than floor supports for apparatus. Numerous other details were considered.

The subject of stability was further discussed by several of the physicists and astronomers present.

Charles K. Wead,
Secretary.

DISCUSSION AND CORRESPONDENCE.

RECENT WASHINGTON RHIZOBIA EXPERIMENTS.*

In 1902 Dr. Geo T. Moore published a paper in which he gave a brief outline of the history of the study of the free nitrogen-assimilating microbes of leguminous plants.† In this paper the author outlines a method for increasing the nitrogen-assimilating power of rhizobia by growing them upon artificial nitrogen-free media, which is said also greatly to increase their tubercle-forming power. According to the paper by Grosvenor, Dr. Moore has continued his experiments along the same line and has patented the process, giving the patent rights over to the government for the sole benefit of the farmer. It is stated that by the use of these nitrogen-hungry rhizobia the yield of any leguminous crop may be increased very greatly (from 40 to 400 per cent.). The results are said to be far superior to those obtainable from the use of the 'Nitragin,' patented by Nobbe and Hiltner of Germany. Instead of bottling the cultures (of nitrogen-hungry rhizobia) in a dry pulverulent state, as did Nobbe and Hiltner, Dr. Moore infiltrates absorbent cotton with the cultures and dries it, whereupon it is ready for shipment to the farmer, at a nominal cost.

If the claims of the paper can be verified by further tests, Dr. Moore deserves credit for having accomplished a work which will prove to be of great benefit to farmers. It will of course not do away with the necessity of crop rotation.

It is regrettable that Dr. Moore did not see fit to contribute the article himself and that it did not appear in some scientific publication rather than a literary magazine. This is not at all intended as a criticism of Mr. Grosvenor's presentation of the work done by Dr. Moore, only the custom prevails for those who do the actual scientific work to also present it to the world first-hand, nor are we in the habit of looking for reports of research work in publications devoted almost wholly to fiction.

Albert Schneider.

* Gilbert H. Grosvenor, 'Inoculating the Ground: A Remarkable Discovery in Scientific Agriculture,' *The Century Magazine*, **68**: 831–839 (October), 1904.

† Geo. T. Moore, 'Bacteria and the Nitrogen Problem,' Year-book of the Department of Agriculture, pp. 333–342, 1902.

SPECIAL ARTICLES.

A NEW CODE OF NOMENCLATURE.

In *The Condor* for January, 1905 (Vol. VII., pp. 28–30), is an abstract of a new code of nomenclature, "which will shortly appear under the joint authorship of Doctors Jordan, Evermann and Gilbert, * * * entitled 'Nomenclature in Ichthyology. A Provisional Code Based on the Code of the American Ornithologists' Union.'" It is said:

> The recent preparation of numerous papers in systematic ichthyology has necessitated the reconsideration of many problems of zoological nomenclature, and as some of these are not covered by any canon in any recognized code, and again, as certain canons in the best considered of the various codes of nomenclature, that of the American Ornithologists' Union, are not available in the study of fishes, we have ventured to draw up a code for our own use in ichthyology. * * * The different canons in this code are based on those composing the code of the American Ornithologists' Union, and so far as possible the language of that admirable document has been followed. We have, however, omitted certain matters which may be considered as self-evident, and we have omitted all reference to groups of higher than family rank.

The points in which the ichthyological code differs from the ornithological are then stated; the text of these parts of the new code is given apparently in full, and relates to six of the canons of the earlier code. As the perfect code has not as yet been devised, all improvements on preceding codes should, of course, be welcomed, but changes from well-established methods of procedure should carry convincing evidence that they are improvements in order to secure adoption.

Not many months ago the American

botanists issued a new 'Code of Botanical Nomenclature,'* they having found the Paris Code of 1867 out of date and unsatisfactory. This code does not depart essentially from the A. O. U. Code, but on some points it is fuller and more explicit, and at the same time more concise. As said in another connection:

> The A. O. U. Code was a pioneer in innovations which have now become very generally accepted, but which then (1886) required argument and extended illustration. * * * Provision is made for a few points not covered by the A. O. U. Code, but the spirit and principles of this code are * * * closely followed. * * * †

In the Ichthyological Code the new rulings principally relate: (1) To competitive specific names published simultaneously; (2) to competitive generic names published simultaneously; (3) to the determination of a generic type in cases where no type has been indicated by the author; (4) to the admissibility of orthographic variants of generic names.

The primary purpose of all codes of nomenclature is stability of names; how best to accomplish this under complicated conditions is still an open question. The points on which leading authorities still differ are mainly those above stated, and respecting which a new departure is proposed. These may be taken up briefly in sequence.

1. *Specific Names Published Simultaneously.*—"Canon VI. Of competitive names otherwise tenable, given by the *same*‡ author, that one is to be preferred which stands first in the text. In case of competitive names otherwise tenable, given by *different* authors of the same actual date, so far as ascertainable, the one standing on the earlier page of *its publication* must be chosen."

To this ruling there is no objection, provided authors will uniformly adhere to it. This method was considered in framing the A. O. U. Code, but was deemed too arbitrary, as the author publishing a large book, with new names introduced in the middle portion or toward the end, would have no chance against the man publishing new names in a small book or in a short pamphlet, however superior his accompanying diagnoses might be. For this reason the A. O. U. Code (Canon XVII.) proposed alternatives, perhaps better applicable in ornithology than in some other branches of zoology. Thus preference is to be given, first, to the name founded on the male to that founded on the female; second, to that founded on the adult to that founded on the young; third, to that founded on the nuptial condition to that on the pre- or post-nuptial condition.

2. *Generic Names Published Simultaneously.*—"Canon VII. In case of competitive generic names otherwise tenable, published in the same work, preference shall be given to the one standing first in the work. Of competitive generic names of the same actual or ostensible date (no exact date being ascertainable) given by *different authors,* that one is to be taken which is proposed on the *earlier page of the volume in which it appears.* When the same generic name is given to two distinct genera of animals at the same date (as far as ascertainable), the name appearing on the earlier page shall be deemed to have precedence."

Here again the ruling is rigidly arbitrary as between earlier and later pagination in *different publications.* The A. O. U. Code (Canon XVIII.) provides, under such contingencies, that: "1. A name accompanied by the specification of a type takes precedence over a name unaccompanied by such specification. 2. If all, or none, of the genera have types indicated, that generic name takes precedence the diagnosis of which is most pertinent." Here comes in the element of personal decision as against arbitrary rule, but the cases are extremely few where the proper course of action is not evident.

3. *The Determination of Generic Types.*—Canon X. of the Ichthyological Code relates to the fixing of the type of a genus, when no type has been indicated by the author. On no nomenclatorial question is there greater

* *Bulletin Torrey Botanical Club*, Vol. XXXI., No. 5, May, 1904, pp. 249–290.

† *Auk*, Vol. XXI., July, 1904, pp. 404, 405.

‡ The italics in these quotations are not in the original, but are used here to draw attention to special points.

diversity of usage or greater strenuosity of opinion than on this, although the tendency is, or was formerly, to follow one of two courses, either to take the first species as the type, or to determine the type by the principle of elimination, under certain reasonable restrictions. Of late the latter has been the course favored by the greater part of those systematists who have any special regard for rules of nomenclature. Two qualifications of the strict rule of determining the type by elimination have been widely accepted. One is that when a genus containing a number of species is divided, and the name of one of the species is chosen as the name of a new genus, the type of that genus shall be the species the name of which has been selected as the name of the genus—a perfectly logical, unequivocal proceeding, open to no reasonable objection.

A second exception is that of the A. O. U. Code, which provides that if a "genus contains both exotic and non-exotic species—from the standpoint of the original author—and the generic term is one originally applied by the ancient Greeks and Romans, the process of elimination is to be restricted to the non-exotic species." In this way the name is retained in nearly its ancient sense, and its transference to an irrelevant association is prevented. This exception comes in mainly, of course, in connection with Linnæan and Brissonian names, and is akin to that other rule, more or less tacitly held in the minds of many systematists, that the type of a Linnæan genus should be the best known European or officinal species originally included within it.

Canon XXI. of the A. O. U. Code is: "When no type is clearly indicated the author who first subdivides a genus may restrict the original name to such part of it as he may judge advisable, and such assignment shall not be subject to subsequent modification." This was not a new rule when announced by the A. O. U. in 1886, but was a part of the British Association Code originally promulgated in 1842, and reaffirmed by nearly every later code down to 1905, when three revolutionary ichthyologists came forward with the following as their Canon X.: "The type of a genus can be indicated by the original author only. * * * In every case, the determination of the type of a genus shall rest on evidence offered by the original author, and shall be in no wise affected by restrictions or modifications of the genus in question introduced by subsequent authors, nor shall the views or the dates of subsequent authors be considered as affecting the assignment of the type of a genus"! For such a reactionary and far-reaching proposition there should certainly be most convincing and satisfactory reasons, for it involves the overthrow of the consistent usage of the majority of systematists for the last half century, and invites at least temporary chaos in the place of what seemed permanent stability. The proposed new ruling should leave nothing to personal opinion, but should provide a rule of unquestionable applicability to all cases.

The argument for the new proposition is as follows: "It is believed that the principle that a generic name must be fixed by its original author is one of vital importance in nomenclature. All processes of fixing types by elimination or by any other resting on subsequent literature, lead only to confusion and to the frittering of time on irrelevant questions. The method of elimination can not be so defined as to lead to constant results in different hands. In general it is much more difficult to know to what types subsequent authors have restricted any name *than to know what the original author would have chosen as his type.* Most early writers who have dealt with Linnæan species have consciously or unconsciously encroached on the Linnæan groups rather than made definite restrictions in the meaning of the generic names."

In determining types and the tenability of names it is notorious that the systematist is and must be guided by what an author has done and not by what he may have intended to do, no matter how evident the unaccomplished intention may be. Rules, to be effective, must be rigidly enforced, regardless of personal preference in favor of some particular result. But the foregoing is a proposition to override rules and usages that have brought nomenclature to a reasonable condition of stability respecting a wide class of cases it is

now proposed to reopen and subject to a new decision based largely on personal caprice. How is thus stated: "This may be done by direct statement [on the part of the author] that a certain species is a type species [a statement at present always respected and welcomed], the leading species, the 'chef de file,' or by other phraseology conveying the same idea [information always welcomed and in these days earnestly searched for and regarded]; it may be indicated by the choice of a Linnæan or other specific name as the name of a genus [also, as said above, recognized as a guiding principle], or by some statement which shall clearly *indicate an idea in the author's mind* corresponding in fact, if not in name, to the modern conception of the type of a genus. [Here, unfortunately, is the loophole for diversity of opinion as to whether the author had such an idea, and, if so, which of several species best meets the author's unexpressed conception. The decision of one author, in many instances, is likely, in the nature of the case, to be different from that of another, and the firm ground absolutely necessary for the proposed revolutionary procedure is wanting. Finally,] The type of a Linnæan genus must be, in the phraseology attributed to Linnæus, 'the best known European or officinal species,' included by that author within the genus [—an injunction already in force]."

We have here then several sound principles, which are not new but already in force, and a new proposition to enable an author who is in too much of a hurry or too indolent to find out what other authors have done under the principle of elimination toward fixing the type of a genus not otherwise determined, to fix the type offhand for himself on the basis of his own conception of what the author's idea was as to the type of his group, when, in a large proportion of cases, the author almost unquestionably never gave the matter a thought, or even entertained the idea of a type in the modern sense. What he may have thought is, in most cases, purely a matter of guesswork.

It is not quite true, as said in the new ichthyological code, that 'the method of elimination can not be so defined as to lead to constant results in different hands.' The results will vary somewhat with the experience and qualifications of the user of the method, if the conditions of the question are especially complicated and perplexing; but my experience has been that experts in such cases rarely reach different conclusions, especially if they are able to confer and discuss the case.

Canon XI. of the new code is in line with Canon X. It reads: "In case a genus requiring subdivision or modification contains as originally formed more than one species, and the author of the genus does not in any way clearly indicate the type, the first species named in the text by the author as certainly belonging to the genus shall be considered as its type." The enforcement of this rule would obviously, in some instances at least, lead to the gratuitous displacement of generic names which have long since reached a stable equilibrium under the principle of the determination of the generic type by elimination—the disturbance of simple cases universally accepted as settled, and, therefore, a well nigh wanton proceeding.

4. *The Recognition of Variants of Generic Names.*—Modern codes of nomenclature are practically unanimous in ruling that a generic name is untenable 'which has been previously used for some other genus in the same kingdom.' It has been so generally understood that 'name' is to be taken in the philological sense of a district word, that no ruling appears to have been deemed necessary as to what really constitutes a name in a nomenclatorial sense; but usage—one may almost say universal usage—shows that words varying merely by endings denoting gender, or compound words differing only in the connective vowel, or in which certain consonants, notably *l* and *r*, are used single or double, or, in certain words of Greek origin, the retention or elimination of the aspirate, or the use of *i* in place of *y*, or *vice versa*, etc., do not constitute distinct words or 'names' in a nomenclatorial sense. In other words, it is held that names of genera must be etymologically distinct, however similar they may be in form or pronunciation. This is affirmed by the uniform practise of systematists for a century.

In view of the discovery in recent years of

the double employ of such a multitude of names in zoology, and the consequent wholesale elimination of those preoccupied though often of long currency; and also in view of the wide acceptance of the A. O. U. rule that names, generic or specific, 'are not to be rejected because of barbarous origin, for faulty construction, for inapplicability of meaning, or for erroneous signification,' and can be changed only to correct typographical errors, there has arisen a tendency to extend the rule of priority to the form of words, and to adopt names that vary to the extent of a single letter as tenable, whether etymologically the same or not. The first outbreak of this tendency, however, in code form, is furnished by the new ichthyological code, of which Canon XI., as given in *The Osprey,* reads:

"As a name is a word without necessary meaning, and as names are identified by their orthography, a generic name (typographical errors corrected) is distinct from all others not spelled in exactly the same way. Questions of etymology are not pertinent in case of adoption or rejection of names deemed preoccupied." The explanatory note following states that this canon "permits the use of generic names of like origin but of different genders or termination to remain tenable. All manner of confusion has been brought into nomenclature by the change of names because others nearly the same are in use. Thus the Ornithologists' Union sanction the cancellation of *Eremophila* because of the earlier genus *Eremophilus,* of *Parula* because of the earlier *Parulus,* and of *Helminthophaga* on account of *Helminthophagus.* On the other hand, *Pica* and *Picus* are allowed.* In ornithology this matter has been handled by a general agreement on the relatively few cases concerned. But in other groups, the matter is by no means simple, and every degree of similarity can be found."

* In this exceptional case of *Pica* and *Picus,* so often cited as an inconsistency, these two words are not gender forms of one name, but etymologically distinct words, used by the ancient classical writers as the names of two widely different birds, just as they are still used in ornithological nomenclature. Furthermore, it is a unique case.

This is the 'one-letter rule' *par excellence,* of which there have been mutterings of late in various quarters. Its promoters have good intentions, and high hopes, no doubt, that it will prove a panacea for an admitted evil. Possibly a beneficial compromise may result. When we reflect, however, that two forms of the same name, differing only by a single letter, sometimes occur in the same class, and often in the same branch, and that the same name when used for the same genus is current in several forms, differing sometimes more radically than by a single letter, and that, in many cases, the author of a name has himself used it at different times in all three genders, and sometimes in more than one gender in the same paper, and that many authors have in the past, and some still continue to exercise their own judgment or preference as to the correct gender of names, it seems hopeless to expect such a radical innovation to meet with general acceptance. By a slip of the pen or other lapsus even authors the most careful in such matters are sometimes caught using one form when they intended to use another. Many generic names have four to six variants that have been used for the same genus, while some of them may also have been current for wholly different genera. This seemingly should be enough to lay the goblin of the 'one-letter rule,' but it evidently is not, even with otherwise level-headed naturalists.

It would take too much space to illustrate the confusion and inconvenience that would arise from its serious adoption. For the full-fledged systematist illustration by concrete examples would seem to be superfluous.

It is a grievous inconvenience to have to abandon a long-current bird name or fish name for which one has almost formed an attachment as a household word, because some one has discovered that it had a prior use, perhaps only in a closely similar form, for some other genus of animals, perhaps insects, or mollusks, or cœlenterates, which had never before come within his horizon. In early days it was held that the same generic name could not be used for both animals and plants. The codes later ruled that there was no necessary connection between botanical nomenclature

and zoological nomenclature, and that the use of a generic name in the one kingdom did not debar its use in the other. The different branches of zoology have now become so extended and specialized that the same rule of divorce might well be extended to the different branches of zoology. Little, if any, confusion could arise to ornithologists, or mammalogists, or ichthyologists, if a bird name, a mammal name, or a fish name should have currency for a genus of insects, or mollusks, or crustaceans, or echinoderms, or in each of these branches. If it could be agreed—and I am aware of no opposition—that the same generic name may hold good in different branches of the animal kingdom, but must not be used twice in the same branch (as in vertebrates, for example), it would result in the restoration of not a few familiar names that have had to give way under the animal kingdom priority rule, and lessen, if not quite do away with the present incipient call for an impracticable 'one-letter rule.'

5. *The Authority for Names.*—It is difficult to see the reason for Canon XXIX., which appears not to be published in full in *The Condor*. It is contrary to current usage and to other modern codes, that the authority for a name, given in manuscript on a museum label, is to be cited as the proper authority for such names when published by another author, who supplies the description and assumes the responsibility for the species. This canon says: "If a writer ascribes one of his species to some one else, we must take his word for it. Thus the manuscript species of Kuhl and Van Hasselt in the Museum of Leyden, although printed by Cuvier and Valenciennes, should be ascribed to Kuhl and Van Hasselt." This is not only a confusion of responsibility, but is bibliographically misleading, tending to throw the investigator off the track in looking for the original description of the species. Unless the publishing author endorses the supposed new species, he simply ignores the manuscript name and takes the responsibility for its suppression, just as in the other case he takes the responsibility for its publication and supplies the necessary description. If the author of a manuscript name supplies a description to accompany it, which only rarely happens, and the publishing author uses it as inedited manuscript, then the author of the name is also the author of the description and is to be cited as the authority for the species. In the other case, the name should be cited, in synonyny, as Cuvier (ex Kuhl, MS.), and otherwise as simply Cuvier. In the case of inedited matter, the citation would be Kuhl (in Cuvier, etc.), and otherwise as Kuhl. This, like the other points criticized above, is a singularly retrograde step.

J. A. ALLEN.

CURRENT NOTES ON METEOROLOGY.

METEOROLOGICAL RESULTS OF THE BLUE HILL KITE WORK.

THE meteorological work done at the Blue Hill Observatory by means of kites has so often been alluded to in these 'Notes' that no comments on the value of this work are necessary at this time. The latest publication in this connection is a valuable report by H. H. Clayton, entitled 'The Diurnal and Annual Periods of Temperature, Humidity and Wind Velocity up to Four Kilometers in the Free Air, and the Average Vertical Gradients of these Elements at Blue Hill' (*Annals Astron. Obs. Harv. Coll.*, LVIII., Pt. I., 1904). Although some of the results herein discussed have already been brought forward in previous publications by Mr. Rotch and Mr. Clayton, the compact and careful summary now issued will be welcomed as giving a definite and complete presentation of the principal conclusions which have been reached through the well-known, extended and laborious series of scientific kite flights—a field of investigation in which Blue Hill has taken a front rank.

A study of the sources of error in the instruments and methods precedes the discussion of the results. Six possible sources of constant error are recognized as influencing the records, and also one source of error, not constant, which arises from temporary local differences of condition, and from the fact that the kites do not rise vertically. A glance at these preliminary pages will show with what extreme care the observations have been treat-