DISCUSSION

THE QUANTITATIVE THEORY OF SEX

IN a recent note in SCIENCE (Vol. 65, p. 596) Dr. R. Goldschmidt comments on an earlier communication in which the present writer (SCIENCE, Vol. 65, p. 139) questioned the completeness of Dr. Goldschmidt's proprietary rights to "The Quantitative Theory of Sex"—as claimed or implied in his initial request for "acknowledgments" from the "Columbia school" (SCIENCE, Vol. 64, p. 299).. It was our purpose to point out that "Goldschmidt can properly claim precedence" in the elaboration of "a quantitative theory of normal sex determination," but not for "the quantitative theory of sex as this has developed during the last fifteen years." In his reply Goldschmidt not only fails to assist in the clarification of this essential point, but he turns the discussion to fragments—considered by him as wholes—of various aspects of the work of Riddle which, he is distressed to find, afford "no proofs" of either "experimental intersexuality or experimental sex-reversal." The original point is a matter of interest to a relatively large group of workers, and some of his misstatements concerning studies made in our laboratory require a word of comment.

Quite apart from any question of the adequacy of supporting facts, and wholly irrespective of whether one or a dozen workers obtained the results upon which the theory is based, it is simply a fact that a quantitative theory of sex exists apart from, and was founded before, any theory of Goldschmidt's on quantitative sexuality. Further, it is a fact, sufficiently known to workers in this field, that the present writer has taken a not negligible share in the formulation of this theory, being specifically responsible for a series of views concerning the relation of metabolic rate to sex: Namely, that such a difference extends to the two kinds of gametes produced by the heterogametic sex; that here the prospectively male gametes show the higher, female gametes the lower metabolic rate; that a difference in this same direction also characterizes later (embryo and adult) stages of the development of sex; that such metabolic differences can, in experiment, override the normally controlling influence of the chromosomes—thus resulting in the sex-reversal exhibited in several animal forms; that the sex chromosomes or genes probably exercise their normal sex-determining function by aiding the establishment of a higher and a lower metabolic rate; that intersexes and hermaphrodites can arise from chromosomal or genic causes, but they can arise also from a metabolic cause while chromosomes and genes are normal; and that the metabolic distinction found can not be interpreted as a secondary sex character.

The evidence on which this theory is based has, practically from the beginning, rested on the results not only of Whitman-Riddle in doves, but those of the Hertwig school on frogs, of G. Smith on crabs, and still other work. It included the much older facts and idea of Geddes and Thomson, and had indeed such a background of varied and promising fact that it is little wonder that the great advances since made—including some more recent and still unacknowledged interpretations of Goldschmidt—have definitely turned in this direction.

In his book, Goldschmidt (1923, p. 116) says: "In the case of insects intersexuality could only be obtained through abnormal zygotic constitution, because the production of hormones is not localized in special organs, but takes place within the individual cells. In the other group (vertebrates) it is possible to obtain intersexuality independent of the zygotic constitution because the hormone production is localized in organs which can be removed or transplanted, and their action independently of the zygotic constitution which originally called them forth can in this way be investigated." If then intersexuality in insects—with which Goldschmidt worked—can only be obtained by "abnormal zygotic constitution" (hence, no plasticity here), are we not correct in placing Goldschmidt's results as important to the theory of "sex-determination," and "unimportant to the quantitative theory of sex as this has developed in the hands of others" (in whose vertebrate material plasticity is found)? Truly enough, "some of Riddle's arguments are based on work in hybridizing doves" but we think —though very erroneously, says Goldschmidt—we find a plasticity in a part of our vertebrate material indicating that one and the same zygotic constitution here does not deliver equivalent sexuality but varies according to identifiable conditions; and we have measured the concurrent gametic metabolic change that coincides with this plastic change of sex. Goldschmidt's results fall far short of this (since by his own admission "intersexuality could only be obtained through abnormal zygotic constitution"), and for this reason—not because "they happen to be found by Goldschmidt" instead of by Riddle—his results are unimportant to the quantitative theory of sex now under discussion.

The different gradations in sex behavior produced and measured by us in doves are entirely discarded by Goldschmidt as evidence of intersexuality. He wants "morphological" mixings. After wondering how, on Goldschmidt's view, psychologists and psychiatrists could ever become acquainted with "sex" in their fields—and leaving the question to our colleagues and the future—we may note that Goldschmidt should read more and better before suggesting that graduated intersexual behavior is "the only fact" provided by us for intersexuality. We freely admit that we have thus far given only short and incomplete accounts of the many kinds and cases of intersexuality encountered in our material. We acknowledge and regret, and are steadily supplementing, this incompleteness. But morphology, beloved of Goldschmidt, is I presume adequately represented by oviducts in males (*Anat. Rec.*, 1925, 31, p. 349); by persistent, even functional, right ovaries in females (*Amer. Nat.*, 1916, 50); and by the hermaphrodites listed (Whitman, 2, 1919), or referred to in connection with rather full descriptions of some other abnormal (possibly not intersexual) gonad conditions (*Brit. Jour. Exp. Biol.*, 1925, 2). If these, as yet little described, cases of hermaphroditism should lead our critic to dispose of them by the further assertion that Riddle can not properly recognize an hermaphrodite he is entirely welcome to that position.

Goldschmidt states that our "claim to the experimental production of sex-reversal by reproductive overwork and by crossing . . . is based on the assumption that the first egg of a clutch is male, the second female." This is simply not true. "Our studies on 'sex control' manage to get on whether the eggs come in normal order, reversed order or utter disorder" (*Amer. Nat.*, 1925, 59). Also, according to Goldschmidt we have "never proved experimental sex-reversal or made it even probable." Waiving the large question of proofs, we may note that calculation of probabilities in a single result obtained in one of our very few "family" crosses indicates that—apart from sex-reversal—this result "could be expected to occur only once in 9,384 trials" (*Anat. Rec.*, 1925, 31). So apparently, either Goldschmidt must read more, or in my items of data I must eliminate part of *one* chance in 9,384.

To say that "Riddle's theory of sex determination by different metabolic rates . . . fails in the normal case of male heterogamety; it fails in such cases of female heterogamety as the gipsy moth, etc.," is merely to use words without meaning. The theory was founded upon forms showing "female heterogamety" (pigeons), and early applied, successfully we think, to forms (frogs) which later proved to show "male heterogamety"; moreover, as earlier pointed out, parts of this metabolic theory were later borrowed and lugged unacknowledged into Goldschmidt's own theory of sex-determination in the gipsy moth.

Well or ill founded—and much in addition to work with pigeons forms part of its foundation—there exists a vigorous quantitative theory of sex, based on real or fanciful sex-reversal and intersexuality apart from zygotic composition (on which Goldschmidt's studies are based), and on measurements of metabolic sex distinction in all stages—ovum to adult. We and others have taken a good or a bad part in all this, and the quantitative theory of sex can not be properly discussed—as Goldschmidt would have it—as the private affair of the "Columbia school" and the laboratory of Goldschmidt.

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ZOOLOGICAL NOMENCLATURE

REFERRING to the recent referendum on Dr. Poche's (Vienna, Austria) three propositions in regard to the Rules of Zoological Nomenclature, the undersigned has the honor to report to the zoological profession the following results of the ballot:

Poche's proposition I: 8 votes for; 549 votes against.
Poche's proposition II: 4 votes for; 550 votes against.
Poche's proposition III: 4 votes for; 551 votes against.

A detailed report will be made to the Tenth International Zoological Congress (Budapest) and the undersigned unreservedly accepts the unambiguous results of this referendum as definite instructions from the profession in the United States for him to cast his vote (in the congress as delegate, and in the commission as member) against all three propositions.

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"OPALINA ELONGATA" GOURV. IS CEPEDEA SAHARANA METCALF

V. GOURVITSCH describes as new an Opalinid from "*Rana ridibunda*" from Tashkent, Turkestan.[1] He names this "*Opalina elongata.*" It is a *Cepedea* and from his description seems to be the form I have described as *Cepeden saharana* from *Rana esculenta ridibunda* collected at Biskra, Algeria.[2] It seems well to call attention to this to prevent confusion.

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QUOTATIONS

PUBLICITY AND SCIENCE

IN this day of personal horn-blowing it is refreshing to come upon a group of men who are doing great things, yet who shun publicity as they would the plague. As a matter of fact, they would not shun the

[1] V. Gourvitsch: The protozoan fauna of the intestines of frogs from the vicinity of Tashkent—in the *Bulletin* of the Government University of Central Asia, No. 14, 1926. [Russian.]

[2] M. M. Metcalf: "The Opalinid Ciliate Infusorians," United States National Museum, No. 120, 1923.