

### 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-