INFORMATION CONCERNING SUMMER PLANS OF AMERICAN MEN OF SCIENCE

MEMBERS of the American Association for the Advancement of Science who are to be away from their regular addresses for the summer, especially if they are to be engaged on scientific expeditions or missions or if they are to attend meetings of learned societies abroad, are asked to inform the permanent secretary's office as to their plans, giving information by which they may be reached during their absence. If this request meets with the general response the permanent secretary's office may function as an exchange for information on the whereabouts of members during a period when they are sometimes difficult to locate. The permanent secretary hopes to inaugurate in this way a useful service to members and to the work of the association. The file of summer plans and addresses may serve as a basis for subsequent study regarding the summer doings of American workers in science. The Washington office is always glad to supply information to members as to the whereabouts of other members, as far as such information is at hand.

BURTON E. LIVINGSTON,
Permanent Secretary

THE QUANTITATIVE THEORY OF SEX

IN a note published in SCIENCE (Vol. 65, Number 1675) Dr. Riddle complains that the present writer when publishing a note on the quantitative theory of sex (SCIENCE, Vol. 64, p. 299) made no reference to Riddle's work, which also aims at a quantitative theory of sex. I am still unable to see where in the note in question I could possibly have mentioned Riddle's work, which in no point whatsoever has anything to do with the quantitative relation or balance of sex-genes, with which I was dealing. As far as I can see the only point in common between what I call—rightly or wrongly—my quantitative theory of sex and Riddle's views about metabolic rate and sex is the word "quantitative." Riddle wants me to write "*A*" instead of "*The*" quantitative theory; I am gladly willing to do so. But his following remarks can not pass without comment. According to Riddle my work is "unimportant" for *the* quantitative theory of sex, as developed by him. "This [*i.e.*, Riddle's] particular theory rests essentially on the demonstrated fact that the entire normal genetic equipment (or the chromosomal determiners) for femaleness may, under experiment, be made to produce a male and *vice versa*; and that intermediate stages of sexuality may be thus produced." I had always believed that this was exactly what I have accomplished, the production of all stages of intersexuality between males and females and *vice versa*; and sex reversal in both directions within the genetic constitution of the original sex. I had also always believed that my theory was first derived from and for the explanation of these experiments. Now I am informed that my work has only to do with normal sex-determination, the facts of which form, according to Riddle, only an unimportant part of the present question. In the passage in question Riddle prints *normal* factorial basis in italics. This might possibly mean that in my experiments the factorial basis was not normal on account of crossing. But this can hardly be the case, as some of Riddle's own main arguments are based on work in hybridizing doves. Thus I am completely at a loss to understand why the facts of first importance, according to Riddle, are "indeed unimportant," when they happen to be found by myself.

But, since Riddle has now raised the point, I may be permitted to explain why the present writer (like most geneticists) has always been unable to accept Riddle's contentions as proven facts. I am not discussing now the recent work of many writers on actual sex-reversal in birds and toads, but the old work of Riddle on which his contentions are based. Riddle has stated over and over that he produced experimentally all stages of intersexuality and further sex-reversal. After repeated search among his papers I have completely failed to find any proof of the production of intersexuality. The only facts which I could find were the claim that after crossing of pigeons females were produced, which besides being morphologically normal and behaving normally towards males, showed abnormal mating instincts towards females, namely, acting in different degrees like males. Let us compare this with the work in moths, where intersexes were experimentally produced which in every organ of their body, including the gonads, showed all transitions from one sex to the other. As long as Riddle can not produce any other evidence of intersexuality than these mating instincts, no geneticist will accept his claim of experimental production of intersexuality.

His second claim is the experimental production of sex-reversal by reproductive overwork and by crossing. This contention is based on the assumption that the first egg of a clutch in pigeons is male, the second female. In family crosses the otherwise normal female produces only male offspring; generic crosses produce male offspring in spring, after overwork in autumn only females. From these and similar data Riddle draws the conclusion that sex-reversal within the egg has been accomplished. Further evidence for this is given in a number of tests, mostly dealing with egg-size and stored energy. As I have recently ("Ergebnisse der Biologie," II, 1927) discussed these things I do not want to repeat the details here. It is simply a fact that no genetical or embryological proofs of sex-reversal have been furnished in this case (both have been amply furnished in my work with moths), that all evidence is circumstantial and in addition based on assumptions of the nature of a *petitio principii,* as shown in my recent discussion. With all due respect for Riddle's physiological and biochemical work involved in the study of his case, I must state that he has never produced (or never published) experimental intersexuality and never proved experimental sex-reversal or made it even probable.

Has then Riddle's theory of sex determination by different metabolic rates within the eggs in spite of its poor foundation, helped in any way towards an understanding of sex-phenomena? I am unable to see such a success. The theory already fails in the normal case of male heterogamety; it fails in such cases of female heterogamety as the gipsy moth, where the same egg can be made to develop into a female, a male, a female intersex and a male intersex if fertilized with the proper sperm; it fails in such cases as Drosophila where a normal egg gives a male with a Y-sperm, but an XX egg a female with the same sperm. Of course, Riddle might say that in all these cases the metabolic rate has been changed. But we want to know why it has been changed. There is nothing new in the idea that in the last resort sex, like everything else, is the outcome of chemical processes, which might also be called metabolic. To measure metabolic processes in the two sexes is certainly very meritorious work; but in my opinion it has not led a single step towards an understanding of the problems of sex-determination, although the methods used are quantitative in nature.

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BERLIN-DAHLEM

THE ORIGIN AND ANTIQUITY OF MAN; A CORRECTION

MY colleague, Aleš Hrdlička, in a letter dated May 23, 1927, has courteously called my attention to two or three serious errors in my article entitled "Recent Discoveries relating to the Origin and Antiquity of Man," which appeared in SCIENCE on May 20. I deeply regret that these errors and omissions should have been overlooked in correcting the proof and I find they are not due to mistakes of the printer.

On page 484, right column, last sentence: "We consequently reach an entirely new estimate of the brain capacity of the human race at the close of Pliocene time and the beginning of Pleistocene time, a period estimated at between 1,250,000 to 1,600,000 (not 1,000,000,000) years before our era."

On page 484, right column: The statement, "This Dawn Man has a flat vertical forehead like the modern Bushman. . . " is correct.

On page 485, left column, and on page 488, left column: The tables should have had the word 'Minimum' prefixed to the measurements of existing native and European races; as it is, the figures give a false impression. The three races especially concerned in these tables are the 'native Indian Veddahs,' the 'living broad-head race of Czechoslovakia,' and the 'average modern Swiss.' The mean, minimum and maximum, male and female ccm. brain capacity of these races is as follows:

	Male			Female		
	Mean	Min.	Max.	Mean	Min.	Max.
Native Indian Veddahs	1250	1012	1408	1139	1037	1217
Living broad-head race of Czechoslovakia	1415	1230	1800	1266	1000	1400
Average modern Swiss	1467	1200	1660	1349	1230	1510

In the above measurements in my article of May 20, assembled by my colleague, Dr. H. L. Shapiro, it will be observed that comparison was made between the skull capacity of the Stone Age races and the *minimum* capacity of existing races, namely the 'native Indian Veddahs,' min. male 1012, min. female 1037; the 'living broad-head race of Czechoslovakia,' min. male 1230, min. female 1000; the 'average modern Swiss,' min. male 1200, min. female 1230.

Dr. Hrdlička remarks, "it is a general law that the males of any people exceed the females in capacity by from 150 to 200 ccm." also, "Finally on page 488 left column you give the capacity of the living broad-head race of Czechoslovakia as 1230 ccm. for the males and 1000 ccm. for the females—which is quite incorrect, the capacity of these people, stature for stature, equaling and even exceeding (see Weisbach) that of other European nations." Unfortunately, we do not know the sex of either the Piltdown or Trinil races. In the Neanderthaloid races, the female Gibraltar brain is known to be inferior in capacity to the male Neanderthal.

Finally, the general contention of my article is sustained, namely that at the close of Pliocene and the beginning of Pleistocene time, a cube of brain capacity was attained not quite equaling the minimum existing capacity.

HENRY FAIRFIELD OSBORN

MAY 26, 1927