

posterior pair of palato-pterygoid dental plates in the new genus described by Dr. Hussakof we are still without information, but the presence of the latter, at least, may be predicated as a logical necessity.

Newberry's recognition of *M. terrelli* as a distinct species is justified by appreciable differences between the mandible upon which it was founded and those characterized by him as *M. variabile*. Generic differences between it and other Mylostomids are now indicated by the characters of its (supposed) upper dental pavement. Hence, in order to give effectiveness to the theoretical association of parts here proposed, it becomes necessary first of all to unite the two "species" of *ferox* and *terrelli*; and secondly, to substitute the latter specific title, on grounds of priority, as genotype of *Dinognathus*. C. R. EASTMAN

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A LAWYER ON THE NOMENCLATURE QUESTION

DISCUSSIONS of the past year or two in scientific journals—more particularly in SCIENCE—move the undersigned to free his mind on the above subject. Trained first as a zoologist and later as a lawyer, he now follows law as his vocation and zoology as his avocation. This is a good combination of schoolings for the appreciation of some aspects of the nomenclature question.

In the first place, nomenclature as an art—and it is already an art and a very specialized one at that—is not science at all, but law pure and simple. It is the art of interpreting and applying to various states of natural fact the unnatural man-made rules which have grown up during the last century and a half, partly by unwritten custom, partly by precedents, and partly by conscious legislation, just exactly as other systems of law have grown up. Doubtless it is because the scientific men who handle this body of law have no legal training and try to handle it as if it were science (as some undoubtedly suppose it to be) that they make such a prodigious bungle of it. Their chief blunder is that they endeavor to carry on and administer and build up this system of law *without any courts!* Consequently

every piece of litigation (conducted most uneconomically and unsystematically by loose correspondence and articles in scientific journals which ought to be reserved for better things) is indeterminate and each litigant remains of the same opinion still and acts accordingly. If merchants and business men were so stupid as to try to administer the complicated rules of their game for themselves, to the ruinous neglect of their real interests, without special training in the making and interpreting of rules, and *without tribunals for the settlement of their questions*, we should have an exact parallel to the situation which has arisen in zoology and botany.

Some may doubt my dictum that the field of nomenclature is a field of law, not science. Let me add to this dictum one to the effect that many, if not most, of the questions of nomenclature (like many questions of law) are of utterly insignificant importance so only that they be settled *one way or the other, quickly, definitely and permanently*. Then let me cite an instance—and a fair one too—illustrative of both dicta.

Picking up the April number of the *Proceedings of the Malacological Society of London*, I see that A. J. Jukes-Brown, a competent authority in the malacological world, differs widely and strenuously, though courteously, from our own Dr. Dall (a highly competent authority) as to the nomenclature of certain groups of the Veneridæ. In part his difference turns on different findings and interpretations of facts. These are scientific differences. The scientific methods of which each is master will enable the two men to agree—or if they can not reach the same conclusion then to agree to differ. Neither can, or should, after his final reexamination of the evidence, yield his honest opinion a jot to anybody or to any tribunal. But in part his difference turns on the following point. Dr. Paul Fischer, in his "Manuel de Conchyliologie" (etc.), did much rearranging and collating of generic and subgeneric groups. For each group he had the habit of naming one species with the prefix "*ex.*," standing, of course, for "example." Dr. Dall, perhaps not

unnaturally considering it as of little significance whether the species selected as illustrative of a group was called "the type" or "the example" of it, regarded Fischer in these cases as having designated the type with the usual train of consequences under the rules. Jukes-Brown holds that of course Fischer did nothing of the kind, examples not necessarily being types.

I should be willing to argue either side of this if paid for it—but not otherwise! It is a law-point pure and simple and a dry one at that. It is not of the very smallest import to any aspect of science which way it is decided. To flip a coin would be a good way to settle it. Yet in the present state of things it is quite supposable that Dr. Dall and Mr. Jukes-Brown, in order to reach a working agreement as to the nomenclature of the Veneridæ which *is* of scientific import, may feel compelled to give considerable amounts of their time, and considerable space in a crowded journal, to a necessarily inconclusive attempt to thresh it out.

It is a sin and a shame, a reproach to science and scientific men, that the time of master specialists, every available moment of which is needed by science, should be taken up by utterly vain questions like this, which any whipper-snapper just out of a law school could actually handle better than they can because trained for it and not taking it so much in earnest. And their time *is* taken up by questions like this, and it *has* to be under the present lack of system.

And what, pray, is the great difficulty in settling these things as they should be settled? If the next International Zoological Congress voted to establish an international court of five members sitting for three months annually to decide in writing every question submitted, with or without argument, taking counsel with specialists when necessary, publishing its decisions in an annual volume with or without the course of reasoning in each case, how many years would it take before the *main* questions were *all settled* and the business of the court reduced to a thin trickle of new puzzles? Of course such a court should have absolute power to settle absolutely everything

nomenclatorial except questions of natural fact and scientific interpretation. Equally of course it would have to treat the priority rule as a *prima facie* rule made by sane men for sane men, not as the superstition and incubus it has become. If they saw fit to rule *in* one auction catalogue as a nomenclatorial source for merely practical reasons, and rule *out* another for similar reasons, they should have a free hand to do so without feeling that any one by being the first to name, or perhaps mis-name, a natural object thereby acquired a vested right to retard the progress of science for centuries. Of course in the present absence of such a tribunal, or of *any* tribunal, an absolute priority rule has an excuse as being the nearest present approach to a universal touchstone for our names, and so long as that situation endures systematists are bound to live strictly up to it. But with such a tribunal suggested the direful necessity for it would pass away, and the "Museum Smithianum," 1832, having once been ruled in we could then apply its names and learn them without the probability of someone's discovering next year that the "Museum Jonesianum"—date hitherto unknown—was in fact published in 1831. Nay more, a discovery that the "Museum Smithianum" was a rank forgery and in large part non-binomial need not worry us. Once ruled in or out, mistakenly or not, it stays so. The name "*Octopus*" once adjudicated to be the name of the group, typified by *O. vulgaris* L. stays so no matter how clear the proof that the court ought by every known rule to have made it "*Polypus*." Is there any ethical question involved? No. And does it matter to science which it is called? Not an iota so long as we know which.

Would it not tend to "crystallize" and "fossilize" science? No, but it would tend to crystallize and fossilize the artificial Latin nomenclature of science which *ought* to be crystallized and fossilized, and the sooner the better. Of course no tribunal can ever pass on the question whether a given form is a variety, a subspecies or a species; whether it belongs to this or to that genus or subgenus; what are the limits of a family; *nor on any*

other question of science properly so called. And equally of course nomenclature can never be definitely settled. But its puerile and yet forbidding aspect can be vastly altered for the better.

Is there any real practical difficulty in the way of doing all I have suggested and doing it at once? Emphatically *no*! Men more trained to cooperation than scientific men—business men, administrators, lawyers, politicians—would have done it long ago.

FRANCIS N. BALCH

JAMAICA PLAIN,
May 21, 1909

PERSONAL NAMES AND NOMENCLATURE

THE use of personal names in nomenclature which has been somewhat criticized by various correspondents is perhaps defensible under certain circumstances. While its objections in many instances have been pointed out yet the absurdity of the practise becomes strikingly apparent when one notes such a paper as that on Paleozoic Ostracods in a recent volume of the Proceedings of the National Museum. In all, sixteen generic names are used in the article; nine of these are old and seven new. Among the old names, five are certainly personal in origin, four may not be, although two of these probably are. Among the seven new names, absolutely every one is personal. Either this indicates an extraordinary number of distinguished men in this field or an unfortunate lack of mental energy on the part of the authors.

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SIR WILLIAM GAIRDNER'S PAPERS

TO THE EDITOR OF SCIENCE: In response to the wishes of Lady Gairdner and her family, I have undertaken to edit the medical and scientific papers and articles of the late Sir William Tennant Gairdner, and to preface the collection with a biography.

In order to render the work as worthy as possible of the memory of the late professor, I am desirous of enlisting the sympathy and help of his friends. I venture therefore to request through your columns that any one who has in his possession any letters or other

literary remains of Sir William Gairdner will be so kind as to communicate with me.

G. A. GIBSON

3 DRUMSHEUGH GARDENS,
EDINBURGH,
May 12, 1909

SCIENTIFIC BOOKS

The Book of Wheat. By PETER TRACY DONDINGER, Ph.D., formerly Professor of Mathematics in Fairmount College. With 60 illustrations. Pp. xi + 369. New York, Orange Judd Company; London, Kegan Paul, Trench, Trübner & Co., Limited. 1908.

When we think of the great importance of the cereal wheat in the food economy of nations it is surprising that there has not been more written on the subject. The book now before us is something that might well have been looked for years ago. The author has furnished portions of his manuscript at different times to the writer of this review, and the latter has, therefore, known something of what was to be expected in the book itself.

Naturally a writer is likely to give more prominence, in discussing a subject, to those features with which he has come most often in daily contact, and so in this instance there is proportionately not as much space given to the discussion of wheat as a plant as to the milling operations, the commercial and economical position, etc. The work is particularly lacking in its presentation of wheat classification, discussion of varieties and other matters of botanical and agronomic interest. On the other hand, there is a very full discussion of the machinery for harvesting and threshing, crop rotations, fertilizers, marketing, milling, prices, movement and consumption. A commendable feature, also, is the addition of a very complete bibliography, though it must be said that the proof-reading of this bibliography was very faulty.

Considerable attention is also properly given to the topic of diseases and insect enemies.

In making use of the map (page 9) showing wheat distribution, which was formerly published by the U. S. Department of Agricul-