embellishment of the campus and grounds. These grounds are located in Manoa, a suburban valley with both mountain and sea views, and comprise about ninety acres. Sixty acres were purchased and thirty acres were set aside by the government. The total grounds with its water has a market value of about $125,000.

M. ALBERT KAHN, of Paris, who has established traveling fellowships in several foreign countries, has given $2,500 for such a fellowship in the United States. It is expected that the fellow selected will travel around the world giving a year to the trip. Selection of the fellow will be made by the trustees, who are Edward D. Adams, Nicholas Murray Butler, Charles W. Eliot, Henry Fairfield Osborn and Charles D. Walcott, and they are to choose preferably professors in isolated southern and western institutions.

DR. H. Y. BENEDICT, professor of applied mathematics and director of the department of extension of the University of Texas, has been made dean of the College of Arts.

AT the University of Pennsylvania Dr. Richard M. Pearce has been transferred from the chair of pathology to that of experimental pathology, and Dr. Allen J. Smith has been transferred from the chair of tropical diseases to that of pathology, formerly occupied by him.

DR. LUTHER WILLIAM BAHNEY, assistant professor of metallurgy at Leland Stanford University, has been appointed assistant professor of mining and metallurgy in the Sheffield Scientific School, Yale University.

DR. CLARENCE A. PIERCE, of Cornell University, has been appointed assistant professor of theoretical electrical engineering at the Worcester Polytechnic Institute to succeed Dr. George R. Olshausen, who has resigned after four years of service.

DR. WALTER S. TOWER, assistant professor of geography in the University of Pensylvania, has been called to the University of Chicago.

DR. J. FRANK DANIEL has been promoted to be assistant professor of zoology in the University of California.

DISCUSSION AND CORRESPONDENCE

THE LAW THAT INHERES IN NOMENCLATURE

DR. JORDAN'S answer[1] to my inquiry,[2] "Whether there is not a better way of disposing of our nomeclatural trouble than first making it as burdensome as possible and then making it permanent?" is, if I understand him aright, that, alas, there is none; at least, there is none yet in sight, or likely to appear. Hence it were better to take up the burden cheerfully, and begin getting used to it.

Whether one be pleased with this prospect or not, he must be grateful for Dr. Jordan's clear and forceful statement of certain guiding principles. This, for example, seems to me to go to the heart of the matter under discussion:

"A writer dealing with scientific names must either call an animal or plant what he pleases, or else he must conform to regulations inherent in the nature of his work. Arbitrary rules will soon be disregarded. The necessary regulations are those which future workers will approve, and we who are working in the infancy of taxonomy must lay foundations on which the future can build." With this we may all agree; though we may hold somewhat different views as to what is the law that inheres in the nature of our work, and as to what rules are arbitrary.

Surely no argument is needed against a return to the loose nomenclatural methods of the past. I protest against the implication that I have advocated anything of the sort. On the contrary, I have advocated the strictest application of the laws that have been evolved by our past nomenclatural experience. I would accept a list of names exactly as furnished by the best historical knowledge that could be brought into service in producing it. And then, because such a system would be more than human nature can bear, more than language can use, and more than our science can make its best progress under, I would provide for general use a terminology giving expression to the same system in simpler form, with fewer, briefer and simpler names, and

[1] SCIENCE, March 10, 1911.

[2] SCIENCE, September 2, 1910.

symbols. That is the whole of it. No plan for solving zoological problems by rule is proposed; only a plan for conserving time and energy, offered in the belief that the purely clerical work of biological science might be accomplished with less waste. The simpler system would stand in the same relation to the existing system as that in which the Linnæan names have stood to the long descriptive phrases that preceded them.

To be sure, this plan, which allows choice of names (one out of a score more or less in every group), does not necessitate that the oldest one shall be forced into general use in the new system: rather, it leaves the selection to those most competent, most interested and most responsible for the future in each group. This feature may hold the derogation of democracy to which Dr. Jordan refers, but if so, I do not understand what sort of a democracy systematic zoology is considered to be. Is a law of priority its only possible standard of equality? I profess to be a democrat, and, in a very small way, a systematist; yet I confess I never heard of anything like this. May not this democracy abide the recognition of merit? Is it already irrevocably bound up with a statute of nomenclatural primogeniture? Does the determination of priority in and of itself necessitate that all good democrats must acclaim the restoration of lost names to the places they once transiently occupied in spite of all that may have happened in the intervening years?

I have myself long pursued priority in the hope of names that would be both stable and usable. I have even advocated the forcing of prior forgotten names back into general nomenclature. I did so as long as mere temporary convenience seemed at stake. I did so while names doubled in length, trebled in absurdity and quadrupled in number. I did so until family names began to fall and to be set up again in exchanged places. I did so until I became unable to read the literature in several groups of which I had once been a student, or to converse with modern students of those groups. I did so until it became well nigh impossible for me to give to my classes intelligible references to the literature they most needed to consult in their work.[3] And

[3] Recently, while providing tables for the work of a small class in limnology, I encountered the following situation in aquatic diptera. Half of the names of dipterous families containing aquatic larvæ have been victims of the rule of priority. Here are the names of the families of our fauna, as found in all the text-books, manuals, monographs and general reference books.

Psychodidæ	* Leptidæ
* Ptychopteridæ	Empididæ
Tipulidæ	x * Stratiomyiidæ
x * Blepharoceridæ	Syrphidæ
Dixidæ	* Borboridæ
* Chironomidæ	Ephydridæ
Culicidæ	x * Cordyluridæ or
* Simuliidæ	* Scatophagidæ
Tabanidæ	Sciomyzidæ

Only those unmarked in the list remain unchanged. Of the others, three (marked x) have been changed in spelling only, return to an incorrect or inelegant form being required in this line of progress. One of these names, Cordyluridæ, is in less common use than Scatophagidæ, but Scatophaga also falls. In addition to this, the well-known names *Syrphus* and *Sciomyza* have been shifted to designate new groups of species in their respective families. So, also, has *Corethra* within its subfamily. All these familiar groups will now bear unfamiliar names.

Now, perhaps, a better democrat than I would have adopted all these changes willingly and pursued priority to the bitter end. But I did not. I wished my class to use the literature that has grown up about the names *Corethra, Chironomus, Simulium, Eristalis*, etc., names that are the subjects of books, of memoirs and of classic investigations in many fields of biology, and that have nowhere any uncertain meaning. As a teacher I could not afford the time and effort necessary to explain to rational young people why the "interests of taxonomy" require that *Corethra* or *Syrphus* be removed from their accustomed places after one hundred years, and used to designate entirely different groups of flies. In fact, I can not explain this; nor why, if the zoologists of the world have been able to agree on a law of priority, they might not yet be able to agree upon something less distressing.

Any one who speaks of this as a matter of *temporary* inconvenience surely is thinking in terms of geologic time.

then I began to entertain doubts as to the approval of posterity, the best kind of foundations, etc. I began to lose faith in the law of priority as a cure-all for nomenclatural ills. For the real burden of nomenclature will be but little altered by the strictest application of this law. At worst (and surely the worst is now in sight) it will have added but a little dead weight of stupid and unnecessary confusion—so little, indeed, it would hardly be noticeable were not the load already at the endurance limit. With all the arduous labor now required of any youth for gaining even an elemental conception of the world's accumulated store of knowledge, why should any man, even though a profound scholar, familiar with the intricacies of his own field, so far forget or minimize the difficulties of the long way by which he has come as to be willing to leave the path harder for the next comer. Ought not the way that leads to a working knowledge of plants and animals to be as easy and plain as we can possibly make it? I think so. And so thinking, I ventured to propose, after long consideration, the simplification that is now under discussion.

My plan would accept the facts of nature as they are—exceedingly complicated. They are not more complex under one system than under another. And it is a great error to assume that because facts are numerous and relations complex, the method of handling them must be equally so.

My plan would accept human nature as it is—exceedingly prone to differences of opinion; yet, withal, able often to agree upon such matters as dates of publication.

My plan would accept the results of the application of the law of priority *in toto*, conserving all the good work that has been done by the zoologists of the world in their search of early literature. It would keep the results of this work forever accessible, without making of its by-products stumbling blocks in the way of beginners, of general students, and of the increasing thousands who may have an interest in biological sciences. This work is of great historic value. It is worth while to have all the old and unused names set in their proper order and sequence. But to have any such of them as have lain buried during the growth of a great literature, used when exhumed to replace the names about which that literature has grown, making its treasures less accessible, is a lamentable abuse of the historic method.[4] Let us accept the good work that has been done in determining priority at its historical value, and then let us use it like rational beings for our assistance, without making it a source of embarrassment for future generations.

My plan would accept the Linnæan system as it is, recognizing species as real entities that have received and that will continue to receive names. Were Linnæus resurrected to-day, he might have difficulty in recognizing his own system, in its present dropsical condition. Those who value it so highly should at least remember that, whatever it has become, it was in the beginning simply and solely an effort at simplification of nomenclature.

The matter of numbering species is so simple it is hard to understand how any difficulty is found in applying it. Given a list of the names now recognized in any group written down in their original form and in their historic sequence, any common clerk could affix the numerals correctly. Their stability would be assured by the only means whereby anything becomes stable—by adoption and use. Any one who will read my proposal with reasonable care will see (1) that it accepts every name exactly as given by its author, and finds a place for it in its proper sequence; (2) that it matters not at all where we begin numbering, and (3) that it matters not at all whether *Balanoglossus* and the tunicates are fishes or not.

I regret Dr. Jordan did not see these things, for then he might have saved space for a statement of the inherent law of nomenclature. Formulation of it is badly needed.

[4] My proposal, however, was to let the principal workers in any group decide upon the names to be used in it. If those who study lancelets do not wish to use the name *Amphioxus*, neither do I wish to use it.

Elsewhere real progress is found in the direction of simplification, which makes for convenience, saves time, and meets the limitations of memory by instituting more concise methods of making records. Does the law that inheres in nomenclature differ so much from that which obtains in all other vast accumulations of facts? If so, let us have a statement of it, so that we may, by understanding it, attain to acquiescence in the inevitable.

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ON EVIDENCE OF SOMA INFLUENCE ON OFFSPRING FROM ENGRAFTED OVARIAN TISSUE

TO THE EDITOR OF SCIENCE: In publication No. 144 of the Carnegie Institution of Washington entitled, "On Germinal Transplantation in Vertebrates," by Castle and Phillips, issued March 14, 1911, an attempt is made to overthrow my experiments on transplantation of ovaries in fowls,[1] and Magnus's[2] experiments of similar character on rabbits, and to establish a claim to priority in the demonstration that offspring may result from transplanted ovaries; and the effect, if any, of soma influence on such offspring. Therefore, I feel it incumbent to call attention briefly to certain of the statements in order that no misunderstanding may result. Since my papers with the experiments are readily available, I shall avoid all unnecessary repetition.

In a word, the situation is as follows: By exchanging the ovaries of fowls and breeding the fowls, I obtained results which seem to show that the transplanted ovaries preserved their reproductive function; and the resulting offspring presented evidence of soma or foster-mother influence. The results are given in detail in my several papers. I may add that since I had no allegiance with any school of theorists, I was not involuntarily partial in observing and recording the results. Whether the results would substantiate either or neither of the theories built largely upon speculation as to the relationship of reproductive tissues to their environment, or whether the character of the offspring would conform to Mendel's results of studies of inheritance in peas, gave me no concern.

The primary object of the experiments was to determine if an engrafted ovary might retain its reproductive function. Therefore, an answer to the question was obtained. And incidentally information on soma influence was secured. Following this, it seemed of additional interest to reverse the matings of the parent stock. And also, by breeding, to study the character of the offspring from the offspring obtained from engrafted ovaries. Unfortunately before this was accomplished, the experiments were terminated by an outbreak of disease among the fowls. But I did not consider then, nor have I since come to believe, that the character of the offspring of the second generation could do more than indicate whether or not soma influence might be evident in the character of the offspring of this generation, that is, the grand chicks. But owing to a degree of familiarity with the general principles of physiological experimentation and interpretation, from the beginning I saw the limitations to the absoluteness of any evidence that might be obtained by continuation of such experiments. For example, before drawing the provisional conclusions in the announcement of my results, the statement was made that "more data must be had on these points before definite conclusions can be drawn."[3] Apparently Castle has

[1] "Results of Removal and Transplantation of Ovaries in Chickens," presented before the American Physiological Society in connection with the seventh meeting of the Congress of American Physicians and Surgeons, Washington, D. C., May 7–9, 1907 (*American Journal of Physiology*, 1907, XIX., xvi–xvii). "Further Results of Transplantation of Ovaries in Chickens," *Journal of Experimental Zoology*, 1908, V., 563. "On Graft Hybrids," presented before the American Breeders' Association, Omaha, December, 1909. "Survival of Engrafted Tissues. I. (A) Ovaries and (B) Testicles," *Journal of Experimental Medicine*, 1910, XII., 269.

[2] Magnus, "Transplantation af Ovarier med Saerligt Hensyn til Afkommet," *Norsk Magazin for Laegevidenskaben*, 1907, No. 9.

[3] *Journal of Experimental Zoology*, June, 1908, V., p. 570.