

## DISCUSSION AND CORRESPONDENCE

DR. MONTGOMERY'S PROPOSED AMENDMENT TO  
THE RULES OF NOMENCLATURE

DR. MONTGOMERY'S communication to SCIENCE of July 5, seems to be based partly on a misconception of the meaning of the word "indication" in Art. 25, ¶ a.

This word is generally understood to cover cases where a name newly proposed is based (1) on a reference to a previously published description or figure; or (2) on a figure accompanying the new name; or (3) on a list of previously established species now first associated in a new group.

That a new name in zoology might be based on a mere reference to an otherwise unnamed specimen in a museum, is a proposition which would hardly be maintained by any one, and which Dr. Montgomery hardly needed to condemn.

But Dr. Montgomery's other suggestion, that a name must be accompanied by a description, and that this description must be "adequate" or the figure "recognizable," is a reversion to a state of mind from which, or rather from the consequences of which, modern nomenclature has been struggling for half a century to free itself. It would perhaps have been as well if the original requirement of some sort of a description had been maintained, not because the description in itself would have been of great value, but because this rule would have eliminated from consideration many publications which have added greatly to the complexity of nomenclatorial problems. However, it is too late now to recede, in regard to this point. But the determination of what is or is not "adequate," or "recognizable," would plunge the investigator into a morass of personal opinions which would render any attempt at a stable nomenclature hopeless. WILLIAM H. DALL

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## THE RULES OF NOMENCLATURE

IN SCIENCE of July 5, Dr. Montgomery so well stated the opinion held by naturalists who require that something more than an "indication" should accompany a name be-

fore it merits adoption into zoological nomenclature, that space need not be taken to elaborate his argument, and my purpose is only to lay stress upon an additional need which follows logically.

There will always be many to whom the proposition that in naming systematic groups we are naming objects, not concepts, is philosophically unacceptable, and to these persons concepts must be defined before they can be named. Such naturalists now and always will require that a generic name, like those of higher groups, must be associated with a definition which, as a concession to lack of knowledge at an earlier day, may be incomplete, but must not be actually erroneous or contradictory to the facts which at a later day it is sought to bring under it.

An example of the anomalous and absurd result sometimes reached by the contrary practise under the Draconian law of uncorrected priority is found in the water snakes. This group has been generally known under the name *Tropidonotus* Kuhl (1826). Cope in 1888 substituted *Natrix* Laurenti (1768) on the ground that while *Natrix* was a heterogeneous collection, its type was *Natrix vulgaris* (= *T. natrix*) the type of *Tropidonotus*, and in this he has been followed by some American herpetologists. Now Laurenti's definition of *Natrix* was as shapeless as definitions usually were in his time. Loosely rendered it is: "Head shielded with flat scales; flattened and triangular; the hinder part broad; in front contracted to the snout. Body smooth and shining; narrower behind the head; the middle between the head and end of tail much thicker. Tail conical, elongated and attenuated." The one character of value in identification, "*Truncus glaber nitidus*," is all there is in the definition that might not be applied to almost any snake known, and yet the method of "type by tautonomy" applies the name to a group having the exactly opposite character of most conspicuously rough, keeled scales. Indeed, few snakes are more at fault with Laurenti's language. Laurenti named under *Natrix* twenty-two species, of which eight are unrecognizable and the re-

mainder are now assigned to eight widely separated genera. Of these the two belonging to *Tropidonotus* are the only ones which fail to correspond to the generic character quoted above. No matter, says the extremist in priority, under the rules they must furnish the type!

It has by now become quite clear that uniformity is not to be reached through any of the codes in use, if indeed it ever can be retroactively established by any other not yet constructed, for there will always be some who will not purchase it at too high a price, and the prevailing demand of the moment forgets that there is value also in diversity. Then again, the uncertainty attending the practical application of some of the rules now most advocated precludes denial.

A high authority in matters of nomenclature, whom we all respect and esteem, has lately said in *SCIENCE* that even elimination can lead to only one result when properly applied—but the trouble is that each eliminator thinks that his way of applying it is the proper one. It is easy to get men to agree to abide by law, but another thing to get agreement as to how the law works.

The devious paths to diverse goals followed by those who have attempted the elimination of *Coluber* Linn. is illuminating as to the certainty of the method—but who shall say, as yet, which one is right?

Cope in 1886 was led by the "rules" to *Natrix* as the proper name for *Coluber*. In 1888 the "rules" led him to substitute it for *Tropidonotus*.

The fact is that meaningless conglomerates such as *Natrix* and many other genera of the early days of zoological classification can not be used now under the rules for determining types without doing occasional violence to intelligence. They never did represent definite conceptions and they ought not to be considered in nomenclature. By consent we allow them to Linnæus, but there is no reason why the privilege should be extended to his successors.

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#### THE DISTANCES OF THE FIXED STARS

IN various astronomical and other scientific publications misleading statements are frequently made concerning our knowledge of the distances of the fixed stars. In parallax work practically all reliable observations are of a differential nature, and the interpretations of the resulting measures for distance are largely dependent upon preconceived views as to the arrangement of the stars in space.

For some years past I have been engaged in observational and theoretical work on that intricate problem—where is the origin and what is the physical structure of our sidereal system? The results so far obtained are novel, since they indicate that the structure is radial, in other words the stars and nebulae of our system are moving either directly towards or directly away from our sun; the observed derivations from radial motion being attributed to the unsymmetrical distribution of the attracting masses, and also to the presence of bodies having a secondary origin.

The indications also point to the conclusion that, as seen from our sun, a vast majority of the stars and nebulae are confined to a region whose radial depth is much less than the distance of this region from our sun. Since bodies so situated may be comparatively near to us and still have various radial velocities without causing sensible changes in the configuration of the heavens, the seemingly unchanging aspect of the Milky Way<sup>1</sup> and other celestial regions is explained without the necessity of assigning such great distances (and consequently such great masses) to the bodies of our system.

Considering the still undetermined constants entering into the problem, and the lack of a rigorous method for making direct measures, it surely is no exaggeration to say that a trustworthy value of a star's parallax has not yet been obtained.

The award of the Boyden Premium by the

<sup>1</sup> Whether the theory is in agreement with the actual facts or not, I demonstrate that the inclination of the plane (?) of a Milky Way to the plane of the sun's equator is a necessary consequence of such a structure.