It was my fortune in Gratz to hear Dr. Rhumbler read his paper advocating the use of prefixes to generic names, by which the systematic position of the genus should be indicated. I believed then as now that there are two serious objections to his own rather clumsy scheme or to any other of like nature. It will never be adopted, and if adopted would only add to the present confusion. For in my judgment the difficulties do not mainly arise from our system of naming, but from the gigantic problem set before us by nature herself. Agassiz used to say: "Try to interpret what really exists." The Linnaean system is as good for this purpose as any other could be, and our whole literature of geographical distribution and of evolution rests upon it. Its chief faults—the needless synonymy and clumsy names—are faults of the workers, not of the system. Our rules are slowly bringing back uniformity in spite of generations of carelessness and of bad taste.

I can not believe that ignorance of the class or family, to be restored by prefixes or other permanent attachments to the word, could possibly help. No one writing in any group fails to know whether the genera he deals with are birds, insects or snakes. That is the least of our troubles. It might have been better if we had allowed duplication in different classes. It is now too late to change, because hundreds of new names have been legally adopted since the animals were separated, in this regard, from plants. It is not necessary to follow the unpleasant precedents of *Edvardotrouessartia, Asmithwoodardia* and the like. When Nichols broke out in *Microstomatichthyoborus* in 1917, I expressed the pious hope that "no one will ever attempt to break this record as to length of generic name." Such well-known records of bad taste as Ameghino's may stand as "awful examples," and the usually senseless "pseudo" may die out in time.

"A name is a name without necessary meaning" and we do not depend on it to fix our ideas of relations. If one does not know the genera of a group, he need not write about it, and a thousand names beginning with *Icro* would be no easier to remember than would a thousand names mustered under the family of *Coccinellidae*.

Our experience shows that it may never be possible in the future to eliminate any of the "nearly 2,000 prefixes of *Para* and *Pseud.*" Priority stands above assumed convenience, for it is a matter to be definitely fixed, whereas convenience, good taste and good sense vary with each individual. This the now rejected substitute names of Cuvier and other really great authors clearly show.

The proposition to indicate species by numerals is wholly untenable. It is hard to remember specific names in general, the commonest, as *gracilis, lineatus, minimus* and the like, especially so.

But to most of us the remembrance of meaningless numerals is a thousand times more difficult. To ascertain the identity of Number 43, with that of Number 86, to know which author got in his Number 46 first, and as to whether the hastily described Number 39 of Coccinella really belonged to that genus are matters which no international commission could or would ever try to handle.

When the species are all in and the definitions all agreed upon, we may have an international world catalogue with a number attached to each species. But as we barely know half of those which really exist, and as half of those we know are "geminates" and so may be reduced to the rank of subspecies, we are not yet ready for a numerical catalogue without agreement as to general validity.

The trouble is therefore not with our system of nomenclature but with nature itself, so prolific with forms of life in comparison with the number of us seriously interested in trying to find out what really exists. Nor is it possible, or in any way desirable, to drop our recognition of the "140,000 more or less current generic names" to return to the meaningless pigeon holes into which species were carelessly dropped by the early authors who had never dreamed that evolution and taxonomy would ultimately be one and the same.

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BIOGRAPHICAL NOTE RELATING TO J. J. SYLVESTER

THE recent semi-centennial celebration of the Johns Hopkins University naturally tends to increase temporary interest in the biography of J. J. Sylvester, who occupies a very prominent position in her early history as well as in the history of American mathematics. Hence it may be opportune to note here that in such popular works of reference as the "Dictionary of National Biography" (1898), the "New International Encyclopedia" (1923), the "Encyclopedia Americana" (1920) and D. E. Smith's "History of Mathematics," volume 1 (1923), one finds, under the name of Sylvester, statements equivalent to saying that he was called to the Johns Hopkins University in 1877. On the contrary, the appendix to the first president's report of the Johns Hopkins University states that he was appointed as professor of mathematics on March 5, 1876, and this report states also that he was present at the beginning of the first academic year in October, 1876.

Slight errors as to date are usually of little conse-

quence since the reader can easily find correct information by consulting other authorities, but when such errors are widespread and appear in many of the works which the reader would naturally consult with great confidence, they seem to call for correction in a widely read periodical even if the author of such corrections might by some be put therefore into the class of those who "rail at those who arrive." While the average reader may be satisfied with approximately true statements there are those who seek exact information, and this class deserves attention since it embodies most students to whom the world must look for scientific advances. It was Gauss who insisted on accuracy as regards the last figure in tables of logarithms and brought about a reform relating thereto. It is true that he did not achieve greatness thereby but he exhibited a point of view which is fundamental.

G. A. MILLER

CAPACITY AND FREQUENCY MEASUREMENT BY MEANS OF THE NEON TUBE

IN the June 18th number of SCIENCE there appeared an abstract by Professor Frederick Bedell and Herbert J. Reich describing the use of a neon tube oscillator for obtaining a time axis in the study of alternating current wave forms by means of the cathode ray oscillograph. The oscillator consisted of a condenser which was charged at a constant rate through a saturated vacuum-tube rectifier and discharged periodically and automatically by a neon lamp shunted across the condenser. The method of controlling the frequency of discharge, *i.e.*, changing the rectifier plate current by adjusting the filament rheostat, suggested the possibility of using the plate current as a means of measuring the condenser capacity or the frequency of oscillation.

An analysis of the circuit yields the following simple equation for the frequency in terms of the capacity, plate current and maximum and minimum discharge voltages of the neon tube:

$$f = \frac{I_p - I_o}{(C_c + C_o)\ (E_{max} - E_{min})}$$

where C_o is made up of the neon tube capacity, wiring capacity and coupling capacity to the amplifier or phones, and I_o is the leakage current through the condenser and through the neon tube at the time of discharge. This expression seems to be checked very closely by experiment.

With a preliminary set-up containing low-precision rheostats and meters, readings accurate to within one quarter of one per cent. have been obtained in measuring capacity and frequency. For capacity measurement the frequency is maintained constant by comparison with a standard tuning-fork oscillator. Improvement of the apparatus promises to yield a very simple method of measuring capacity with a degree of accuracy quite sufficient for all ordinary purposes.

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OUR WORLD IN THE MAKING

UNDER the above title Professor Herman L. Fairchild has presented "a brief comparison of some geologic problems analyzed under the two views (Laplacian and Planetesimal) of the primitive earth."[1]

Professor Fairchild says:

> An example of innate conservatism, in science instead of religion, is found in the tenacity with which even scientific men are holding to a discredited hypothesis of world origin.

One is entirely justified in assuming that the carefully worked out analysis which follows the above introduction embodies the latest authoritative opinion on the various phases of geological science involved. Under the circumstances the analysis may appear somewhat dogmatic because of the omission of any reference to such contributions to world origin events as those of Jeans,[2] Jeffreys,[3] and others.

Furthermore, certain details of the analysis, such as the discussion of petroleum origin, may appeal to many readers as somewhat out of harmony with this general purport of the paper. According to the analysis one is led to believe that the organic origin of oil is a view engendered by the necessity of a "surficial origin" imposed by the Laplacian hypothesis. There is the further implication that the organic origin of oil can have no general application because "it is not entirely satisfactory for some localized reservoirs of great volume; nor for the peculiar relations in the 'salt domes' of the Gulf coastal plain; nor for the association of the hydrocarbons with crystalline rocks and volcanic phenomena."

It is safe to assume that there is no general recognition of a special problem in the origin of the oil associated with salt domes although the origin of the domes themselves may be considered still a moot question. And again it is generally conceded that in the strikingly few cases of association of hydrocarbons with igneous or metamorphic rocks the association is that of hydrocarbons of exotic origin.

In the light of the overwhelming evidence—less conservative might call it proof—of the organic origin

[1] SCIENCE, Vol. 44, No. 1659, pp. 365–367. October, 1926.

[2] Jeans, James H. Problems of Cosmogony and Stellar Dynamics. Cambridge (Eng.) University Press. 1919.

[3] Jeffreys, Harold. The Earth, Its Origin, History and Physical Constitution. Cambridge (Eng.) University Press. 1924.