DR. JOHN R. NEAL, senior counsel for John T. Scopes, filed a petition on January 25 with L. D. Smith, state attorney general, to protect the interests of the defense pending an agreement among associate counsel as to the next move. The petition requested a rehearing in the anti-evolution case which the state has nolle prossed. It is reported that the Tennessee Supreme Court has declined to consider the petition.

AN anti-evolution bill was introduced in the Missouri legislature on January 18. The proposed law would prevent "the teaching of any theory or hypothesis in regard to the origin of life on this planet in the public schools that is contradictory of the divine account of creation as set forth in the first and second chapters of Genesis in the Holy Bible."

A RESOLUTION which would have prohibited the teaching of evolution in West Virginia's public schools was defeated in the house of delegates on January 21. The vote on the resolution was 57 to 36.

THE extensive collection of Coleoptera accumulated by the late Mr. Fred C. Bowditch, of Brookline, Mass., has been presented to the Museum of Comparative Zoology by his family.

UNIVERSITY AND EDUCATIONAL NOTES

GROUND has been broken for the new laboratories of chemistry at Harvard University which will consist of two buildings connected by a tunnel. The first and larger of these is to go up on Oxford Street between the new lecture hall and the university museum, while the second will be immediately behind it on Frisbie Place.

THE estate of Dr. Clarence A. McWilliams will go to Princeton University after the death of his two sisters, who share the $75,000 estate during their lives.

BY the will of the late Colonel George R. Hooper, of Montreal, McGill University will receive $100,000.

WASHINGTON COLLEGE, Maryland, has received a gift of $30,000 from a friend of the college, whose name has been withheld, to endow a chair of mathematics in honor of J. S. William Jones, dean of the faculty.

DR. S. CHARLETY, rector of the University of Strasbourg, has been named rector of the Sorbonne to succeed Dr. Paul Lapie, who died on January 24.

DR. WILBURT CORNELL DAVISON, associate professor of pediatrics and assistant dean of the medical school at the Johns Hopkins University, has been appointed dean of the new school of medicine to be established at Duke University. Dr. Davison will at once assume responsibility of the organization of the school and the construction of the hospital and medical college buildings.

PROFESSOR H. S. TAYLOR has been appointed chairman of the department of chemistry of Princeton University upon the resignation of Professor Lauder W. Jones. Professor Jones continues as director of research and teaching of organic chemistry in the university.

DR. WILLIAM MCDOUGALL, professor of psychology at Harvard University, has resigned to accept a position on the faculty of Duke University, Durham, N. C.

DR. CYRUS C. STURGIS, assistant professor of medicine at the Harvard Medical School and an associate at the Peter Bent Brigham Hospital, has been appointed professor of internal medicine and director of the new Simpson Memorial Institute at the University of Michigan.

ASSOCIATE PROFESSOR ABRAHAM COHEN, of the Johns Hopkins University, has been promoted to a full professorship of mathematics.

DR. WILLIAM SOMERVILLE, Sibthorpian professor of rural economy at Oxford University, has retired and has been succeeded by J. A. S. Watson, professor of agriculture and rural economy of the University of Edinburgh.

M. EUGENE BLOCH has succeded M. Leduc as professor of theoretical physics at the University of Paris.

DISCUSSION AND CORRESPONDENCE

"THE QUANTITATIVE THEORY OF SEX"

UNDER the above title R. Goldschmidt in a recent issue of SCIENCE[1] writes that "*The quantitative theory of sex* was first derived by the present writer in 1912 (preliminary note in 1911), in essentially the same form as it stands to-day, from his experiments on intersexuality in the gipsy moth. (The term intersexuality was only used since 1915.) The theory claims that in both sexes determiners for femaleness and maleness are present, the relative quantities of which are balanced in such a way that one or the other has the upper hand in the respective sexes. Which of them is to be present in the higher quantity is decided by the mechanism of the sex chromosomes, etc." He further states that "during the years between 1912 and 1922 the Columbia group of Drosophila workers was solidly opposed" to his theory of sex; that this group of workers has since arrived at conclusions essentially confirmatory of his theory and that they should have made or should now make such

[1] SCIENCE, n. s., Vol. 64, p. 299, 1926.

acknowledgement. Goldschmidt makes no reference to the work of the present writer.

If Goldschmidt had written under another title, namely, "A Quantitative Theory of *Normal* Sex-determination," his discussion would afford less cause for my comment. On this last-mentioned subject—but not on "the quantitative theory of sex"—Goldschmidt can properly claim precedence, since he found genes influencing sexuality in the autosomes and obtained evidence that these determiners were not of equal potency in various races or species of moths. But those contributions are of very subsidiary importance—indeed they are unimportant—to "the quantitative theory of sex," as this has developed during the last fifteen years in quite other hands than those of Goldschmidt. This 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. In this basic fact it is relatively unimportant whether in addition to determiners for sex in the X- and Y-chromosomes there are other determiners in the autosomes; and equally immaterial whether these factors or determiners are of constant or of variable potency in various races or species. In either case the fact of first importance is that the complete and *normal* factorial basis of the one sex has been forced to deliver the opposite sex. Whatever the facts of normal sex-determination, they form but a part—here an unimportant one—of the whole of the present question of *the quantitative nature of sexuality.*

The first evidence definitely interpreted as involving the essential fact of sex-reversal (along with a quantitative basis of sex as well) was presented by the writer in 1912[2] (paper before the American Society of Zoologists, 1911). Other evidence was given in 1914,[3,4] 1916,[5] 1917[6] and in several later papers. In succeeding years true sex-reversal has been accomplished, and so interpreted, in several animal groups by several investigators. It is now a well-established fact.

The second point of importance to "the quantitative theory of sex" (as many or most students of sexuality now regard it) is the following: What is the essential thing about sex that is quantitated? That this is *not* the complete normal genetic equipment (determiners) for normal sex-determination is plain, since as just noted above, this equipment for one sex can be made to yield the opposite or an intermediate sex. Probably this *is* the same thing that overrides the genetic equipment (also upon which the determiners normally act) and enforces the development of the opposite sex. The first evidence concerning the functional unit or process that overrides the genetic basis of sex was presented by the writer[2] in 1912 (to the American Society of Zoologists, 1911). Other evidence was given in 1914,[4] 1916,[5] 1917[6,7] and in later papers. This functional unit was found by me to be *metabolic rate.*

In 1912 (p. 463) we could give a bit of evidence that the male-producing ova of the pigeon were distinguished from female-producing ova "by smaller size, higher water content, and smaller energy content (later written as less *stored* energy)." In 1914[4] (p. 276) this size difference was confirmed and it was specifically stated that "a high *storage* power in the egg is, however, probably an index of lower *oxidizing* capacity." In later papers these points have been rather fully developed, and those American readers of SCIENCE who are students of the sex problem know that the writer has been continuously engaged in a study of this particular aspect of the problem for nearly sixteen years. Several investigators have accepted part or much of this quantitative and metabolic theory of sex.

Indeed, it may next be noted that Goldschmidt himself has somewhat belatedly accepted important parts of our theory, though he has nowhere acknowledged it. In 1916[8] he made his *first* attempt to identify his determiners (of various potencies in various races) with a functional unit or process—and chose to call them "enzymes." In 1917[9] he again considered the "male and female enzymes" as *qualitatively* different and chose to call these hypothetical bodies andrase and gynase. He had, of course, neither found nor measured variable quantities of "enzymes" associated with sex determiners of variable potency. It was wholly speculative. Our special point here, however, is that if this speculation were true it would become a beautiful evidence for the wide applicability of the metabolic theory of sex, as sponsored by myself. Enzymes, many hormones and all catalysts of whatever kind—even inorganic ones—are but tools and handmaidens in the garden of metabolism; though several writers on sex phenomena have seemed en-

[2] Riddle, O., SCIENCE, n. s., Vol. 35, p. 462, 1912. (Abstract.)

[3] Riddle, O., SCIENCE, n. s., Vol. 39, p. 440, 1914.

[4] Riddle, O., *Amer. Acad. Med.,* Vol. 15, p. 265, 1914.

[5] Riddle, O., *Amer. Nat.,* Vol. 50, p. 385, 1916.

[6] Riddle, O., *Jour.* Wash. Acad. Sci., Vol. 12, p. 319, 1917.

[7] Riddle, O., SCIENCE, n. s., Vol. 46, p. 19, 1917.

[8] Goldschmidt, R., SCIENCE, n. s., Vol. 43, p. 98, 1916.

[9] Goldschmidt, R., *Jour. Exp. Zool.,* Vol. 22, p. 593, 1917.

tirely unconscious of this fact. Later, in 1919, "hormones" (again without identifications or measurements) are added to Goldschmidt's theory of sex-development as extended to vertebrates. And, finally, by 1923, in this latter field even the word *metabolism* (no measurements) is sometimes agreeably utilized. Such definite progress is of course gratifying to the writer.

From the preceding it may appear that the writer has contributed something to the "quantitative theory of sex." Personally, I am fairly indifferent toward "acknowledgments" in favor of myself. But if it is desirable for the "Columbia group of Drosophila workers who solidly opposed" Goldschmidt's theory of sex-determination now "to make a frank statement" of its acceptance, I can merely suggest that the little matter of acknowledgments might well be extended beyond the Columbia group.

It remains to note that the past fifteen years have provided so much advance in the problem of sexuality, from contributions of so many distinct kinds, that it is probably too much to expect any single individual to be aware of the total advance. At any rate no one gives evidence of knowing more than a fraction of the available data. Perhaps the interests of the subject and of individual investigators of it will be adequately served if the main lines of progress are known, and if a part is not too often mistaken for the whole.

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GOLDSCHMIDT'S article on "The Quantitative Theory of Sex" in SCIENCE for September 24 recalled to the writer strongly, though indirectly, phenomena of sex that deserve careful and general attention.

Extended observations on human material, body, crania, bones, organs, appendages, show that sex in man manifests itself not as a uniform but as a variable character. And its variability compares with that due to other fundamental factors, such as age, race, etc., and with that of individual organic features in general.

The female as well as the male sex in man, under even the most normal conditions, is a broad stream of variations, so that one woman is by no means sexually equal to another woman, or one man to another man. The ranges of sexual variation in individual features, as well as that of sexuality as a whole, give, if we have sufficient numbers, regular binomial curves of distribution, with their usual characteristics; and these curves in their extremes do not merely approach but overlap each other. Thus there are females of hyper- to hypo-feminine, with males of hypo- to hyper-masculine grades, and the least feminine individuals stand no more apart but interdigitate with or overlap, in many respects, the least masculine, so as to be separable perhaps only by the main sex organs; while the most masculine and feminine are decidedly farther apart than the large majority, who may be termed more or less orthosexual.

Perhaps the simplest demonstration of these facts, which must be of much genetic as they are of other significance, is obtained on the human skull and bones. Taking the individual larger bones of the skeleton, the pelvis, the lower jaw, or the skull, an experienced worker will be able to positively identify as to sex from approximately 50 to 93 per cent. of the specimens, according to their kind. The pelvis, and the skull with its lower jaw, give the highest proportion of identifiable cases, yet even of these on the average about seven in each hundred present features either so intermediate that the sex remains uncertain, or approaching so close to those of the other sex that an erroneous identification is possible. Even where the complete skeleton is present there are still two to three cases in every hundred in which it is impossible to be certain whether the remains are those of a male or a female. On the other hand there are specimens of which the sex is most patent, is in fact exaggerated; while a large majority range between these two extremes. And what is true of bones is true of brains and probably of all other organs, except the genital; though even in the latter there is ample variation.

The subject of sex, seen in this light, rouses much thought; but the object of this note is merely to call attention to the broadness of the unit character of sex; to the merging of the male and female characteristics on the confines of their normal variation; and to the similarity of their behavior with that of all other organic manifestations in man, as well as, doubtless, in other living forms. Perhaps this may call for a re-atunement of our views on the sex question.

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SOME NOTES ON THE "ELDEN PUEBLO"

IN a recent number of SCIENCE (November 19, 1926) among the proceedings of the National Academy of Sciences meeting at Philadelphia, Dr. J. Walter Fewkes has presented a paper on the "Elden Pueblo" which contains several confusing statements. These have to do with its discovery and selection of the name. The author writes of the ruin: "The object of the communication was to announce the discovery by archeological methods, of a pre-historic ruin which had never been described. . . . It was