Letters to the Editor

On Printing Wings of Insects for the Study of Venation

The wing to be printed is placed between two glass slides, being careful that all folds are pressed out. A drop of alcohol will facilitate smoothing out wings that have a tendency to fold. Labels may be prepared by writing on the glass slide with India ink. The slide is then used as an ordinary negative, placed in a photographic enlarger, and printed on sensitive photographic paper in the usual manner. The length of exposure will depend upon the type and thickness of the wing being printed.

All clear or transparent wings, such as those belonging to the orders of Diptera, Hymenoptera, Homoptera, etc., may be treated in this manner. It is possible to remove the scales on the wings of some of the Lepidoptera by soaking the wings in glacial acetic acid or concentrated ammonium hydroxide for about 24 hours. The scales may then be removed by gently brushing them off with the finger.

DARRELL T. SULLIVAN

Department of Horticulture, University of Georgia

Newton and Applied Mathematics

In the leading article entitled "On the problem of applied mathematics" (*Science*, 1945, **102**, 315–320) J. H. Taylor discusses the formulation of Newton's Law of Gravitation in a paragraph beginning in the first column of page 319 and extending into the second. Although the type of discussion leading up to the formulation of an empirical law which Dr. Taylor outlines has often been used and will continue to be useful in the future, it certainly is very far from that followed by Newton in his *System of the world*. Newton himself credits the inverse square law to Hooke, Halley, and Wren; but he, in the words of Agnes Clerke (*Encyclopedia Britannica*, 14th ed., Vol. 2, p. 585), "was the only man of his generation who both recognized the law and had power to demonstrate its validity," by combining results obtained by astronomers with the dynamical principles developed by himself. One may call attention to Richard Stevenson's *Newton's lunar theory exhibited analytically* (Cambridge, 1834), in which is set forth a demonstration in modern notation. It seems to me that this or Newton's treatment in his *System of the world* would have served Dr. Taylor even better than his own discussions in exhibiting a contrast between mechanics and economics.

EDWIN B. WILSON

Written from *University of Glasgow*

Prior Use of the Rutherford Unit

Referring to the letter of S. C. Lind (*Science*, 1946, **103**, 761) regarding the suggestion of E. U. Condon and F. L. Curtiss (*Science*, 1946, **103**, 712) that the term "rutherford" be given to a unit expressing the strength of radioactive sources, allow me to point out that in a paper entitled "The sub-microscopic structure of matter" (*Colloid chemistry, theoretical and applied.* Vol. I. New York: Chemical Catalog Co., 1926), a table appears on pages 14–15 containing a chart, showing sizes of various material units at various magnifications. In this, the term "Rutherford Unit" is defined as "1,000,000 μμ, or 100,000 Angström Units." An explanatory footnote states:

> In dealing with the extremely minute sizes involved in describing nuclear diameters, it is convenient to use a term of measurement one million times smaller than 1 μμ. This I have termed a "Rutherford Unit" (R.U.), and therefore 1 R.U. $= \frac{1}{100{,}000}$ A.U. $= \frac{1}{1{,}000{,}000}$ μμ.

Since one of the greatest of Lord Rutherford's many achievements was the demonstration of the nuclear atom, it still seems to me that it is most appropriate that his name be connected with the atomic nucleus. The nucleus is the source of the tremendous energy released by what is erroneously called the "atomic" bomb, and the nucleus and its internal structure are under intensive study involving sizes conveniently expressed in R.U. as above defined, with the exception that in present-day notation μμ is written mμ.

JEROME ALEXANDER

50 East 41st Street, New York City

An Improved Synthesis of N-Methyl-L-glucosaminic Acid

The recent discovery that N-methyl-L-glucosamine (F. A. Kuehl, Jr., E. H. Flynn, F. W. Holly, R. Mozingo, and K. Folkner. *J. Amer. chem. Soc.*, 1946, 68, 536), exists as a component of streptomycin calls attention to the fact that there is no satisfactory method for its preparation. Following Fischer and Leuchs' (*Ber.*, 1902, 35, 3787; 1903, 36, 24) classical syntheses of the enantiomorphous forms of glucosaminic acid, Votoček and Lukeš (*Coll. Czech. Chem. Commun.*, 1935, 7, 424; *Chem. Listy*, 1935, 29, 308) prepared N-methyl-D-glucosaminic acid by treating an aqueous solution of D-arabinose with methylamine and hydrogen cyanide. They allowed this mixture to stand for a period of three weeks, removed the resultant tar, and then hydrolyzed successively with acid and base to produce, after acidification, an unspecified yield of the amino acid. Folkers and co-workers have stated that the same general method was applied to L-arabinose to yield the enantiomorphous N-methyl-L-glucosaminic acid. We have found that, by operating in anhydrous ethanol instead of in water, the reaction is greatly improved, and both the N-methylamine derivative and the cyanohydrin are readily isolable in crystalline form. Treatment of a suspension of L-arabinose in absolute ethanol with dry methylamine yielded L-arabinosyl-N-methylamine—m. p. 118–120°, $[\alpha]^{20}$ D +43° (initial) → +51° (60 min., water). Treatment of this compound, or of a mixture

of L-arabinose and methylamine, in absolute ethanol with anhydrous liquid hydrogen cyanide led to the ready crystallization of N-methyl-L-glucosaminic acid nitrile—m. p. 113°, $[\alpha]^{25}$ D −17.5° → −21° (50 min.) → −8.3° (final, water); pentacetate—m. p. 132–134°, $[\alpha]^{23}$ D −38° (chloroform). On hydrolysis of the nitrile with acid followed by base there was obtained, on acidification, N-methyl-L-glucosaminic acid—m. p. 236° (dec.), $[\alpha]^{24}$ D −4.6° (water). From the mother liquors of the nitrile there was isolated an amorphous product which on hydrolysis led to a crystalline acid now under further investigation.

Full details will be communicated at a later date.

M. L. WOLFROM, ALVA THOMPSON, and I. R. HOOPER
Department of Chemistry, The Ohio State University

The Rh System in the Chimpanzee

A. S. Wiener and M. Wade (*Science*, 1945, **102**, 177) have shown that the erythrocytes of chimpanzees do not absorb the Rh agglutinins from the human antisera, anti-Rh_0, anti-Rh′, and anti-Rh″, but do absorb the agglutinin from the anti-Hr serum. As this parallels the behavior of the human Rh– cells, it is concluded that chimpanzees are Rh–.

A chimpanzee died recently in the Gardens of the Zoological Society of London, and through the kindness of Prof. E. Hindle and Col. A. E. Hamerton we were provided with a sample of the blood. Absorption tests with the chimpanzee blood were clear cut and confirmed the findings of Wiener and Wade; that is to say, in the language of Fisher's theory (cited by R. R. Race. *Nature, Lond.*, 1944, **153**, 771; R. A. Fisher and R. R. Race. *Nature, Lond.*, 1946, **157**, 48), anti-D, anti-C, and anti-E agglutinins were not absorbed, but anti-c was. We found, however, that the chimpanzee cells failed to absorb the anti-e agglutinin, recently discovered by one of us (A. E. Mourant. *Nature, Lond.*, 1945, **155**, 542).

The antigen e is present in double dose on human Rh– red cells, which strongly absorb anti-e. In terms of Fisher's theory the failure to absorb either anti-E or anti-e means that the chimpanzee possesses neither of the antigens determined by the E-e locus in man. Either the chimpanzee possesses a third antigen determined by the same locus or, more probably, the locus is absent altogether.

The apparent separability of this group of Rh antigens would seem to support Fisher's belief that they are, in fact, controlled by a separate locus.

A. E. MOURANT and R. R. RACE
Galton Laboratory Serum Unit, Medical Research Council Emergency Blood Transfusion Service Cambridge, England

Misuse of the Linnaean System of Nomenclature

A. Byron Leonard (*Science*, 1946, **104**, 17) has recently criticized under this same heading certain nomenclatural usages of E. E. Dickerman and in doing so has himself so misused the Linnaean system of zoological nomenclature (as codified in the Règles Internationales de la Nomenclature Zoologique) that a protest is called for.

We have not examined Dr. Dickerman's paper, because the reference given by Dr. Leonard is so incomplete that previous knowledge is required to identify it. Since we are neither parasitologists nor helminthologists, we will not presume to discuss Dr. Dickerman's usage that is criticized by Dr. Leonard, but will merely comment on Dr. Leonard's conclusions from his stated premises.

In general terms, of course, Dr. Leonard is correct in his position that in Linnaean nomenclature all growth stages of a single species must be called by the same name for taxonomic purposes. Furthermore, the choice of names is governed by priority of publication and not at all by the ontogenetic stage named (Règles Internationales, Art. 27 and 28, not Art. 26 as stated by Leonard). But the reasoning by which Dr. Leonard arrives at a conclusion that is correct in general terms (whether these terms are applicable to Dr. Dickerman's usage or not) is erroneous. In the first place, Dr. Leonard seems to assume that the names *Cercaria* and *Proterometra* were both proposed by Dickerman as new generic names, for he argues that priority between them could be determined by page precedence in Dickerman's work. This is far from being true, for *Cercaria* was first published by Mueller in 1773 and *Proterometra* by Horsfall in 1933 (A. S. Neave. *Nomenclator Zoologicus*, 1940). Furthermore, Dr. Leonard would have realized that the question of priority must have been settled long ago unless he had thought that the names were newly proposed. Priority of publication, not page precedence, would require that *Cercaria* be employed, unless the Règles were suspended by a special act of the International Commission or unless there was some other special circumstance. For example, there is some outside evidence that helminthologists in general, and probably Dr. Dickerman, do not regard *Cercaria* as the name of a genus but of a collective group treated for convenience as if it were a genus (see Art. 8 of the Règles). If this were the case, there might be no nomenclatural conflict, since only one of these names would be available as a generic name under the Règles.

But let us pass this over and assume that both names did originate in Dickerman's paper of 1945 as Dr. Leonard seems to believe, or at least that different growth stages of the same species were called for the first time variously *Cercaria sagittaria* Dickerman (p. 37) and *Proterometra sagittaria* Dickerman (p. 39). Dr. Leonard argues that by virtue of page precedence *Cercaria sagittaria* Dickerman is the "correct name" for the species described. This is fallacious, and it is this argument by Dr. Leonard to which we wish to take vigorous exception. It is particularly important to protest this because of the widespread misapprehension on the subject that probably stems from the fact that page precedence was an important criterion in several other codes of nomenclature long ago superseded by the Règles Internationales.

In the Règles, the code under which zoologists have operated for about 50 years and which Dr. Leonard undoubtedly thinks is the authority for his position, page precedence has no role at all for determining priority