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.

C. W. STILES,
Professor of Zoology, U. S. Public Health Service

"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.

MAYNARD M. METCALF

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.