own share in terminologic discussion. Later I may comment upon certain points, *e. g.*, the alleged 'obscurities of the system' (which —in view of my long preaching and practise of clearness as the first essential of all scientific composition—you must pardon me for regarding as subjective), and the nature and extent of my philologic transgressions (in which connection I may refer to a paper read, by invitation, before the American Philological Association last winter). Now, in view of the fact that all my publications upon the subject either have been sent you or are otherwise accessible, I must express surprise and regret that the foot-note (translated from His) should cite only three of my less extended contributions (two of them privately printed), without mentioning the earlier, the later and the more comprehensive, *e. g.*, the article 'Anatomical Terminology' by S. H. Gage and myself, in the first edition of the 'Reference Handbook of the Medical Sciences,' 1889, our 'Anatomical Technology,' 1882 and 1897, my 'Neural Terms, International and National' (*Journal of Comparative Neurology,* 1896), and 'Some Misapprehensions as to the Simplified Nomenclature of Anatomy' (1898), Science, April 21, 1899. The several reports of the committees of the Association of American Anatomists, the American Neurological Association and the American Association for the Advancement of Science should have been specified, and it would have been simple justice to name Mrs. Gage, Gerrish, Gould, Huntington, Leidy, the Spitzkas, father and son, and others. Finally, American students should be aware that the subject was definitely brought before the American Association for the Advancement of Science as long ago as 1880, and that a committee of that body was appointed in 1884, three years prior to the date when, as stated by you, 'Germany took the lead.'

In my 'Neural Terms' and 'Some Misapprehensions' I tried to give due credit to earlier simplifiers, Barclay, Owen, Henle, etc. When you and some other anatomists in this country take equal pains to inform yourselves fully as to the facts and principles involved, I believe you will concede that the good and enduring features of the neurologic portion of the [B N A] had been previously adopted or proposed by me, and you will realize that the unprejudiced consideration of the terms preferred by me would have been more advantageous to anatomy and more creditable to yourselves than their premature condemnation.

A copy of this letter will be sent to Science and *American Medicine.*

Very truly yours,

Burt G. Wilder.

October 11, 1906.

LEFT-HANDEDNESS.

To the Editor of Science: The question of right-handedness has been brought to my notice, and I should like to inquire whether any of your readers has actually counted the number of left-handed men and women in a tribe. Very few implements of savagery are reliable witnesses. The throwing sticks of Eskimo men and the short-handed skin dressers of the women are infallible, since they fit only one hand. In the National Museum, among a great number of throwing sticks—from east Greenland to Sitka, only two are left-handed and both are from the same locality. There is not a left-handed woman's implement in the museum.

O. T. Mason.

October 20, 1906.

SPECIAL ARTICLES.

THE RELATIVE MERITS OF THE 'ELIMINATION' AND 'FIRST SPECIES' METHOD IN FIXING THE TYPES OF GENERA—WITH SPECIAL REFERENCE TO ORNITHOLOGY.

In attempting to fix the types of any group of genera we shall find that a large number are monotypic, another lot have had their types designated by their authors, a few are fixed by the rule of tautonomy[1] and a certain number are left without any indication of a type—usually complex heterogeneous genera of the older authors. It is these that are always giving us trouble and these alone with which the problem of fixing types is concerned.

It seems to me that it is the duty of those engaged in nomenclatural work to-day to establish our names on as firm a basis as pos-

[1] See Science, V., No. 16, pp. 114–115, July 18, 1902.

sible, fortified by rules that will leave no chance for personal opinion and subsequent alteration.

With this idea in mind I have given the subject of 'type-fixing' much thought and study, taking for my especial investigation the genera of North American birds, a group which for twenty years has been constantly under the scrutiny of a committee on nomenclature and which has been subjected to about as much changing as any group of genera with which I am acquainted.

As to the merits of the two principal methods of fixing types, my investigations lead me to strongly favor the plan of selecting the first species mentioned.[2]

Its advantages are:

(*a*) Personal opinion is eliminated, two persons can not reach different conclusions.

(*b*) The question is settled independently for each genus, the result does not depend upon the fixing of the type of some other genus.

(*c*) The possibility of change in a generic name rests solely upon the question of priority, and the discovery of an error in the usually accepted date of a publication has no bearing upon the *types* of genera.

(*d*) It is necessary to consult only the original reference to ascertain the type of the genus.

Contrasting with these my objections to the method of 'elimination' as embodied in the A. O. U. Code:

(*a*) It permits the greatest range of personal opinion in the method of its application and the almost endless combinations of principles which it presents.

(*b*) In ascertaining the type of one genus it is often necessary to eliminate one or more others first and an error in one operation affects the others; in fact the genera stand in an interdependent series and a change in the type of one may affect a number of others.

(*c*) The discovery of an error in the date of a publication affects not only the priority of the genera therein described but also every operation of elimination where these genera have been involved; and the types of other genera will be altered simply because the type of one of these genera has been taken out at the wrong date.

(*d*) To ascertain the type by elimination it is necessary to consult every work in which genera have been erected upon any of the included species; also every work where some subsequent author may have specifically selected a type for the genus. It is manifestly impossible to be sure when one has exhausted the latter literature.

Dr. C. W. Stiles's method, as detailed in his paper on 'The Determination of Generic Types,'[3] seems to me to be the perfection of the 'elimination' idea and while better than that, inasmuch as it is more complete and more logical, it is open to objection in even greater degree on account of its necessary complexity.

While I have the greatest admiration for Dr. Stiles's handling of this subject, I can not see how his method can be generally adopted. The systematist can not afford to waste time in studying the application of nineteen rules and recommendations containing thirteen secondary suggestions in fixing the type of a genus. What he must have is simplicity and definiteness. To use a mathematical simile he wants elementary arithmetic rather than calculus.

Now as to the arguments advanced in favor of elimination. It is claimed that:

(*a*) It upholds the work of our predecessors by accepting the genera that they have from year to year separated off from the original composite genus, so that the residue must be what they regarded as the type of the original genus. This argument however, amounts to little or nothing, as in the past many men were working wholly independently of one another and by 'elimination' we inextricably confuse two or more independent lines of work, arriving at results which are probably not in accord

[2] In the case of Linnæan genera I realize that no good can come of enforcing this rule, but we have practical unanimity of opinion on the types of these genera, and I see no reason why we may not accept them arbitrarily just as we accept the genera themselves as our starting point. The A. O. U. Code moreover does not demand consistent elimination for Linnæan names.

[3] Bull. 79, Bureau Anim. Indust., U. S. Dept. Agric.

with either. Furthermore, many early authors had no conception of a type species and here it seems to me our selection must by any method be an arbitrary one.

(*b*) It is claimed that because we have followed 'elimination' so long in certain groups —as in North American birds, for instance— we should be unwarranted in reversing our method, because it would involve an immense number of changes in generic names which have been fixed by elimination.

This is a serious question and one which I have looked into very carefully. As a test I have consulted the original publication of 391 genera and subgenera of North American birds as given in the A. O. U. Check List, the works in which the other 19 occurred being inaccessible. I find 9 based upon diagnoses without citation of species, while 5 are *nomina nuda* or not used in a generic sense; 173 are monotypic, 59 have their types designated by their authors and 21 are fixed by tautonomy, leaving 124 composite genera with no type stated.

Of these the type as accepted in the A. O. U. Check List is the first species in 92 cases and some other species in 32 cases. Of the latter 16 are Linnæan genera where the type is arbitrarily fixed (see antea), reducing the number that would be changed by adopting the 'first species' rule to 16. In two of these the new type would be congeneric with the old, so that there would be no change in the generic name and three must be changed in any case for reasons of priority. Some of the remainder, however, involve the change of two names each and the total change incident to the adoption of the first species rule would be 10 generic and 4 subgeneric names.

But let us look further. How consistently has elimination been applied? There are 92 genera in which the first species is taken as the type in the A. O. U. Check List, presumably as the result of elimination; but was elimination employed in each case? Let us see.

In 25 cases the original species were all congeneric, and elimination being impossible the first species was selected as the type. In 5 only a partial elimination was possible and the same plan was adopted.

In 21 cases elimination, as I understand it, fixes the type on the first species, as accepted in the Check List, but in 12 cases it fixes it upon some other species. There are also two cases where the tautonomy rule will compel a change and one where the designation of a type by a subsequent author has been overlooked, while 18 are Linnæan genera and 8 I can not decide positively by elimination.

In all 12 generic and 3 subgeneric names will certainly be changed if elimination is consistently applied, and the types of 5 other genera will change but fortunately fall upon congeneric species.

It may be claimed that I did not eliminate properly in all these instances, but in all confusing cases I have followed the practise of Dr. J. A. Allen, who was one of the framers of the code and who has freely and cordially advised me in this matter.

It will be seen from the above that far from causing an overturning of our ornithological nomenclature the adoption of the first species rule will cause less change than our adherence to elimination.

As a further test I have examined the bird genera of the world from 1758 to 1820, comprising 513 names exclusive of Linnæus. Of these 282 are monotypic, 94 have their types fixed by tautonomy, 18 are based upon diagnoses only, leaving 119 composite genera in which no type is indicated. The selected type, according to the British Museum Catalogue, is the first species in 102 cases and some other species in 17 cases.[4] The task of working out the results by elimination I have not ventured to attempt.

It remains now to show the various ways in which 'elimination' is applied in practise. For this purpose I prepared the following series of questions which were intended to cover the elementary principles of elimination:

Question I.

Genus A, 1850.
Species 1 = type of *B* 1860.
2 = type of *C* 1870.
3 = type of *D* 1880.

[4] In these the action is usually arbitrary, seldom or never the result of 'elimination.'

Genus X, 1850.
Species 4 = type of *Y* 1810.
5 = type of *Z* 1820.
6 = type of *W* 1860.

Is not 3 the type of *A* and 6 the type of *X*?

Question II.

Genus A, 1800.
Species 1 = type of *B* 1810.
2 belongs to *B* (according to our present views).
3 = type of *C* 1820.

Which is the type of *A*, species 2 or 3?

Question III.

Genus A, 1760.
Species 1 = type of *B* 1770.
2 belongs to *C* 1780 (the type of *C* is another species not included in *A*).
3 = type of *D* 1790.

Which is the type of *A*, species 2 or 3?

Question IV.

Genus A, 1850.
Species 1 = type of *B* 1860.
2 = type of *C* 1870.
3 = type of *D* 1880.

1. In our opinion to-day *D* is a synonym of another genus *E*, 1855. Do we consider species 3 removed from *A* at 1855 or 1880?

2. Suppose we consider *D* a synonym of *B*. Is species 3 then removed at 1860 or 1880?

Question V.

Genus A, 1800.
Species 1 = type of *B* 1810.
2 = type of *C* 1820.
3 = type of *D* 1830.
4 = type of *E* 1840.

1. Suppose we regard *E* as a synonym of *C* and consider both 2 and 4 removed at 1820, then is not 3 the type of *A*?

2. Suppose we regard *C* and *E* as distinct genera, then is not 4 the type of *A*?

3. Now if an author adopting the first view makes 3 the type of *A*, must a subsequent author holding the second view adopt 3 or may he change the type of *A* to 4?

Question VI.

When a reviser explicitly selects a type for an early composite genus must he take a species that has never up to that time been removed from that genus as the basis of a new one, or is his action binding, no matter what species he may select so long as it is one of the originally included species?

Question VII.

Genus A, 1800.
Species 1 = type of *C* 1804.
2 = type of *D* 1806.
4 = type of *F* 1805.

Genus B, 1802.
Species 1 = type of *C* 1804.
2 = type of *D* 1806.
3 = type of *E* 1808.

Two genera erected independently for nearly the same species. In eliminating *A* genus B must be considered, since 1 and 2 are contained in it, but we can not ascertain the type of *B* until we know the type of *A*. How can such cases be treated?

Question VIII.

Genus A, 1800.
Species 1 = type of *C* 1820.
2 = type of *D* 1825.
3 = type of *E* 1830.

Genus F, 1840.
Species 1 = type of *C* 1820.
2 = type of *D* 1825.
3 = { type of *E* 1830. / type of *A* 1800. }
4 = type of *B* 1810.

Two genera established for nearly the same species. Is not 3 the type of *A*? If so what is the type of *F*?

These were submitted to the following twenty-five systematic zoologists and botanists:

Vertebrate Zoologists.—J. A. Allen, B. W. Evermann, Theodore Gill, O. P. Hay,* H. W. Henshaw, D. S. Jordan, C. Hart Merriam, G. S. Miller, Jr.,* H. C. Oberholser, W. H. Osgood, T. S. Palmer, C. W. Richmond, Robt. Ridgway, Leonhard Stejneger,* Witmer Stone.

Invertebrate Zoologists.—T. D. A. Cockerell, W. H. Dall, L. O. Howard,* H. A. Pilsbry, Mary J. Rathbun, C. W. Stiles.

Botanists.—J. H. Barnhart, N. L. Britton, O. F. Cook, F. V. Coville.

Replies were received from all but those marked with an asterisk, and for these answers I desire to express my thanks. All of the botanists and Professor Cockerell believe in

adopting the first species as the type and, therefore, not being accustomed to use elimination, declined to attempt to answer the questions. Dr. Jordan and some others also adopt the first species rule, but, having used elimination at one time or another, answered according to their interpretation of this method.

Dr. Stiles adopts his method of exclusion; and Drs. Gill, Palmer and Evermann believe that further knowledge of individual cases would necessitate different answers from those they have given, a view which to my mind makes a hard and fast rule impossible and opens the door wider than ever to individual opinion.

A summary of the answers follows:

Question I.

13 answer yes to both.
1 answers yes to (*a*), no to (*b*).
1 answers yes, 'with reservations.'
1 'depends on further history.'

Question II.

10 answer sp. 3.
4 answer sp. 2.
1 answers sp. 1.
1 'depends on further history.'

Question III.

9 answer sp. 3.
5 answer sp. 2.
1 answers sp. 1.
1 'depends on history.'

Question IV.

(*a*)

7 say 1855.
8 say 1880.
1 says date when synonymy was first recognized.

(*b*)

8 say 1860.
6 say 1880.
2 say 'depends on history.'

Question V.

(*a*)

12 say yes (4 with reservations).
3 say no.
1 'depends.'

(*b*)

15 say yes.
1 'depends.'

(*c*)

5 say change.
8 say no change.
3 in doubt.

Question VI.

14 say yes.
1 says no.
1 'not necessarily.'

Question VII.

(*A*)

8 say sp. 2.
5 say sp. 4.
2 in doubt.
1 $A = B$ absolutely.

(*B*)

14 say sp. 3.
1 in doubt.

Question VIII.

(*A*)

15 say type $A = 3$.
1 says $A = F$ absolutely.

(*B*)

4 say sp. 4.
2 say sp. 2.
4 say sp. 3.
1 says 1, 2, 3 or 4.
1 says ?
3 say F has no standing.

These questions were purposely made as simple as possible in order not to involve two or more principles in one example, but the cases encountered in actual practise are usually far more complicated; the diversity of opinion upon them can readily be imagined.

The points that I have tried to bring out in this discussion are:

(*a*) That 'elimination' even in the best hands will not give uniform results and that any attempt to formulate minute rules for its application will create a system too complicated for general use.

(*b*) That if elimination be uniformly applied to all complex genera, our nomenclature will undergo more changes than if the 'first species' rule be adopted. I have, I think, proved this so far as ornithology is concerned, and I have no doubt the same conditions will be found to prevail in other branches. Elimination has never been practised in Europe and does not seem to be understood by foreign

writers, and in the vast majority of cases the first species is taken by them as the type. In nearly every case where the A. O. U. Check List and the British Museum Catalogue differ in the selection of a type species for an ornithological genus, the adoption of the first species by the Americans will bring them into accord.

(*c*) That we have in the 'first species rule' a method that can lead to but one result and can be practised by any one, and by which the type of a genus can be ascertained at once by consulting one reference, instead of involving the examination of many works and the expenditure of much time and thought.

WITMER STONE.

ACADEMY OF NATURAL SCIENCES, PHILADELPHIA.

GENERIC NAMES OF MERYCOIDODONTS.

As there has been no recent thorough revision of the *Merycoidodonts* (*Oreodonts auctorum*), based upon an examination and comparison of all the types, there has been much confusion and error in the use of nearly all of the generic names. Many new forms have recently been discovered, and investigation has been greatly retarded by uncertainty as to where many of these should be placed. By the kindness of those who are in charge of the various museums which contain the types of the genera, the writer has had the opportunity of examining all of the older types, and he here gives his conclusions concerning the various names which have been used.

MERYCOIDODON Leidy.

Type *Merycoidodon culbertsoni* Leidy. Proceedings Academy Natural Sciences, Philadelphia, Vol. IV., 1848, p. 47, Plate.

Synonyms: *Oreodon culbertsoni* (Leidy), *O. priscum* Leidy, *Cotylops speciosa* Leidy.

The type is a portion of the upper jaw with the last two molars, and a fragment of the mandible with all the lower molars. The outer cusps of the second upper molar, and the heel of the last lower molar are gone. The type was sent from the Bad Lands of Dakota by Mr. T. Culbertson and is now the property of the Academy of Natural Sciences in Philadelphia. The two specimens probably belong to the same individual, as the last molar in both jaws is in about the same stage of eruption. These molars, though fully formed, had not yet attained the level of the other teeth, but they are well exposed, so that their structure can be easily seen.

Dr. O. P. Hay (SCIENCE, Vol. IX., April 21, 1899, p. 593, and 'Catalogue of the Fossil Vertebrates of North America,' p. 665) has reinstated the original name *Merycoidodon* in the place of *Oreodon,* which had come into universal use. He says that *Merycoidodon* clearly has priority over both *Oreodon* and *Cotylops.* There is a close similarity in the teeth of the Middle Oligocene *Merycoidodonts,* and it seemed best, at least until the type should be found and its identity with '*Oreodon*' demonstrated, to use the commonly accepted name; but now, after having examined the type and compared it with various specimens of so-called *Oreodon culbertsoni,* I believe that the original name should be used for the following reasons:

1. The type specimen was fully described by Leidy and figures were published, which, though not clear enough, perhaps, to distinguish *Merycoidodon* from specimens of closely allied genera, leave no doubt as to the identity of the type specimen.

2. The name *Cotylops* was given to a young individual with the milk dentition. The type of *Oreodon* was the 'greater portion of a cranium' with teeth in a very much mutilated condition, sent to Dr. Leidy by Dr. Hiram Prout, of St. Louis. (Proceedings Academy Natural Sciences of Philadelphia, Vol. V., p. 237.)

3. This type appears to be lost, but Dr. Leidy, who was a careful observer, had the types of *Merycoidodon, Oreodon* and *Cotylops* all before him and he said that the true molars of *Oreodon* had exactly the same form and very nearly the same size as the posterior two molars of *Merycoidodon.* He afterwards concluded that these genera belonged to the same species.

4. So far as the present writer has observed there are differences, though not great, which separate *Merycoidodon* from nearly related