency is a consequence of race mixture? Is not the dachshund equally lacking in beauty and efficiency? The crossbred has inherited the same dominant achondroplastic gene which is possessed by all dachshunds. There is no more disharmony in the progeny than in the dachshund ancestors. The dachshund breed is itself a race of hereditary defectives.

Suppose that a white man who was affected with Huntington's chorea should marry a Negro woman and half their children should prove to be choreic (as in all probability they would), could we ascribe this unfortunate occurrence to race mixture? By no means, the same result would have followed had the wife been a white woman.

(2) As to the supposed occurrence of disproportionally small kidneys and hearts in crosses between races of southern and northern Europe, there is more than room for doubt; there is a complete *vacuum* of evidence. I would invite any anatomist who has studied the size and efficiency of human kidneys and hearts to produce evidence that the size of these organs in relation to general body size is more variable in crossbreds or less closely correlated with general size than in either pure Nordics or pure Mediterraneans (if he can find such). The fallacy involved in this case is one which Jennings in other parts of his book has pointed out, that of supposing the anatomical parts of the body to be unit-characters and so independent of each other in their inheritance. In his zeal to avoid the fallacy Jennings condemns the term unit-character altogether, yet is himself guilty of endorsing the idea that a small kidney or small heart as such might be inherited from a small ancestor, and a large body to contain them from a large ancestor. Any one who has studied size inheritance in a mammal would know that nothing of the sort can be detected in size crosses much more extreme than any possible in the human species. For example, in rabbits races have been crossed, one of which had four times the average weight of the other. Each of these races was quite uniform in weight, more so probably than any human race. The crossbreds were with equal uniformity of an intermediate size, and in the second crossbred generation (F_2) no excessive variability was found or the slightest evidence of disharmonies, though an express search was made for them in a careful study of the correlation between skeletal parts among themselves and in relation to total body weight. It was found that when, by crossbreeding, races of intermediate size are produced, all parts of the skeleton are also intermediate and suited to each other, all being developed under a common influence, the fundamental rate of growth, which is the master controller of all general ontogenetic processes from early cleavage stages on.[4]

(3) As to the reputed occurrence of inharmonious combinations between parts of the body that are measurable, in the recent study made by Davenport and Steggerda of whites, blacks and browns in Jamaica, let us appeal from the generalizations to the detailed evidence presented by these authors in their final report. In over a hundred photographs of individuals in varied attire we look in vain for indications of one having "the long legs of the Negro and the short arms of the white, which would put them at a disadvantage in picking up things from the ground." I should greatly like to see such a picture or any other sort of evidence that a mulatto was at a physical disadvantage in picking up things because his arms were too short and his legs too long. Yet Davenport and Steggerda say in their summary of conclusions (p. 473) "The leg of the blacks is much longer than that of the whites." Turning to the detailed measurements on which this conclusion is ostensibly based (p. 119), we find the following statistics concerning leg length (in cm) in adult male blacks, browns and whites.

	Blacks (53)	Browns (91)	Whites (48)
Mean and P. E.	92.5 ± 0.4	92.3 ± .03	92.0 ± 0.4
σ and P. E.	4.8 ± 0.3	4.8 ± 0.2	4.2 ± 0.3
C. V.	5.2 p. c.	5.2 p. c.	4.6 p. c.

The reputed "much longer" leg length of the blacks turns out to be on the average longer by five tenths

[3] C. B. Davenport and M. Steggerda, "Race Crossing in Jamaica," Pub. No. 395, Carnegie Institution of Washington, 516 pp., 1929.

[4] Castle, *Jour. Exp. Zool.*, 1929.

of a centimeter! The leg length of browns is strictly intermediate between that of blacks and whites and not more variable, since the standard deviation (σ) and coefficient of variability (C. V.) are exactly the same for blacks as for browns. Surely a leg longer by three tenths of a centimeter would not be a serious physical handicap to a brown in competition with a white. Actually we look in vain for the extra-long-legged brown. No single brown has longer legs than the longest-legged whites, and two browns have shorter legs than any white.

But perhaps it is leg length in relation to total stature that puts browns at a physical disadvantage in relation to whites and blacks. Let us see. On page 45 we find the following data on total height of adult males of the three groups.

	Blacks (54)	Browns (93)	Whites (50)
Mean and P. E.	170.6 ± 0.6	170.2 ± 0.5	172.7 ± 0.7
σ and P. E.	6.8 ± 0.4	6.7 ± 0.3	6.9 ± 0.5
C. V.	3.96 p. c.	3.97 p. c.	3.97 p. c.

On the average whites are two centimeters taller than blacks and browns, but the variability of all three groups is substantially the same, although that of browns is the lowest, notwithstanding the fact that it is the most numerous group, and if recombination occurs in browns of genes for tallness and shortness obtained from whites and blacks respectively, we should expect browns to be most variable. Accordingly, I am at a loss to see where either Davenport or Jennings finds justification for the idea that the brown Jamaicans have dangerously disharmonious combinations of stature and leg-length.

But according to the quotation, it is the short arms inherited by the browns from the whites which, combined with the supposed long legs derived from the blacks, put them at a disadvantage. Let us see how great the difference in arm length is. On page 88 (Davenport and Steggerda) the following statistics are given for arm length (acromion-stylion) of adult males.

	Blacks	Browns	Whites
Mean and P. E.	57.3 ± 0.3	57.9 ± 0.2	56.8 ± 0.3
σ and P. E.	3.1 ± 0.2	3.1 ± 0.1	2.9 ± 0.2
C. V.	5.5 p. c.	5.4 p. c.	5.2 p. c.

The difference in mean arm length between blacks and whites is five tenths of a centimeter, exactly the same as in mean leg length. In other words, there is evidently a very close correlation in man as in rabbits between arm length and leg length. The blacks have longer arms as well as longer legs than the whites, but the difference is very small, five tenths of a centimeter.

Now let us suppose that the worst fears of Davenport and Jennings were realized and a brown inherited "the long legs of the Negro and the short arms of the white, which would put them [him] at a disadvantage in picking things up from the ground," how much farther would he have to stoop in order to reach the ground with his finger tips? *Just one centimeter!* I do not think that an employer would find a brown so constituted at a serious disadvantage as a field hand in comparison with either blacks or whites. As a matter of fact, I very much question the independent inheritance of leg length and arm length in man. I think if the matter were studied from the records of Davenport and Steggerda (which they do not publish in a form available for this purpose) it would be found in man, as I have found in rabbits, that a close correlation exists between the dimensions of upper and lower extremities. The coefficient of correlation in rabbits was found to be for femur-humerus, 0.91; for tibia-humerus, 0.90; for femur-tibia, 0.93 (Castle, 1922).

If there were unit-character inheritance of the difference in arm length and leg length between blacks and whites, then we should expect to find browns more variable than either blacks or whites through recombination of units. This they are emphatically not, according to the statistics of Davenport and Steggerda. Hence we may dismiss as wholly unsupported by evidence as yet produced the idea that a brown *could* inherit "the long legs of the Negro and the short arms of the white." Even if he did, it would be a matter of no consequence, as Jennings would doubtless admit. He cites it merely as an indication that "similarly inharmonious combinations of a more serious character may likewise occur, giving rise to insufficiency of heart or kidneys." Since the evidence of the assumed independent inheritance of arm and leg length is wholly lacking, the case is valueless to support an apprehension itself without other support and almost incapable of verification.

We like to think of the Negro as an inferior. We like to think of Negro-white crosses as a degradation of the white race. We look for evidence in support of the idea and try to persuade ourselves that we have found it even when the resemblance is very slight. The honestly made records of Davenport and Steggerda tell a very different story about hybrid Jamaicans from that which Davenport and Jennings tell about them in broad sweeping statements. The former will never reach the ears of eugenics propagandists and Congressional committees; the latter

will be with us as the bogey men of pure-race enthusiasts for the next hundred years.

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THE PLANT QUARANTINES ONCE MORE

THE article by Professor E. O. Essig in a recent number of SCIENCE[1] presents a fair statement of the point of view of those who support the program of federal and state plant quarantines which has reached its most conspicuous phase in connection with the invasion of Florida by the Mediterranean fruit-fly. And as such a statement it reveals clearly the weaknesses in the arguments of the supporters of these measures. The most pronounced of those weaknesses is the refusal of these supporters to face squarely certain disagreeable facts.

Professor Essig sums up the value of the fruit industry in the area where the fruit-fly might live and contrasts its annual crop value of an estimated $240,000,000 with the paltry sums aggregating—according to his figures—$5,250,000, which have been expended by governmental agencies in the war against the Mediterranean fruit-fly in Florida. There is in his article no intimation that this is not a full and adequate statement of the case. Nor is there any recognition of the fact that a proper accounting will charge against the quarantines and their associated measures not merely the sums of money expended by government officials in their administration but all other sums for which the quarantines are responsible.

The actual damage done by the fruit-fly in Florida through the spoiling of fruit and rendering it inedible was relatively insignificant. It was so small that it had not impressed itself upon the consciousness of the growers. But the application of the quarantines, with their restrictions upon marketing of fruit and vegetables, brought about bank losses which have been estimated at $60,000,000. I know of no estimates of the losses suffered by growers and not included in the bank losses, but surely an estimate of $5,000,000 would not be too high. We have then, instead of about $5,000,000, at least $70,000,000 to be charged up as the cost of the "protective" measures which were undertaken. This is an amount equal to more than one fourth of the annual value of the crop which is being protected. Furthermore, it should be noted that only a part of this fruit crop is subject to infestation by the fly, for the evidence indicates clearly that many fruits will not be attacked in any significant degree. It would probably be closer to the facts to estimate that the actual costs in Florida for the first year of the campaign against the fly which are to be charged up to the restrictive and control measures alone and not to the damage done by the insect are equal—on the basis of the figures given by Professor Essig—to at least one half the value of the fruit crop for one year in the area subject to infestation. And at the present writing the fly is not yet eradicated.

The ignoring of such facts as this is accompanied by an equally obtuse refusal to face certain other facts. The monetary losses of the fruit-fly campaign have been accompanied by the development of sectional bitterness that it will take long to remove and that is perhaps as important as are the financial aspects. Such effects can not be set aside as unimportant. The effect of the quarantine measures in stirring up intersectional and interstate resentments and competition outside of the normal and legitimate range of commercial rivalry is a dangerous aspect of their results. They set up what are in effect prohibitive tariff walls among the states, walls which under our national organization are not supposed to exist. They function also as prohibitive international tariffs to which consumers have every right to object.

No reference to such effects is to be found in the rosy picture of the plant quarantines which is presented by Professor Essig. Nor is any reference there to be found to the absurdities of quarantines directed against wind-borne insects, to the absurdities of intercounty quarantines, to the inadequacy of training of personnel, to the gaps that are left open by quarantine administrators, to the cost of a service to which adequate salaries would attract adequately trained men and which would permit the actual achievement of the functions for which the quarantines are presumably established. These are things which the supporters of the quarantines may discuss in private, but in public never.

That the effectiveness of the quarantines has in some cases been greatly overrated is indicated by the following example. In the mimeographed copy of a speech delivered in December, 1929, by Mr. G. H. Hecke, state director of agriculture of California, there is contained the following statement:

> For eight years the alfalfa weevil has been halted at the California line, although it jumped from the Salt Lake Valley in Utah into the Reno-Sparks district of Nevada in twenty-four months. Unrelenting quarantine vigilance has kept it from the major alfalfa-producing districts of this state.

I have available an authoritative statement of the actual facts concerning the spread of the alfalfa weevil. According to this statement the weevil was present in Salt Lake County, Utah, before 1905. It appeared in Washoe County, Nevada—which is the

[1] SCIENCE, N. S. 71: 350–353, April 4, 1930.