this profound way. Thus if his point of view is accepted my paper quite lacks a *raison d'être;* I was combating windmills.

In my former paper I made no attempt to show that Driesch's views were of the character that I set forth, because it seemed to me (and still seems to me) that he had stated, in his published works as fully and unequivocally as it is possible in words, that they are of that character; and that, moreover, his whole argument loses its coherence and becomes incomprehensible if they are not.[2] I therefore did not expect any one who had made a careful examination of Driesch's "Science and Philosophy of the Organism" to question this.

Since, however, it has been questioned by one so competent as Lovejoy, with the intimation, as quoted above, that my own scrutiny had not been sufficiently close, it is of interest to learn Driesch's own opinion on this point, when the matter at issue is put explicitly before him. I quote, by permission, from letters received from Dr. Driesch:

You are quite right in saying "the biologist can not from a knowledge of the total physical configuration predict what will happen even after he has observed it." This is indeed a consequence of my vitalism and I am very glad to see that you fully appreciate it.

I reject absolute indeterminism but *accept* experimental indeterminism.

In other words: A complete knowledge of all physico-chemical things and relations (including possible relations) of a given system at the time *t* gives *not* a complete characteristic of that system in the case that it is a living system.

Or: Two systems, absolutely identical in every physico-chemical respect, may behave differently under absolutely identical conditions, in case that the systems are living systems.

For: the specificity of a certain entelechy is among the complete characteristics of a living organism, and about this entelechy knowledge of physico-chemical things and relations teaches *nothing.*

My short formula about the matter in question is: No absolute, but "experimental" indeterminism.

Dr. Driesch's statements of the matter are then fully as strong as my own. If he understands his own philosophy, it therefore appears to me that the further reasoning in my former paper was quite justified, and is entitled to the careful consideration of any others who have leaned toward Driesch's vitalism without realizing that it means experimental indeterminism.

H. S. JENNINGS

[2] Driesch's argument is one by exclusion, running essentially as follows: Since there are no diversities in the physical conditions that explain satisfactorily the diverse results in certain different cases, and since we must hold to determinism, it follows that there must be something non-physical (*i. e.*, entelechy) to account for the diversities in results. It appears to me that the failure to correctly apprehend Driesch's argument is what causes Lovejoy to intimate frequently that the entelechy concept is superfluous in Driesch's vitalism; merely "dragged into the situation," as he expresses it. Without entelechy a yawning hiatus is left in Driesch's system; it is all that saves him from absolute indeterminism.

ZOOLOGICAL NOMENCLATURE

TO THE EDITOR OF SCIENCE: In SCIENCE for August 9, my esteemed friend Dr. Kingsley, makes a plea for various exceptions to the rule of priority in names of animals and to other rules which have been adopted by the Commission on Nomenclature of the International Zoological Congress.

It is no doubt exasperating to many zoologists who have to use only a few systematic names in their work and then at long intervals, to find that in these intervals older names, carelessly or ignorantly neglected in the past, have risen to take their places. It is also exasperating to professional taxonomists and students of geographic and other relations of species, to be told that their efforts to bring past confusion into order shall be set aside whenever these efforts discommode workers in other fields of zoology, who for the most part neither know nor care for the part accurate bookkeeping must play in the study of systematic zoology and botany.

Taxonomy with geographical and geological distribution constitutes a science by itself, with methods of its own wholly separate from those of anatomy, embryology and histology. We have found, by weary experience, that either the use of names must be governed by rule, or else each man may call anything whatever he pleases. The latter has been done too long. We have been for eighty years making progress toward order, and the Zoological Commission has done fairly well in bringing the variant points of view of actual workers in taxonomy into practical harmony. Compromises have been necessary, but we must remember that no compromise not founded in the nature of things will be respected by future workers. The shield of high authority of men like Cuvier has not sufficed to cover his lapses of failure to recognize the work of earlier but less favored authors. Investigators who deal with a few common species may use as vernacular names words like *Amphioxus, Bdellostoma* and the like, not sanctioned by priority, but there is no line which taxonomists can draw which should retain these names invalidated under the law of priority, while retaining order in the other parts of the taxonomic system.

The chief real confusion centers about the need to restrict to a definite type the wide-ranging, ill-defined, incoherent groups of some of the earlier systematists. The genera of Linnæus correspond in general to the families of to-day, while in very many cases, the same species, under other names, appears in two or more different genera.

To limit these genera we have in general two methods. One is to settle the matter on the basis of the words of the original author. If he designates no type, let the first species he names under a genus stand as type. This method has the tremendous advantage of absolute fixity. It would involve a few dozen changes from current nomenclature, but it would stand once for all. Some writers still adhere to it, through thick and thin.

The other method allows the author who deals next with the genus to fix its type. The first one who does so completes the genus and fastens it once for all on some definite species. This method makes necessary much bibliographic research, otherwise unprofitable, and as many writers have no clear conception of generic type, it is often not certain whether such have fixed the type or not.

A third method, that of elimination, by which the type is fixed of a genus for the species which remains after the others have been removed has never been defined and is not practicable. The second method, as a compromise between the first and third, was adopted at the Boston meeting of the Zoological Congress in 1909. If it fails, taxonomists will have no recourse but to fall back into two mutually criticizing camps: those who fix a genus absolutely to the first species named, and those who fix it where they please, according to their treatment of the exigencies of elimination.

The present writer believes that the first species rule would have been best, but as it can not secure a majority vote of taxonomists, he favors the second rule adopted unanimously at Boston. Non-taxonomists have no rights in this matter. We might as well ask them to make their cells visible to the naked eye, laying aside their technique, as for them to ask for the abolition of the technique of taxonomists. To submit to rules of nomenclature "to the plenum of the congress to vote," is to destroy all possibility of taxonomic technique. The botanists have already furnished the awful example. Rules which no investigator can or will follow or which may be set aside in the interest of choice or convenience do not contribute to the fixity of nomenclature.

As to the specific propositions quoted by Dr. Kingsley:

1. To exempt a list of names "in common use before 1900" or "employed in instruction."

It is hard to see by what authority this can be done and the list made permanent. Taking individual cases: *Echidna* is the name of a large and widely distributed genus of eels as well of the Australian spiny monotreme, the eel-name having been in use 140 years. Why should the ichthyologist give it up? As to *Amia,* it is a pity that Linnæus chose that name for the ganoid bowfin when Gronow had used it five years before for a perch-like fish. Personally I preferred to reject all Gronow's non-binomial generic names, leaving *Amia* instead of *Amiatus* for the bowfin, but I follow the decisions of my colleagues. We can not use the same name for two genera. The list of genera, the retention of which is desired as printed in SCIENCE, contains 38 names, the changing of most of which has been unpleasant to taxonomists as well as to others. But these 38 we would like to keep are very few among the thousands of generic names which only a recognition of the law of priority and of some law for fixing the type of incongruous genera can hope to regulate.

The second proposed rule is this:

> The transfer of generic or specific names from one genus or species to another shall not be allowed when this will lead to lasting confusion or error.

This reads fairly, but it is not possible to give it definite application. Some names occur so frequently in literature that they may be said to be definitely fixed. Most names the world over have only a tentative status. The fauna of the world is very large, and we are only at the beginning of our knowledge of it. The fauna of western Europe, to which many of the 38 names belong, is only a minute fragment of it. The main source of confusion and error is, however, in leaving a name where it does not belong, after its right place or right usage has been made clear. But if this rule could be lucidly framed so as to permit regularity of application, it has its merits.

The third proposition, the rejection of certain authors on their merits as non-binomial, has its advantages. The non-binomial writings of Brisson and Gronow have been accepted by the commission. A non-binomial condensed reprint of Klein has been rejected. Either view of the case, if generally followed, leads to stability. Before the ruling of the commission Brisson's names were accepted by a majority, those of Gronow, on the same footing, by a minority. The commission has voted to accept both. The matter is likely to come up again at the Monaco meeting.

The fourth proposition, the rejection of non-scientific catalogues, newspapers and the like, might be reasonable if it could be properly defined.

The vital thing is the recognition of law as superior to personal preference or temporary convenience. The "deplorable results" of adherence to the rigid rule of priority are as a drop in the bucket compared to the "deplorable results" that have followed the go-as-you-please acceptance, rejection or change of generic and specific names. And this latter form of "deplorable results" does not trouble the non-systematist who uses scientific names casually as labels for his preparations or who may deal with a small part of a long-known fauna. They vex the systematist who must map out and record some broad part of the vast system of the life of the globe. In his bookkeeping he must follow the same methods throughout regardless of local usage or of personal preferences.

DAVID STARR JORDAN

THE PHYSIOLOGICAL SIGNIFICANCE OF THE SEGMENTED STRUCTURE OF STRIATED MUSCLE

TO THE EDITOR OF SCIENCE: In my article, "The Physiological Significance of the Segmented Structure of Striated Muscle," published in your issue of August 23, I make, on page 251, the following criticism of certain current hypotheses of muscular contraction:

> A further disadvantage of the ''swelling-hypotheses''—as contrasted with the surface-tension hypothesis—is that they offer no suggestion as to the nature of the connection between the electrical variation accompanying contraction and the contractile process itself.

Some qualification of this statement is now necessary. In Pauli's recent article, "Kolloidchemie der Muskelkontraktion" (Th. Steinkopff, 1912), which has reached me since my article was printed, an attempt is made to refer the negative variation to the formation of acid-protein compounds within the muscle-cell. Such compounds would yield on dissociation mobile anions, *e. g.*, lactate ions, and immobile or colloidal cations. On the assumption of a free permeability of the plasma-membrane to these anions, the formation of such compounds would theoretically give rise to a negative variation. But this conception appears to me insufficient to account for the entire phenomena of action and demarcation