A Discussion of Certain Questions of Nomenclature, as applied to Parasites.

By

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One of the most important, most scientific, and most admirable papers which has ever been published on the Trematodes was issued about a year ago by Prof. ARTHUR LOOSS[1]), of Cairo, Egypt. While some helminthologists may for the time being remain rather reserved in regard to certain of the genera which LOOSS has proposed, probably none of us will refuse to acknowledge that he has issued a record-breaking work which calls for high admiration. Exact and thorough as the article is, when viewed from the standpoint of anatomy, there are nomenclatural rulings adopted and propositions made, which if generally followed, would result in serious confusion. On this account, the Zoological Laboratory of this Bureau has been endeavoring to collect all of the technical names of Trematodes printed since 1758, with their date of publication. By issuing such a list, together with a general discussion of nomenclatural principles as applied to animal parasites, it is hoped that the present confusion in the subject may be reduced and future confusion avoided. Unavoidable circumstances will delay the proposed paper, but the following pages have been extracted from the manuscript, now a year old, and are offered for publication, as certain questions involved call for an earlier discussion — especially since the weight which LOOSS' opinion naturally carries with

1) Weitere Beiträge zur Kenntniss der Trematoden-Fauna Aegyptens, zugleich Versuch einer natürlichen Gliederung des Genus Distomum RETZIUS, in: Zool. Jahrb., V. 12, Syst., 1899.

it, relative to anatomical matters, has already been accepted by some authors as authoritative support for his nomenclatural views, thus giving the latter an artificial value (Looss himself admitting that he is not a nomenclaturalist). Furthermore, certain views recently expressed[1]) by Braun and by Lühe are also open to discussion.

1. Helminthology is a speciality in zoology, hence subject to general zoological rules.

We should start out with the conviction that helminthologists are zoologists; we are specialists in a small field of zoology; we therefore are bound professionally to make the nomenclature of our speciality conform to principles which are identical with those adopted by zoologists at large, and we should not support or adopt any practice which is antogonistic to the stability of the nomenclature used by our colleagues in other departments of zoology. We are only one organ of a large body, and no precedent can be adopted which is calculated to render that organ a teratological specimen when compared with the entire body. We should not place ourselves in the position of a tail attempting to wag the dog.

On the other hand, we have a right to maintain that our speciality shall have the same consideration in the framing of principles and practices which other zoological specialities enjoy. We may even advance the claim that our field of work is more intimately connected than almost any other speciality in systematic zoology with the nomenclature of physicians and veterinarians; hence that it is well for us to be more or less conservative in nomenclatural propositions, since a change of generic and specific names in our groups is frequently calculated to result in a change of names in medical textbooks, and in official regulations in meat inspection. And while all due weight is given to this intimate relation between parasitology and medicine, let us not forget that we owe a duty not only to the present generation but to future generations as well.

We should impress the fact upon our memories that zoologists have only commenced with the naming of animals. Millions of species still remain unnamed and undescribed. The numerous scientific

1) Almost constant absence from my laboratory since March, 1898, has made it impossible for me to follow all of the recent helminthological writings. Nomenclatural papers by other authors therefore may have escaped my attention.

names in use today, therefore, represent only a fraction of those with which the zoologist several centuries hence will have to deal. Under these circumstances, it is not too much to state that every zoologist is under a professional obligation to future generations of scientific workers not to unjustifiably introduce a new name or change an old one. Under what circumstances he may be justified in such action may be judged from rules of nomenclature which experience and logic have shown to be well-founded.

A code of nomenclature represents the combined opinion of men who have had practical experience in the questions at issue as to the circumstances under which names may be recognized, retained, rejected, and changed.

2. The law of priority.

The law of priority has been described as the "fetich worshiped by nomenclaturalists". Although this was said in ridicule, there appears to be no necessity for disproving the allegation, for it is not entirely without foundation; and if the comparison will only be carried out further, those who oppose this law will easily understand — even if they do not approve of — the tenacity with which we cling uncompromisingly to that "fetich": it is because of our conviction that no other, substitute, proposition has ever been submitted which can be consistently carried out or which offers the possibility of a stable and international system of nomenclature.

Reduced to the last analysis, we have before us a choice of the objective law of priority or of a subjective system of authority. That is to say, we must choose between using the oldest available name, or the name used by some person whom we look upon as an "authority" in the group in question. Those who follow the law of priority are in the right in either case, for the determination of "who are the authorities" in any given group depends to no small extent upon the point of view from which the group is studied. It is perfectly legitimate for a worker to interpret the proposer of the first available name as an "authority" regarding the particular species or genus in question, and if an author wishes to adopt the first available name "on authority of" its proposer, the principle (?) pleaded for by those who oppose the law of priority is complied with. Wherein, therefore, does the writer who follows priority offend against the system of authority?

It will be objected that the first author of a name may not be conceded by all to be an "authority". Granted, but from what standpoint is "authority" to be judged? RUDOLF LEUCKART was an infinitely superior authority in helminthology to RUDOLPHI, when judged from the standpoint of an histologist, embryologist, or anatomist; but it may be considered an open question whether RUDOLPHI was not the greater authority as a systematist. Which "authority" should be selected? And suppose some helminthologist in the near future exceeds in prominence both RUDOLPHI and LEUCKART, shall we then follow his personal whims regarding names?

Reducing the subject to a nutshell, those authors who "worship the fetich of priority" see but two choices open: Either to adopt the oldest available name, regardless of personal interests, national pride, or any other consideration, or to adopt haphazard any name one chooses.

In helminthology, authors have been backward in declaring themselves in favor of "priority". Even within the last few years, leading helminthologists like LEUCKART, BRAUN, and LOOSS, have not only thrown priority to the winds by disregarding it in their writings, but have even argued against it. More recently, however, both BRAUN and LOOSS show a change of opinion on the subject and have apparently become convinced of the advantage of the only system which offers us a stable nomenclature.

Accepting the law of priority as a necessity, it remains to be determined:

3. At what date should the law of priority become operative, LINNAEUS, 1758 or RUDOLPHI, 1819?

The answer to this question is not so selfevident as it would seem, for it involves not only principle but also possibility. It would not be discussed here were it not for a proposition recently made by LOOSS.

We must strive our utmost for the abstract of the principle; but the success of our efforts depends upon the possibilities which result from our powers and the existing conditions, hence upon the practical feasibility of our general rules. Far better is it to make the law our servant — to carry out our purpose of making a stable nomenclature, than to make ourselves its slaves and thereby defeat the very object of the adoption of the law. Far better is it to strive in the spirit of the law so far as there is the slightest outlook for definite results, than to strive beyond that outlook and fall into a chasm of uncertainty and confusion.

In connection with the important point at issue, the following propositions have been made:

1. To accept the 10th edition of LINNAEUS' Systema naturae.
2. To accept the 12th edition of LINNAEUS' Systema naturae.
3. To accept absolute priority, going back to pre-Linnaean names.
4. To reject all names which have not been recognized for twenty-five years ("Statute of Limitation").
5. For each speciality to determine its own starting point.

After a lengthy discussion of the principles and difficulties involved, a discussion extending over many years, engaged in by numerous systematists of different nationalities and representing different groups, LINNAEUS' Systema naturae, ed. X, 1758, has by the vote of a number of zoological societies, national and international, special and general, been adopted as the starting point for the operation of the law of priority for all zoological groups.

LOOSS has recently dissented from this majority decision, and has proposed to accept a special date for helminthology. The idea involved is not a new one. In fact several authors have from time to time advanced the view that different groups might take different dates as basis for their nomenclatural work. All such propositions have had one and the same history: Although several persons have eagerly defended them, they have been rejected by the vast majority of experienced nomenclaturalists and have eventually been forgotten or rejected even by their proposers.

Notwithstanding the past history of this proposal, LOOSS (1899, p. 525) has very recently brought it forward again by definitely proposing to helminthologists that we should adopt RUDOLPHI's Entozoorum synopsis (1819) as our starting point, instead of LINNAEUS' (1758) Systema naturae.

In justification of his suggestion, LOOSS advances the arguments: 1) that although LINNAEUS was the father of binomial nomenclature of the free living animals, RUDOLPHI was the father of binomial nomenclature for the parasitic worms (RUDOLPHI is spoken of as the LINNAEUS of helminthology); 2) that RUDOLPHI did not unnecessarily change preexisting names, but preserved "all the good names of the older authors which fulfilled the scientific requirements"; 3) that to revert to pre-RUDOLPHI authors would result in overturning "a good part" of the current nomenclature of parasites.

Before discussing the proposition, attention may first be directed to the fact that LOOSS himself disarms and weakens his argument by warning his readers that he is not in a position to judge the broader questions of nomenclature. He says:

"Es sei mir gestattet, diese Behauptung hier etwas näher zu begründen und dabei zugleich auf einige weitere Punkte hinzuweisen, in denen eine Aenderung oder wenigstens eine präcisere Fassung der bestehenden Vorschriften wünschenswerth erscheint, wenigstens für die helminthologische Wissenschaft. Dass auch auf andern Specialgebieten der Zoologie ähnliche praktische Schwierigkeiten sich einstellen, ist nicht unmöglich, doch habe ich darüber kein Urtheil."

Thus LOOSS admits from the start that he has given but superficial attention to the principles and practices of zoological nomenclature in general — an admission on his part which must naturally make every reflecting author exceedingly cautious about adopting the new nomenclatural proposition; he admits that his proposal is made without reflecting upon its influence, if adopted, upon other groups of animals; he admits that his study of nomenclatural practices is confined to one small speciality which contains but a small percentage of the known genera and species of the world, and in which the theory and precedents of a scientific system of nomenclature have received but little attention. Let us be duly appreciative of the importance and the frankness of this admission, when judging LOOSS' nomenclatural propositions and rulings.

Turning now to the arguments advanced in favor of his suggestion, it must be submitted in reply that they are not free from criticism.

To the statement that RUDOLPHI is the LINNAEUS of helminthology, it may be replied that with all due appreciation of the keen sense of classification which the learned Austrian exhibited, he had very competent predecessors in his nomenclatural and systematic work. GMELIN (1790), BATSCH (1786), and ZEDER (1800 and 1803) published synopses of the many parasitic worms know to them, and they applied the Linnaean binomial nomenclature as consistently as do many helminthologists to-day. RUDOLPHI'S right to a higher consideration than is granted to GMELIN, BATSCH, and ZEDER is not apparent. Even GOEZE (1782) used a nomenclature which, though often difficult to interpret, is in the spirit of the Linnaean system. Why should we ignore the work of these men? To do so without very good reasons — reasons which can be thoroughly supported by principles and precedents established by men who have had wide experience in nomenclatural studies, is simply to invite some future generation of helminthologists to adopt some very complete systematic work published perhaps in 1950 or 2000, and perhaps based upon a total disregard of the Law of Priority, as their point of departure, and to ignore all preceding work directed toward a stable zoological nomenclature. I, for one, do not desire to set such a dangerous example.

To Looss' second point (that Rudolphi did not unnessarily change preexisting names, but preserved "all the good names of the older authors which fulfilled the scientific requirements"), it must be replied that it is found necessary to take direct issue with my esteemed friend upon this assertion. Not only did Rudolphi unnecessarily and in a most wanton manner change names adopted by his predecessors, but in his later works he unnecessarily changed names which he himself had proposed or adopted in his earlier writings. Wherein lay the necessity of adopting *Distoma* in 1808—10, 1814, and 1819, in place of *Fasciola* used by him in 1801—3? Wherein was the necessity of introducing numerous new specific names for forms which he himself identified as identical with forms described under other names by earlier binomial authors?

Looss' third point (that to revert to pre-Rudolphi names would result in overturning a good part of the current nomenclature of parasites), while rather indefinite, is the same argument which many persons have advanced against the Law of Priority itself, and which any one could equally well advance in favor of accepting even, for certain genera, authors who have published within recent years — rather than to revert to older names. It may also be remarked that Looss has not made a very definite statement as to what constitutes "a good part" of the current nomenclature. Looss' third argument accordingly cannot be given much weight.

Turning now to a phase of the subject which my friend did not discuss: If Rudolphi was such a second Linnaeus and so consistent a nomenclaturalist, why should we adopt his Synopsis (1819) instead of his Historia (1808—10) or his Beobachtungen (1801—3)? Of course, there would be the great advantage that we should then have one year as basis, instead of three. But, by accepting 1819, instead of 1801—3, we should violate one of the most important principles of nomenclature, namely that when an author once publishes a name, he has no rights over that name which other authors do not have. Why should we accord to RUDOLPHI an exception which is contrary to the entire spirit of nomenclatural precedents, and one which is not accorded to any other author, living or dead, — not even to LINNAEUS?

Further, even if it could be admitted that all of Looss' arguments were valid, a moment's consideration will show that his proposition cannot be carried out either theoretically or practically, and an attempt to follow it would set an example and produce a confusion, the influence and extent of which cannot be foreseen, thus defeating the chief object of nomenclatural rules, namely: as great a stability as possible in systematic names. Let it be recalled, for instance, that some animals are parasitic during a part of their life, and free living during the remainder. Would the genus *Gordius* date from LINNAEUS or from RUDOLPHI? What would be done with the hirudineans? Although they are not treated by RUDOLPHI, still BLANCHARD, as a helminthologist, would be justified in the interpretation that they should date from 1819. WHITMAN and MONTGOMERY, on the other hand — men who work chiefly in other fields — could with equal right claim that they should date from 1758, since they may be called free living forms, with the same right that they may be called parasitic.

Again, if we adopt for helminthology a starting point which is different from the date adopted by all other zoologists, we would thereby practically declare our nomenclature independent of zoological nomenclature in general. We would thus lose all logical basis of comparison with the generic names of other groups. In this event, should we accept *Distoma*[1]) because RUDOLPHI used it in 1819, to

1) Postscript. Upon returning to Washington after a prolonged absence, I find that LÜHE has already raised this point and that Looss has recently attempted to reply to it. Looss does not, however, meet the case. Suppose for instance LINNAEUS, 1758, is accepted by ornithologists; LATREILLE, 1796, by entomologists; RUDOLPHI, 1819, by helminthologists; GURLEY, 1894, in Myxosporidia, etc. Upon specializing further, as we are bound to do in the future, each set of workers in a smaller group might claim some new starting point: One for Trematodes, another for Cestodes, another for Nematodes, sixteen to nineteen for insects, etc. Animals are not always placed in the same group. Upon being transferred to another order or class, their nomenclature would take another starting point. Further, if separate starting points

the exclusion of *Fasciola* LINNAEUS, 1758, and in the face of *Distoma* SAV., 1816 (a mollusk)? How should we rule upon *Eurysoma* GISTL., 1829 (coleopteron), *Eurysoma* KOCH, 1840 (arachnoid), and *Eurysoma* DUJARDIN, 1845 (a trematode)? The nomenclature of helminthology must be either independent of general zoological nomenclature, or the two must be interdependent. An interdependence is very difficult, unless we recognize the same work as starting point; an independence would permit the use of *Distoma* and *Eurysoma* in trematodes, and the use of the same names in other groups. To this it cannot be replied that we could recognize their interdependence since 1819, for even if we adopted this date for helminthology, we should still be forced to consider the names published in other groups between 1758 and 1819 (and hence recognize the date, 1758) — and this while we refused to consider the names published in our own group during the same years.

Finally, let us consider the dangerous precedent we should be setting to specialists in other groups, — in the sporozoa, for example. With the same right that we selected 1819 for helminthology, workers in the Myxosporidia could adopt GURLEY 1894, as their starting point. My official duties compel me, personally, to keep myself more or less informed in regard to the worms, the sporozoa, and the insects infesting man and the domesticated animals. Let us now image the confusion if a system of nomenclature permitted me or any other author to adopt one date for worms, another for sporozoa, and a third for insects. Let us assume that the nomenclature of all three groups is declared independent, and that it was permitted to use *Distoma* as a valid name in all these divisions. Let us imagine the lucidity of an article on the parasites of man with *Distoma* X (a worm), *Distoma* Y (as a Sporozoon), *Distoma* Z (as an insect). Such a possibility, absurd as it appears, is the logical result of LOOSS' proposition.

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are taken, few authors would ever go back of the date selected for his own group to determine whether a given name had been used in another group; and even if they did, the point would be raised — what is the starting point for the group in question? If conchologists should accept 1830 as their date, *Distoma* 1816 would be invalidated for mollusks, hence there would be no reason why *Distoma* 1819 should not be used for worms. LOOSS' reply to LÜHE presupposes that helminthologists rule that they accept 1819, and that all other writers accept 1758.

From the above it may be seen that the adoption of LOOSS' proposition would compel us to choose between the following:

1) Either we must take our position, contrary to the precedents of a century and a half, with a small minority, which claims that the same generic name may be used in two different groups of animals, and thus by bringing about an utterly chaotic state, give up immediately all idea of ever having an international nomenclature, or

2) we must ignore all names in our own speciality, published between 1758 and 1819, but theoretically recognize all names in other specialities (names with which we are less familiar) published between those dates.

It is my firm conviction that no group of specialists should adopt any precedent, rule, regulation, or recommendation, which cannot be brought into accord with the precedents of zoologists in general, and while I appreciate as keenly as does my friend LOOSS the difficulties of which he complains, I maintain that it is a professional duty of every helminthologist to bear with these temporary, irritating — often exasperating — troubles, for the general good of all parties concerned. And while LOOSS' proposition to adopt RUDOLPHI's Synopsis instead of LINNAEUS' Systema may appeal (and in fact has appealed) to some authors upon first thought as being an excellent solution of the present situation in helminthology, it is impossible for me to escape the conviction that it is one of the most dangerous and short sighted nomenclatural propositions ever suggested. To my regret, therefore, I am unable to adopt it. On the contrary, it is not clear to me that any arguments have been advanced which would justify a decision in favor of rejecting the 10th edition of the Systema naturae (1758), and hence would justify an author in adopting a plan which would eventually result in rejecting the thousands of nomenclatural decision made on this basis since 1846.

4. The face value of early descriptions.

An important point raised by LOOSS, in connection with the Lex prioritatis, touches the validity of names which are not "clearly defined or indicated" ("erkennbar definirt oder angedeutet"). He objects to speculation as to what an author meant, and practically calls for the acceptance of every diagnosis on its face value. If his article is read carefully, the important point will be noticed that his argument tends to judging the face value of the diagnosis by the present status of science, and not by the condition of the subject at the time the diagnosis was written or the species or genus indicated.

In the first place one must determine what characters are valid in recognizing a genus or species by its definition or indication. In reference to this point, I take the stand that any remark, reference, or indication which enables a specialist in the group in question to recognize with reasonable certainty what form is referred to, is a valid character and must be admitted, especially when judging the work of earlier authors. If, for instance, an earlier author proposed the name X y for "a worm about 3 feet long in the kidney of a dog", we may conclude with reasonable certainty that he referred to the female *Dioctophyme renale*, and we should hence recognize the name X y, although not a single anatomical character except length is given. Should it afterwards develop that two or twenty species, from the modern standpoint, had been included in this supposed single species, I should still feel it obligatory (ceteris paribus) upon me to retain X y for one of these forms.

Further, the truth must not be overlooked that a definition or indication which may perhaps not be recognizable to-day, may at some future date be perfectly clear, or at least it may be clear that the author used the name for such and such forms, which to-day are considered to belong to x different genera and y different species. Accordingly, we may refuse to recognize a name to-day, but may be compelled to give it recognition to-morrow. Hence in studying the nomenclature of earlier authors we should consider their definitions and indications in the light of the science, not only of to-day but also of the time when the articles were published. Any other position than this would necessitate our ignoring thousands of names proposed during the early part of this century, and would equally necessitate that the authors of next century should ignore thousands of names published during the past fifty years, names which can equally well be retained.

To make my position clear: I believe in retaining an early name, whenever we can find a reasonable excuse for doing so, since the older the name, the better. See, below, for instance, the genus *Sphaerostoma*. In this connection, it should be recalled that the earlier date we can give to a name the more secure it is from later rejection on account of the Rule of Homonyms, or from other cause. A generic name dated 1800 or 1850 stands a much greater chance of permanency than one dated 1890, since in the latter case it must compete for survival with thousands of more names proposed during the 90, resp. 40, years. There is less competition for survival among earlier than among later names, hence their chance for permanency is proportionately greater.

It will be noticed above that I have placed emphasis upon the interpretation of an early diagnosis, hence recognition of an early name, by a specialist in the group in question. In this point, I take direct issue with the position set forth by Looss. The latter claims that the definition or indication must be clear to "everyone" ("Jedem"). In opposition to Looss' view, it may be advanced that a definition or an indication of a parasitic worm may be perfectly clear to a helminthologist, but absolutely unintelligible to an ornithologist; it may be clear to a man of forty years experience, yet not to one of two years practical work. If the views expressed by Looss were accepted, consistency would compel the rejection of every diagnosis by the use of which any author or any student in the world has ever made an error in determining any genus or species. It is, therefore, difficult to believe that Looss really holds the view he has expressed.

I find it necessary to admit that I once held the same views Looss implies relative to the strictness with which early diagnoses should be interpreted, but the more I study the problems of nomenclature, the more necessary it seems to me to lay greater stress upon the condition of science in former years, and hence to accept any indication or diagnosis under which a species may be interpreted, rather than to demand of early authors a clear description under which a species must be interpreted. In parasites, the type-host should of course be considered in this connection, and in the case of other animals the type-locality will usually narrow the determination down to a choice between only a few species. Let us take a specific example as illustration. Rudolphi, 1809, p. 364—365, uses the name *Distoma globiporum* for a species which he himself recognized as identical with a form mentioned by earlier authors as *Fasciola bramae*, *Distoma cyprinaceum*, *D. carinatum*, and which he had named *Fasciola globipora*. In other words, he united under one species worms which at least one other author (Zeder) appears to have considered two species. Looss (1894, p. 41) admits that *Fasciola bramae*, *F. longicollis*, *Distoma cyprinaceum*, and *Fasciola globipora* are identical. Under these circumstances there appears to be no valid reason for not adopting the specific name *bramae* in place of *globipora*. But let us assume that the early synonymy cannot be clearly demonstrated from a modern point of view. In this event, it appears to me that since RUDOLPHI himself admitted that he considered his *F. globipora* as identical with the earlier *F. bramae*, and so clearly designated, this action of RUDOLPHI demands our acceptance of *bramae* as the specific name unless some one shows that *F. globipora* is not identical with *F. bramae*.

In other words, in judging the older cases of synonymy adopted by earlier authors, we should adopt the oldest name given unless we can show that their interpretation was incorrect. Such a ruling is entirely in the spirit of the rule for the union of genera, resp. species, which reads:

"A genus [resp. species] formed by the union of genera [resp. species] takes the oldest generic or subgeneric [resp. specific] name of its components. If the names are all of the same date, that selected by the first reviser shall be retained."

While a practical application of such an apparently extreme view would be calculated to strike consternation in the minds of most helminthologists, there can be no question but what it would more rapidly and permanently reduce our nomenclature to a firm foundation.

Thus it will be seen that LOOSS and I take very different positions on the question at issue. The logical application of LOOSS' view is that we start out with science as it is to-day, and so far as the early writers can be interpreted by our standard, let their generic and specific names be recognized. On the other hand, my view is that we should judge generic and specific descriptions of 1800 by the standards of the day when they were written, and not by the standards of a century later.

If LOOSS' view is consistently followed, the natural result will be to reject unnecessarily numerous names published before the days of the staining methods, and to introduce new names, thus subjecting the generic and specific designations to greater competition for survival. If on the other hand, the other plan is followed, namely to adopt for every genus or species the oldest published and indicated name under which that genus or species may be interpreted, we can easily save the old names, reject the new, and reduce to a minimum the competition for survival.

It may be recalled that the parasitologist has one great advantage over the zoologist who studies only free living animals. The latter has the type locality (namely, the locality where the first specimens were collected) as a clue to help him in his determinations, but we have besides the type locality, also the host and the organ from which the parasites were taken. If for instance an early author speaks of finding a red worm, nearly three feet long, in the kidney of a dog, in France, we may conclude with a considerable degree of certainty, what form he was discussing, if he mentions a *Strongylus*, over half an inch long, in the stomach of a sheep in Germany, only a limited number of species can come into consideration, with the chances decidedly in favor of *Str. contortus*.

It may also be recalled that if a name is dated 1860, and we find another designation dated 1850, it is by no means impossible that a few years after the change is made, we shall find still a third name for the same species dated 1830. Many medical and zoological publications have not yet been indexed — but when they are, some changes are bound to occur. Let us therefore in all cases, where it is possible, go back to the oldest name for the use of which any reasonable excuse can be found, and thus diminish the chances of a later change.

5. The relations of the law of priority to type specimens.

Looss further raises the oft discussed point regarding the type specimens, and practically advances the view that if we admit that the reexamination of these types is valid in determining a species in case the diagnosis or indication leaves us in doubt, we thereby practically contradict the wording of the Law of Priority. He argues that the published diagnosis is accessible to all, but the type specimens only to a few. The argument appears well founded until examined critically, and viewed in connection with its consequences.

The Law of Priority does not relieve an author from using every available means to determine a form. An exceedingly small form cannot be determined, no matter how exact the diagnosis, unless a worker has a microscope. Not every zoologist possesses this instrument, yet a man would not be justified in rejecting a name because under these circumstances he was not able to recognize a given species by its description. In some cases, a microtome must also be used, yet the man without a microtome is not justified in declining to recognize a form described from sections. Again, not every published description is immediately accessible to every zoologist in the world, Looss mentions his own difficulties in Egypt in this particular, yet who would claim that, if one of his magnificently described species proved to be identical with a form recognizably described in books accessible to workers in Europe or America, the Law of Priority is not applicable to the names in question?

The type or cotype or paratype specimens are accessible to all of us — where they still exist — if we will visit the museum in which they are deposited, and in many cases we can obtain them by writing for them. This is one of the means open to us to clear up early descriptions, just as much as the staining methods, the microscope, the microtome, or the dissecting knife, and it is difficult to see a contradiction in principle between the results of such a study and the law of priority.

In laying such stress upon the publication, it would appear that our friend has forgotten the history and purpose of requiring publication as a prerequisite to the consideration of a name. Attention may be called to the fact that in the evolution of the rules of nomenclature, the question arose as to whether a name should be recognized in case it is written or printed upon a museum label, but otherwise not accessible, and it has been decided that such names were not entitled to the Law of Priority. Not until a name is made public by definition or indication are we called upon to take it into consideration, but when once published in this manner, it is incumbent upon us to use every method at our disposal to recognize it before rejecting it.

As I read Looss, he is not opposed in principle to the reexamination of types, but simply wishes to direct attention to what he believes to be a contradiction, a contradiction which I am not in a position to admit.

A definition or an indication must, of course, give some tangible clew to the nature of the object named, but the reexamination of types is one of the necessities connected with the gradual evolution of zoology, and it is in recognition of this necessity that zoologists have laid such stress upon preserving types for future workers.

6. Nomina nuda.

Looss raises the important point as to the status of the names which Rudolphi gives to his species dubiae, such as *Distoma* *meropis* RUDOLPHI, 1819, and he maintains that these are nomina nuda.

The case at hand is a difficult one to decide satisfactorily, and one in regard to which there may be a legitimate difference of opinion. *D. meropis* was not named until 1819, and its only indication is the host and organ in which it occurs. This is undoubtedly a clew to the worm, hence *D. meropis* can hardly be called a nomen nudum in the sense in which this term is used in other groups. The organ and host species of a parasite are frequently all that is required for the determination of a species, provided its genus is known. Although it must be admitted that these cases are unfortunate, and although I confess a feeling of uncertainty regarding the position which should be taken in reference to them, still we have here an excuse for excepting earlier names, hence names less liable to later change. At present, therefore, I rather incline to the acceptance of names where only the organ and host species are given.

7. The rule of homonyms.

According to this rule, the later of two homonyms must be rejected. The rule sounds simple enough, yet there is considerable difference of opinion as to exactly what homonyms are. Two extremes of opinion may be noticed: The one extreme is represented by GILL, JORDAN, EVERMAN, and certain other workers (and the writer belongs in this category); the other extreme is represented by BLANCHARD, JENTINK, and a number of other authors. The International Commission endeavored to find a compromise, but its attempt cannot be called a success.

The first extreme starts out from the standpoint that words are either identical or different. If identical, they cannot be different, hence they are homonyms; and in judging the case, absolutely no consideration is given to the etymology of the word. Thus: *Abeona* GIARD 1854, and *Abeona* STÅL 1876, are identical (though, incidentally, of different etymology); *fluvialis*, *fluviatilis*, *fluviaticus*, and *fluviorum*, or *Distomus*, *Distoma*, and *Distomum*, or *silvestris* and *sylvestris*, or *coeruleus* and *caeruleus*, or *Rhopalophorus* and *Ropalophorus*, not being identical, respectively, must be different. Accordingly, these words are not homonyms. According to this view, the difference of a single letter, entirely regardless of the etymology, excludes the possibility of the words being identical, hence excludes the possibility of their being homonyms.

The other extreme places stress upon the etymology of a word, and while admitting that two words of different etymology but identical in form are homonyms, it maintains that two words of the same etymology, but different in form, are also homonyms. Thus, in accordance with this view, the examples given above would be homonyms.

A very limited number of entomologists go even further, and recognize words which sound alike ("phononyms") under the same rule as homonyms — an extreme which cannot count upon much support, since pronunciation differs according to the language we are accustomed to speak.

The point at issue depends to a very large extent upon the position taken with reference to emendations. If it is permitted to emend names, the view supported by Blanchard and others must necessarily prevail. The supporters of the other view, however, consider emendation as incompatible with permanency, and if their view regarding emendation is accepted, their contention regarding homonyms must be accepted as a logical correlative.

To helminthologists, the point at issue is of extreme importance, and has recently been touched upon in a paper by Lühe (1899, p. 524—539). Lühe takes the stand that the generic name *Proteocephalus*[1]) Weinland, 1858, and the family name *Proteocephala*[2]) Blainville, 1828, are homonyms; also that *Tetracotylus* Monticelli, 1892, and *Tetracotyle* Filippi, 1854, come under the same category. Two points are here involved: 1) Can the masculine, feminine, and neuter, exist side by side as separate names? 2) Should generic and family names, if identical, be considered under the rule of homonyms? Lühe answers the first question in the negative; the second in the affirmative. My own view is directly opposite in both cases. As for the masculine, feminine, and neuter names, I fail to find any justification for rejecting one in case another already exists. In English we use the words Count and Countess, Prince and Princess; in German: König and Königin, Kaiser and Kaiserin, etc.; in French: Cousin and Cousine, Lapon and Laponne; in Latin: Fundanius and Fundania, etc.

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1) Weinland, 1858, designated *Taenia ambigua* as type species.

2) Braun, 1900, p. 1675, has followed Lühe, and has rejected *Proteocephalus*, 1858, because of the family name *Proteocephala*, 1828.

No serious inconvenience appears to arise from the use of these terms in the various languages, and it is not clear that the similar use of different genders would cause trouble in zoology. Names are either the same or not the same. If the same, they are homonyms and only one is available; if not the same they are not homonyms, and both (ceteris paribus) are available. Until it is shown that the masculine, feminine, and neuter names are the same (identical), I fail to see why they are homonyms, and not wishing to complicate nomenclature by restrictions which have not yet been shown to be necessary, I would unquestionably favor the admission of *Tetracotylus* and *Tetracotyle*, or even the imaginary case cited by LÜHE: *Bothriocephalus, Bothriocephala*, and *Bothriocephalum.* These are all different combinations of letters, hence different names (not identical), hence all admissible.

Again, to consider masculine, feminine, and neuter generic names as homonyms results in increasing the chances of unnecessarily changing specific names. Suppose, for instance, we have three generic names *X-us* 1820, *X-a* 1825, and *X-um* 1830, with the following species:

X-us albus, 1840 *X-a alba*, 1850 *X-um album*, 1860
X-us niger, 1850 *X-a nigra*, 1860 *X-um nigrum*, 1870

Let it be assumed that all six species are valid. If, now, it is decided with BLANCHARD, JENTINK, and the ornithologists, that *X-us*, *X-a*, and *X-um* are homonyms, not only must the generic names, *X-a* and *X-um* be changed, but also the four names *X-a alba*, *X-um album*, *X-a nigra*, *X-um nigrum.* Thus it is within the limits of possibility that six names are changed at one stroke while we are considering only three genera and six species.

On the other hand, if the genders are recognized as forming distinct names, all of the specific names above given would (ceteris paribus) be valid. This hypothetical case is an extreme one, but it represents a possibility. There can be no question but what many specific names will be endangered if we follow the ornithologists. Why should we take this unnecessary risk?

It would not be pertinent to the case to reply that great confusion would arise if we attempted now to recognize *Distoma* and *Distomum* in worms as two separate genera. Had they originally been proposed as separate genera, there is no reason to assume that difficulties would have arisen. *Distoma* is due to unjustifiably changing the name of *Fasciola*; *Distomum* is due to the pernicious system of alleged emendation. If emendation is permitted, naturally the entire argument in favor of recognizing the three genders as three separate words falls, as does also the letter of the law of priority itself.

Taking this position defines my view regarding *Proteocephalus* and *Proteocephala* without further argument. The question, however, arises: suppose both had been *Proteocephala*? Could a generic name *Proteocephala*, 1858, be used when a family name *Proteocephala*, 1828, existed? To decide this case, which involves the question whether identical family and generic names come under the rule of homonyms, we must first consider precedent and then see whether any good reasons have been advanced to justify us in not following the established usage. Consulting the codes of nomenclature, we find that none of them maintain that the law of priority is to be strictly applied to any group higher than genera; also that nearly all of them distinctly provide that a generic name cannot be used in case the same name has been used for some other genus or subgenus (family is not mentioned) in the same kingdom. We fail to find any code which excludes the use of a word as a generic name in case the same word has previously been used to designate a higher group. The A. O. U. Committee, in fact, distinctly states in its report: "Generic names not to be invalidated by use of same name for a higher group." This same principle applies to species. By almost general consent, the use of a given combination of letters as a specific name does not invalidate the use of the same combination as a generic name (*Trutta trutta*).

Not finding sufficient precedent or sufficient reason for applying the rule of homonyms reciprocally to generic names and those of higher groups (note that family names are not used in combination with specific names), it is necessary to differ with LÜHE and BRAUN in this matter and to accept the precedent of the majority. Furthermore, I fail to find any necessity for attempting to overturn the current usage or any arguments which would suffice to do so.

8. An apparent exception to the law of priority.

The attention of helminthologists may well be directed to an apparent exception to the lex prioritatis made in their favor, and severely criticised, by zoologists of other groups, as an inconsistency. Reference is made to VII, § 3 b, of the German and English editions of the report of the International Commission, § 35 of the French edition, which reads:

„VII. § 3 b. When the larva is named before the adult:

„(Exception is made at least at for the present, for the cestodes, trematodes, nematodes, acanthocephali, acarines, in a word, for animals which submit to a metamorphosis and change of host; otherwise, many of those would have to submit to a nomenclatural revision, which would be only temporary in character and lead to deep confusion, the final result and extent of which it is now impossible to foresee.)"

This concession made in our favor can be looked upon only as temporary. The claim of other zoologists that a permanent concession of this kind is too dangerous a precedent to establish is perfectly justified. The time will unquestionably come when we shall be obliged to take larval forms into consideration in nomenclatural propositions, and on this account we should hold this point constantly in mind. It is, however, not wise to reject the workings of this temporary concession for years to come, and when we do finally yield the point, rulings should be made only on basis of experimental infections. The future may, however, be anticipated in many cases. For instance, let us assume that a generic name *Urogonimus* is suddenly discovered which antedates *Urogonimus* MONTICELLI, 1888; the latter would then fall as a homonym. In this instance, it would be much better to immediately substitute *Leucochloridium* rather than to propose a new generic name. Likewise, let us assume that by some unforeseen combination of circumstances the specific name of *Taenia marginata*, 1782, becomes invalidated; in this event it would be advisable to immediately use the name *Taenia hydatoidea*, 1760 — a name which some day will probably compel recognition.

In fact, it is a very serious question whether we should not immediately adopt *Leucochloridium* instead of *Urogonimus*, and the only reason why I am not inclined to do so at present is that it sets an example, which if followed in cases where the life history has not been demonstrated[1]), would produce an endless confusion.

The above instances will show how exceedingly complicated the subject of nomenclature is in helminthology, and this complication is an additional argument in favor of our being conservative. The outlook, however, that a time will eventually come when zoology, with its millions of species, will have a rational system of scientific names,

1) According to LOOSS and BRAUN, I have recently committed this error in connection with *Clinostomum gracile*.

must be taken as a comfort, and when we are inclined to blame fortune that the irritating changes involved fall to a great extent to our generation, we can comfort ourselves with the thought that our inconveniences — exasperating as they often are (few helminthologists appreciate this more keenly than I do, dealing daily, as I do, with a card catalogue of at least 300 000 entries!), will aid future generations in more easily dealing with the increased number of genera and species which will fall to their lot. And when we feel as if we wished to rebel against that "fetich", the lex prioritatis — as we all occasionally do feel — let us not forget that our generation is not the only one to which zoology and a stable system of zoological names are of interest. We should, on the other hand, recall that to refuse to undergo the inconveniences, to which we are occasionally submitted, is to confess a lack of altruism which unfits a man for the scientific professions.

9. Unavailable, available, and valid names.

Any genus or species may have an unlimited number of available and unavailable names, but it can have only one valid name at a given time; while the valid name may under given circumstances become unavailable and hence invalid. It is essential that we should have a clear idea of the three classes of names and should use only the valid name, where this is clear; for the careless use of an unavailable or an available (yet not valid) name of one species may frequently necessitate the rejection of the name which is valid for another form. These names may best be understood, if we take an example, first of a generic name, then of a specific name. Given the following table of synonymy:

1808. *Hemiurus* Rud. (type: *appendiculatus*) [nec *Hemiurus* 1855; nec *Hemiura* Ridgway, 1887]. Present valid name by lex prioritatis.

1809. *Distoma* (*Apoblema*) Duj. *Distoma* (type: *hepaticum*) is unavailable in this genus because *hepaticum* is not congeneric with *appendiculatus*, but is congeneric with *Fasciola*, 1758, which antedates it. *Apoblema* (type: *appendiculata*) is available at any time, and if *Hemiurus*, 1808, should be shown to be unavailable by the rule of homonyms, *Apoblema* would become valid.

1886. *Eurycoelum* Brock (type: *sluiteri*) is unavailable according to Blanchard, Jentink, and others, because of *Eurycoelus*, 1848; it is available according to Jordan, Evermann, Stiles, and others, and would become valid, 1) if both *Hemiurus* and *Apoblema* should prove unavailable by the rule of homonyms, or 2) if *Hemiurus* should be subdivided, with *appendiculatus* in one genus and *sluiteri* in the other. Further, if *Hemiurus* were divided into two subgenera, and *sluiteri* was not in the same subgenus as *appendiculatus*, *Hemiurus* (*Hemiurus*) would be the first available and hence valid name of the subgenus containing *appendiculatus*; while *Hemiurus* (*Eurycoelum*) would be the first available, hence valid name of the subgenus containing *sluiteri*.

The following example illustrates the same principles applied to specific names, and at the same time shows the effect of the careless use of name.

1899. *Haematoloechus similis* Looss. Not available.
1899. *Dist. simile* Looss, p. 602. Unavailable because of *Dist. simile* Sonsino, 1890.
1899. *Haematoloechus similis* Looss, p. 602. Unavailable because of the still-born homonym *Dist. simile.* A new specific name should be proposed.

This is one of the best illustrations I have seen for some time of the necessity of a careful study of and attention to names. Looss proposed the binomial *Haem. similis* for a new species which he had separated from *H. variegatus.* The name *similis* would have been available (and in this case valid) for his form, had he not mentioned that he had sometime before used — but not published — the name *Dist. simile* for this new worm. By citing in this place the useless combination *Dist. simile* for this species, Looss brought into the world a still-born name, since it is homonym of *Dist. simile* 1890, and, thereby, invalidated and rendered unavailable the name *Haem. similis.*

Unavailable names are usually either homonyms or cases of misdetermination. The above cases illustrate the unavailable homonyms; the following example illustrates a name which is unavailable because of misdermination:

I. *Cittotaenia pectinata* (Goeze, 1782). Present adopted name.

1781. *Taenia acutissima* Pallas. Two opinions may be advanced as to whether this name is available. Goeze evidently considered it identical with his form *T. pectinata*; according to the views expressed on page 167, it would therefore be available unless some one can show that *acutissima* is not identical with *pectinata.* On the other hand, by the A. O. U. Code (Cannon XLV:

Absolute identification is requisite in order to displace a modern current name by an older obscure one) it is not available.

1782. *Taenia pectinata* GOEZE. Available name. Is valid in case *acutissima* is not considered available, see above.

II. *Andrya rhopalocephala* (RIEHM, 1881). Valid name.

1800. *Alyselminthus* "*pectinatus* (GOEZE, 1782)" of ZEDER. Misdetermination, not *T. pectinata*. Hence not available.

1881. *T. rhopalocephala* RIEHM; *rhopalocephala* is available.

1893. *Andrya pectinata* ([GOEZE, 1782] of ZEDER, 1800) RAILLIET; *pectinata* is not available because it was a misdetermination.

RAILLIET adopted the name *Andrya pectinata*, using *pectinata* as if ZEDER had proposed it. In this ruling RAILLIET fell into error. The Int. Code (V, 3) distinctly excludes the use of *pectinata* in this event. The reason of this will be immediately clear, when we recall, that even if *A. pectinatus* ZEDER, 1800, is considered a distinct name it would be a homonym of *T. pectinata* GOEZE, 1782, since *Alyselminthus* is a synonym of *Taenia*.

10. The advantage of reverting to the oldest available name.

A number of helminthologists, and more particularly LOOSS and LÜHE, apparently do not fully appreciate the advantage of adopting the oldest name which can legitimately be given to a genus or species. Such advantage will however be clear when we recall that the older a name is, the less competition it has for supremacy, and conversely, competition increases in proportion to the number of years a name is removed from 1758. Thus, HASSALL and I dated *Sphaerostoma* 1809; LOOSS dated it 1899. If we acknowledge the date 1809, the name competes with the names (including synonyms) published for only about 50 years; if dated 1899, it must compete with the names published during 140 years. The chances of its remaining available in the latter case would therefore be enormously less than in the former. The same point arises in connection with *Brachycoelium*.

11. The type of a new genus which contains the type of an old genus.

It quite frequently occurs that an author proposes a new genus, not designating a type, but enumerating among the species which he includes in his newly proposed group a form which is the type of a preexisting genus. Such cases have given rise to no little confusion.

The only code which seems to have dealt specifically with this class of cases is the B. A. Code §§ 4, 6, 7. According to these paragraphs, if a genus for which the author proposed no type, contains at the time of its proposal the type species of some preexisting genus, that species becomes by virtue of its publication in the original list of species, the type species of the genus in question. Thus: *Planaria* GOEZE, 1782 [nec MÜLLER, 1776] was proposed without a type species; it contained *hepatica* (in *latiuscula*) which had already become the type of *Fasciola*, 1758; *hepatica* is, according to the B. A. rules, type of *Planaria*, 1782.

This ruling is a very just one and should be followed in all cases.

12. The name of the typical subgenus.

Various rules have been proposed for naming the typical subgenus. Some authors prefix an "Eu" to the name of the genus, others give a new name. It has been quite generally overlooked, that no extra rule is necessary, since the lex prioritatis fully covers the case; it was thought best to formulate the principle in the International Code.

By the lex, the name of any genus or any subgenus is the oldest generic or subgeneric name available for the group in question. Take for instance *Taenia* (type: *solium*). The oldest generic or subgeneric name available for *solium* is *Taenia*, hence (lex prioritatis) that is the generic name; by the same rule, it is naturally the subgeneric name. Any other ruling would be contrary to the lex.

13. The name of the typical subspecies.

Exactly the same principle mentioned for the subgenus applies to the subspecies, hence the last sentence of III, 3 of the code, being contrary to the lex prioritatis, is inoperative. To illustrate with an example: Genus *X*, species *a*. The name of the typical subspecies is: *X a a*; other subspecies are *X a b*, *X a c*, *X a d*, etc.

Some authors select the name *typica* for the typical species and subspecies, but such selection is calculated to give rise to great confusion; unless the specific name is *typica*, the subspecific name *typica* is inadmissible. Thus, *X a typica* is inadmissible (lex) as a subspecific name of the typical subspecies.

To show how confusion arises by using the name *typica* as a specific name, we have but to imagine the transfer of the species to another genus, or the suppression of its generic name. Take the hypothetical case: *X*, 1850 genus, with *alba* as type; *Y*, 1875 genus, with *typica* as type. Assume species *alba* and *typica* to be congeneric, *Y* drops into generic synonymy leaving the combinations:

X alba (*alba* original type,

X typica (but *typica* is not type of *X*).

An actual example of this combination of circumstances is found in *Conocephalus typicus*, now *Ascaris typica*, yet *A. lumbricoides* is type of *Ascaris*.

The use of the word *typica* in nomenclature is always calculated to lead into error and confusion, and hence should be strenuously avoided, but when once introduced it is, of course, subject to the same rulings as other names.

14. Is there any disgrace connected with synonyms?

This may seem a very strange question to ask, yet it is not infrequent that we read very sarcastic remarks in articles with reference to names which eventually fall into synonymy, their proposers being referred to in a manner which would lead one to believe that they had committed a crime and were in disgrace. They are contemptuously referred to as "species manufacturers," etc. While it is to be regretted that scientific men are occasionally so immoderate in their reference to work published by their colleagues, and while such language usually impresses the reader with a greater respect for the person attacked than for the attacking author, it is not illegitimate to discuss the point here.

Evermann, in a recent scientific discussion in the Washington Biologic Society, very aptly remarked that, from a practical standpoint, genera and species are nothing more nor less than "pigeon holes" for the classification of our knowledge regarding given animals and plants; to-day our knowledge of any given form may lead us to give it a separate "pigeon hole"; to-morrow additional information may lead us to unite two "pigeon holes" and as a result one name falls as a synonym. Viewed from this standpoint, synonyms are a natural result of our increased knowledge, hence are natural accompaniments of the evolution in the classification of any group.

There can be no doubt regarding the validity of the position of this eminent ichthyologist on this point, and it would be well for us helminthologists to bear his remarks in mind. Evermann did not, however, refer to the wanton, unnecessary, and unjustifiable changes of names of which some authors have been guilty — whereby recognizing the identity of the forms they described, with genera and species previously described by earlier authors, new and unnecessary names have been introduced into science. LINNAEUS and RUDOLPHI are two of the prominent offenders in this respect. The introduction of new names is always to be avoided, when such name is not justified with reference to preexisting names but the fact that an author has proposed a name which later falls as a synonym or perhaps as a homonym should not be judged too harshly; for many circumstances, perhaps not all of which are known to the reviser or reviewer, come into consideration. Several of the names recently proposed (December 30, 1899) by LOOSS fall, because BRAUN (Dec. 7, 1899) and LÜHE (Dec. 29, 1899) proposed names covering the same genera. For any one to critisize LOOSS for these synonyms would be unreasonable. The exercise of more good faith, when judging others' writings would enhance the value of the publications of some authors.

15. The dates of RUDOLPHI's species and genera.

There is a decided lack of uniformity among authors in quoting the dates of RUDOLPHI's genera and species. Many of his 1801—3 species are quoted as 1810, while all of his 1814 species are usually quoted as 1819. Such lack of uniformity is unfortunately calculated to produce confusion, and sets an example which is not free from criticism.

If a species was originally published in 1803 or 1814, why give it the date 1810 or 1819 and thus increase the chances of its rejection by the rule of homonyms? A species or genus should be given its correct date, not an artificial one. This applies to RUDOLPHI's genera also. BRAUN[1] (1900, p. 1660) for instance prefers to adopt 1810 as date of *Bothriocephalus* rather than 1808.

Such a ruling, however, is contrary to evidence, hence it cannot be admitted. The one reference to *Bothriocephalus* on p. 111, RUDOLPHI, 1808, is sufficient to hold the generic name to that date even if RUDOLPHI had not made anatomical references to the genus in other parts of his 1808 volume. The motive which leads BRAUN to his discussion is that it was not until 1810 that RUDOLPHI gave a diagnosis for *Bothriocephalus*. Such a view, however, makes a dangerous pre-

1) "Eine Diagnose giebt RUDOLPHI aber erst im zweiten, 1810 erschienenen Theil des zweiten Bandes desselben Werkes und damit erscheint mir die Bedeutung von *Bothriocephalus* erst festgelegt."

cedent. For instance, Looss published in 1896 in connection with certain species, the three generic names *Lecithodendrium*, *Prosthometra*, and *Pleuronectes*. He did not give a generic diagnosis of *Lecithodendrium* and *Pleuronectes* until his 1899 paper, and has not yet done so for his *Prosthometra*, yet if Braun adopts 1810 for *Bothriocephalus*, consistency should lead him to adopt 1899, instead of 1896, for *Lecithodendrium* and *Pleuronectes*. Likewise he could rule that his own genera of 1899: *Paragonimus*, *Phyllodistomum*, and *Harmostomum*, and Lühe's genera of 1899: *Telorchis*, *Prosthogonimus*, etc., were not published on those dates, for it is difficult to see a sharp distinction between the cases. True, types were proposed for Braun's and for all but one of Lühe's genera, but not for Looss' genera and for one of Lühe's genera.

Quite aside from the desirability of giving a generic or specific name the earliest possible date permissible, in order to decrease the chances of its being rejected under the rule of homonyms, it is exceedingly dangerous to adopt a precedent such as Braun has tried to establish in connection with *Bothriocephalus*.

16. Method of proposing a new genus or species.

Having been obliged to perform so much bibliographic and indexing work during the past ten years, a great impression has been made upon me relative to the different methods followed by authors in their publications; and in the hope that these observations may be of use — to students at least if not to older workers — some of the more important may be reviewed here. The writer feels confident that his colleagues, especially those whose names are mentioned, will accept these suggestions in the same spirit in which they are presented, — namely as an effort to eliminate so far as possible, those features of helminthological writings which have a constant tendency to render our nomenclature unstable. As a matter of fact, here lies the root of half the evil in errors and changes of nomenclature.

The more cautious author will conform as nearly as possible to the following:

I. General remarks regarding the article.

1) The title of an article should first of all be descriptive; secondly, as short as possible. The latter point should, however, be

sacrificed to the former. LOOSS' (1899) title [1]) may be objected to by some parties as being too long, but the objection can not hold as justified, for the title is descriptive. Although there are seventeen words in this title, a critical study will show that not one superfluous word is used. Not only helminthologists, but zoologists in general and even the laity will immediately comprehend it. It is a model which all authors would do well to follow. Titles of this nature are to be found particularly in the Proc. U. S. National Museum.

Compare, now, LOOSS' title with the following title of an article by BRAUN (1899): "Ueber Clinostomum LEIDY". This latter conveys a certain amount of information to helminthologists (although a more explicit one would have been better even for us); but it is relatively unintelligible to the general zoologist, and necessitates his finding out whether *Clinostomum* LEIDY is a fish or a worm. "A short systematic revision of the trematode genus *Clinostomum*, with proposition of three new genera" would have given a much more exact idea of what we find in BRAUN's valuable paper.

All of us have sinned more or less in selecting the titles for our articles, but it is not too late to reform.

2) Methods of writing an author's name. It may seem an insignificant matter whether an author writes his name in full or in an unduly abbreviated form, yet from a bibliographic standpoint this is important. In order to arrange articles and books properly in a catalogue or library, it is essential that the works of one writer should not be placed under the name of another man. Still it not infrequently happens that confusion results because of the methods adopted by authors, hence it is not unreasonable to advance the point that more care in this regard will aid in preventing confusion.

The most objectionable custom, in this regard, known to me is followed by certain German authors who give simply their family name and residence. Thus, one finds articles in some journals written by "SCHMIDT-Berlin" or "SCHULZE-Wien". Now let us suppose that these men change their residence and become "SCHMIDT-Leipzig" and "SCHULZE-Hamburg". The confusion to the bibliographers is by no means insignificant.

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1) Weitere Beiträge zur Kenntniss der Trematodenfauna Aegyptens, zugleich Versuch einer natürlichen Gliederung des Genus Distomum RETZIUS.

In order to aid the bibliographer in every way to clearly distinguish the authors, it is well always to give the more or less complete name; thus, to take illustrations: "RICHARD HEYMONS, Assistant, Zoological Institute, Berlin, Germany" is better than "R. HEYMONS, Berlin"; "MAX BRAUN, Professor of Zoology, Königsberg i. Pr." is better than "M. BRAUN, Königsberg i. Pr."; "RAPHAEL BLANCHARD, Professor of Natural History, Paris Medical School" is better than "R. BLANCHARD, Paris", etc.

3) If reprints are to be distributed, the author should impress it upon the publisher that a) the original pagination should be retained; b) the printed matter should not be shifted; c) the reprint should also bear the name, volume, number, and the date of publication of the journal.

Attention may be called to the reprints now issued by the Zool. Anzeiger and the Centralbl. f. Bakter., Parasitenkunde u. Infektionskrankheiten, which may be taken as models. The custom followed by many medical journals — particularly American — is not to be commended; the printed matter is shifted and the article is repaged from 1—x, with no indication of the original pagination.

4) If a new genus or a new species is proposed, the fact should be brought out clearly by the use of prominent type as a heading or subheading. LOOSS' article may be consulted as a model. Proposing a genus in the text (and even then not adopting it himself), as LOOSS did in 1896 (*Lecithodendrium*, *Pleurogenes*, and *Prosthometra*), or as he has done in 1899 (*Anadasmus*), or as BRAUN, 1899 (*Paragonimus*, *Phyllodistomum*, *Harmostomum*), and LÜHE, 1899 (*Telorchis*, *Prosthogonimus*, etc.), and others have recently done, is calculated to lead readers to overlook them and thus to lead to later confusion, hence this method should be strenuously avoided.

There can not be the slightest doubt but that scores of names so proposed have for the time being been overlooked, and later, on being suddenly discovered, have resulted in unfortunate changes in nomenclature; take *Hemiurus* RUDOLPHI, for instance.

Some authors follow the very commendable plan of giving a complete index to all the scientific names in their article, and placing the new generic and specific names in bold type. Other authors follow an excellent plan of giving a list of the new genera and species mentioned, and this latter plan is adopted by some societies in their proceedings (cf. Soc. Zool. France); The Washington Biological Society adopts the former plan.

Three prominent helminthological publications, in particular, would be much more convenient for consultation if either of these plans had been adopted; reference is made to DUJARDIN (1845), LOOSS (1899), and BRAUN's Vermes.

II. In proposing a new genus.

1) Use bold type, as mentioned above, for heading or subheading.

2) Having selected a name, consult SCUDDER's (1882) Nomenclator Zoologicus, and the index of the Zoological Record, from 1880 to date), in order to see whether or not the name selected is available or preoccupied. Hundreds of changes of names could be avoided by following this very simple plan. It is of course to be regretted that neither of these publications is absolutely complete, but they are of great value, nevertheless.

Beyond a doubt, many authors will raise the point that neither of these works is accessible to them. Both are, however, to be found in nearly all important scientific libraries, and are surely accessible to some one friend of every author.

3) Select a species as type of the genus and clearly state so in the article. Compare discussion under 5.

4) Give a clear, condensed diagnosis, showing the essential characters.

5) Give a differential diagnosis, showing the characters by which the new genus differs from its closest relatives. An analytical key is best for this purpose.

Regarding 3 and 4, it may be remarked that opinion differs among authors as to whether the type or the diagnosis is the more important. LOOSS evidently looks upon the diagnosis as the more important, and in taking this view he sides with many systematists of undoubted ability.

While much is to be said in favor of this view, the latter cannot by any means be admitted as self evident. In this connection it may be noted: a) When a type species has been published for a generic name, this is practically the only definite, unchangeable, and absolutely objective point connected with the whole matter. b) Since the limits of a genus are to no little extent subjective, the diagnosis must necessarily be subjective, in the same degree. c) In trying to evolve a natural classification, the characters selected as of generic value are subject to the existing state of general knowledge regarding the group, to the state of knowledge of the author who writes upon it, to the influence exercised upon him by his study of the value of characters in other groups, to existing necessities of a practical nature — such as technique of the subject, and to other factors.

Taking all these points into consideration, while I do not underestimate the convenience of the diagnosis, it does not appear unreasonable to say that it is more of a convenience than a necessity, while the type is both a convenience and a necessity.

I am unable, therefore, to fully agree with Looss in the view he implies, perhaps, more than expresses, a view certainly definitely expressed by many authors, that the diagnosis is more important than the type — a view, further, which admits that something ephemeral is more important than something permanent. I find it necessary, on the contrary, to give the type the first place of importance in connection with a generic name, and I should much prefer to deal with a large number of generic names established on known species as types, than a large number of names established only on diagnosis, or on long anatomical and histological descriptions not reduced to diagnoses. This should not be construed as meaning that I undervalue the convenience of a diagnosis; quite on the contrary, it should always be given in proposing a new genus; but we must not forget that every published diagnosis is to a large degree, subjective, hence ephemeral, while every published type is absolutely objective, hence permanent.

While not losing sight of the abstract consideration involved, authors may unite upon a practical compromise by considering the type and diagnosis as coordinate.

III. In proposing a new specific name.

1) Give a diagnosis, both specific and differential, or refer clearly to the name for which it is substituted.

2) Having selected a specific name, look up all the specific names, valid, available, and unavailable, already proposed for the species be longing to the genus in question, and also the specific names used in combination with homonyms of the generic name in question.

If, for instance, we have a genus *X-us* 1840 in Trematodes, with a homonym *X-us* 1850 in another group, as birds, and there exists a bird with the name *X-us albus*, the specific name *albus* is not available as a new specific name in the Trematode genus *X-us*.

3) State in connection with the diagnosis where the type, cotypes,

paratypes, or autotypes have been deposited, so that other authors may know where they may be consulted.

I shall be only too willing to assume the responsibility of caring for any types, cotypes, paratypes, or autotypes, entrusted to my care, making them a part of the United State Government collections, or of my own collection as prefered by the donator, and with the distinct understanding that all such specimens shall be subject, under proper conditions, to consultation by any proper person.

17. Are patronymic names to be censured?

Looss (1899, p. 597) enters a respectful but unequivocal protest against naming animals (in this case parasites) after persons. Since this question frequently arises it may be well to examine it. Looss says: "Noch in einer andern Hinsicht berühren die letzten Mittheilungen Mühling's (Studien, etc., und Helminthenfauna, etc.) ausserordentlich wohlthuend. Es sind in denselben 7 neue Arten beschrieben und nicht eine einzige davon ist nach einer Person benannt! Bereits Rudolphi hat die Anwendung von Eigennamen zur Bezeichnung von Eingeweidewürmern als einen nicht empfehlenswerthen Brauch bezeichnet; die ältern Autoren (Dujardin, Diesing, etc.) haben ihn auch nicht angenommen, da der unerschöpfliche Reichthum der classischen Sprachen ihnen Material genug für die Bildung neuer Genus- und Speciesnamen bot. Seit Cobbold aber ist die Benennung von Helminthen mit Personennamen ('zu Ehren' derselben!) geradezu Mode geworden, und man kann es heute gelegentlich erleben, dass ein Autor, der einen Cestoden, einen Trematoden und einen Nematoden neu beschreibt, 2 davon oder alle 3 zu Ehren ihres Entdeckers mit demselben Eigennamen benennt. Ist denn der Wortschatz der classischen Sprachen schon so ganz erschöpft?"

It is interesting to me personally to notice how similar Looss' views on nomenclature agree with those I held before taking up this branch of zoology for careful study. I once held the same ideas as Looss relative to *Sphaerostoma*, the face value of a diagnosis, Rudolphi as starting point for nomenclature of parasites, etc. Once I even went so far as to write a very animated article on the subject of patronymics, expressing the same views recently expressed by Looss, though I fear I used less moderate language. Now Looss asks whether the word-treasure of the classical languages has already been so entirely exhausted. It is not necessary to make a mathematical comparison of the number of generic names known with the number of permutations and combinations of the words in the Latin and Greek lexicons; let us rather take a practical example.

In Looss' paper (1899) he proposed sixty-one generic names for new genera which he recognizes. Of these, ten names (namely: *Astia*, *Anadasmus*, *Baris*, *Creadium*, *Enodia*, *Leptalea*, *Megacetes*, *Microscapha*, *Polysarcus*, and *Stomylus*) or about sixteen and four-tenths per cent, are absolutely identical with names proposed in zoology for other genera, and must be changed or dropped; eight other names, or thirteen and one-tenth per cent (namely: *Acanthostomum*, *Dolichosomum*, *Haematoloechus*, *Lepoderma*, *Liopyge*, *Progonus*, *Psilostomum*, and *Stephanostomum*) differ from zoological names proposed by other authors in the ending and should according to many nomenclaturists be rejected. Thus with the alleged inexhaustible supply of classical names at his disposal from which to select, a total of eighteen names, or twenty-nine and five-tenths per cent, of the generic names proposed by Looss were already more or less exhausted. Other authors have had similar experiences. It may be recalled that Looss during his entire scientific career has thus far proposed less than one hundred generic names, and if we recall that ten of these are unquestionably still-born homonyms, and that eight names are doubtful homonyms, we can imagine the difficulties under which some of our other colleagues labor, notably entomologists like Ashmead, whose generic names run into the hundreds.

That many men should complain of the difficulty of finding available names is not to be considered strange, and when we consider that zoologists have scarcely commenced to name the living and extinct genera and species, it is not unreasonable for us to take refuge now and then, in all sorts of devices, such as patronymics, barbarous names, transpositions, arbitrary combinations of letters, etc., in order to find a nomen which stands in less danger of being suppressed as a still-born homonym. I am not especially devoted to patronymics, still I fail to appreciate the grounds for arguing against them. Our first consideration in nomenclature should be stability; all other considerations are secondary.

The most that we can demand of a name is that it shall be a pronounceable combination of letters, in Latin form. Now let us compare *Athesmia* and *Brandesia*, names of two genera discussed by Looss. Wherein is the combination of letters A-t-h-e-s-m-i-a better than the combination B-r-a-n-d-e-s-i-a? The fact that the former happens to be derived from a Greek word meaning lawless, the latter from a German word which happens to be the family name of one of our esteemed German colleagues, is absolutely irrelevant to the questions at hand. These combinations have no inherent meaning except as applied to objects, and as applied to one genus the combination B-r-a-n-d-e-s-i-a is just as satisfactory as the combination A-t-h-e-s-m-i-a applied to the other. That it is any way disrespectful to BRANDES, that STOSSICH named a genus of parasites after him, cannot be admitted, and if one prefers not to consider it a compliment to BRANDES, there is another way to look at it — namely as a compliment to the worm! But as stated, all such questions are secondary and insignificant compared with the matter of stability, and from this standpoint it would have been better if LOOSS had constructed generic names for the ten still-born homonyms mentioned above, out of the names of ten of his Egyptian colleagues, friends, or servants, and thus avoided the necessity of a later change of names. LOOSS states that he is greatly handicapped for literature in Egypt. This condition can be fully appreciated, and in my opinion would be ample justification for his resorting to all sorts of expedients in order to render his generic names capable of being adopted.

18. The status of generic names proposed in the manner of *Lyperosomum*.

Occasionally authors publish names in a manner which may better be avoided, since it is calculated to lead to confusion. As an example par excellence of this kind, attention may be directed to *Lyperosomum* LOOSS, 1899, p. 635. LOOSS says:

"Sollte es sich im Laufe der Zeit herausstellen, dass zwischen beiden Kategorien noch andere und constante, wenn auch kleine Differenzen existiren, dann dürfte es sich wohl empfehlen, die oben von BRAUN namhaft gemachten Formen, zu denen auch *Dicr. strigosum* gehört, in eine eigene Gattung [vielleicht *Lyperosomum* mit Namen] zu stellen," etc.

This case is not quite so bad in one respect (yet still more regretable in another) as a specific name published not long ago: An author mentioned that Mr. X had collected certain animals, among which was a specimen of a monkey. He (the author) had not yet determined to what genus the monkey belonged, but in case it proved to be a new species, he would suggest that the specific name *y*— be applied to it.

Lyperosomum is not so bad as this case, because the species and ordinarily recognized generic relationships are known. It is, however, open to far more serious censure than the case of the monkey, for the specific name of the latter may be absolutely ignored on the ground that no tangible clue is given to the characters. With *Lyperosomum*, however, a difference of opinion may arise. The wording Looss has used is ambiguous. It would enable him to claim (were he so disposed) that he has proposed the genus *Lyperosomum*, in case this should prove to be a valid genus; but it is equally possible for him or any one else to claim that he did not propose it, in case it should turn out to be invalid. The question arises: What is the status of *Lyperosomum*?

Both Looss and I have already committed ourselves on cases of this nature, in connection with *Sphaerostoma*. Looss maintains that Rudolphi simply mentioned *Sphaerostoma* incidentally and should not be held responsible for it. I maintain that Rudolphi should be held responsible for the genus. I also maintain that *Lyperosomum* and *Anadasmus* must be attributed to Looss, 1899. Any other ruling upon these cases leaves open the door for the widest difference of opinion in numerous cases. When a name is published, with a tangible indication to it, that name must be recognized and its author held responsible for it. There is a tangible indication to both *Lyperosomum* and *Anadasmus* as there is to *Sphaerostoma*, hence Looss must be held responsible for the former as Rudolphi is for the latter.

It is certainly a matter to be regretted that my esteemed friend and colleague Looss, or any one else, should follow such a custom as he has done in these two cases. If he does not think the time is ripe to recognize the groups as genera or subgenera, it would be better for him to content himself with indicating their relationships, and leave the future to decide what name should be proposed. Looss' action in these two cases is only explainable by recalling his admission on p. 523, namely, that he is not in a position to judge the difficulties which have arisen in nomenclature.

In view, however, of his high standing as the greatest authority on the anatomy of the *Fasciolidae* who has ever lived, I would submit in a most respectful and friendly spirit, yet at the same time, in the most positive manner possible, that it is his duty to inform himself upon these difficulties before lending the example and weight of his authority in support of nomenclatural propositions, rulings, and customs which are calculated to increase confusion in the chaos of names into which zoologists in different specialities are endeavoring to introduce some order by the application of general principles, based upon the careful study of thousands of cases by men in different groups.

19. The case of *Sphaerostoma* Rudolphi, 1809.

In 1898, Hassall and I published an inventory[1]) of the names of genera thus far proposed for the *Fasciolidae.* In that list we called attention to the fact that the genus *Sphaerostoma*, proposed by Rudolphi in 1809, had been universally overlooked.

My friend Looss has objected very seriously to the acceptance of *Sphaerostoma* and has criticised us for — as he described it — taking refuge in conjecture, in reference to this name. To any one who has studied carefully the theory and practice of nomenclature, Looss' argument will be quite clear — not as a support of his assertions regarding the genus in question, but as a practical proof of the admission he has made on p. 523, to the effect that he is not well versed in nomenclatural precedents in other fields of zoology. In view of the apparent validity of his statements, however, it may be well to examine the case more closely.

As stated in Note 48, Rudolphi proposed this genus as follows: "*Quae corpore plano, quaeque tereti utantur, genera non separanda, limites enim certi vix adsunt; sed species plurimae (potissimum in piscibus obviae) poris globosis, maximeque mobilibus, saepeque extantibus munitae, olim forsan sub Sphaerostomatis . . . nomine generi peculari reserventur.*"

Looss also quoted part of this passage, yet immediately added that there is not the slightest indication in Rudolphi as to the species which he thought should especially ("speciell") be placed here. He says that if some other authors should claim that Rudolphi referred to such forms as:

1) *Fasciola clavata* Menzies, 1791 [type of *Hirudinella*]; or

2) *F. macrostoma* Rudolphi, 1803 [type of *Urogonimus*]; he would be just as correct as we were in looking upon

3) *F. bramae* [*F. globipora*] as type of *Sphaerostoma.*

He then goes on to say that an author to-day might take *globi-*

1) Notes on parasites, 48. An inventory of the genera and subgenera of the Trematode family Fasciolidae, in: Arch. Parasitol., 1898, p. 81—99.

pora as type, and examining it, make up his mind as to which characters were more important:

4) One might lay special stress upon the suckers, and declare *Podocotyle* [type, *D. angulatum* DUJARDIN, 1845, not known to RUDOLPHI [1])] as synonym of *Sphaerostoma.*

5) Another might take the intestine as character and make *Dicrocoelium* [type *D. lanceatum* STILES et HASSALL, 1896, misdetermined by RUDOLPHI] synonymous.

Replying to LOOSS' position I would submit, in the first place, that in referring to RUDOLPHI's *Sphaerostoma* and selecting *globiporum* as type, we were not carrying out any new or revolutionary ideas, but were simply performing a duty which devolved upon us, and doing so strictly in accordance with precedents which for years have been recognized by nomenclaturists. LOOSS' criticisms are due solely to the fact that — as admitted by himself on p. 523 of his article — he is unacquainted with these customs established by precedent, hence his position can be very readily understood; while of his four suppositions of what some one else might have done, two are unallowable and two improbable.

One of the fundamental rules of nomenclature is, that a generic name once established cannot be ignored in any subsequent subdivision of the group, but must be retained — if otherwise valid — for some portion of that group containing one of the original species. The generic name *Sphaerostoma* had been printed; we considered it then — and we do to-day — published in such a way as to deserve attention, hence we felt obliged to include it in our list. It is certainly not a nomen nudum. At most, it may be objected that RUDOLPHI failed to mention directly any species in connection with it, and that he gave a poor diagnosis. One does not, however, have to take refuge in conjecture to see what RUDOLPHI referred to. To us, at least, it is clear (I cannot of course speak for LOOSS) that RUDOLPHI had certain species in mind; any one of these species may come into consideration in the selection of a type. We are not at this date compelled to take one which he had especially ("speciell") in mind, although it would be wiser to do this.

In the first place: "*potissimum in piscibus obviae*" immediately confines our attention to those fish distomes (*Hemiurus*, of course, excepted) which RUDOLPHI mentioned between p. 352 and 415.

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1) But *D. gibbosum* might have been taken.

"*Poris globosis, maximeque mobilibus, saepeque extantibus munitae*" confines the choice to those fish distomes between p. 352 and 415 which show the characters referred to by repetition of these words or their equivalents in the specific diagnosis. Any one of these forms may be taken as type, provided that it has not already been taken as type for another genus. *D. macrostomum*, which Looss suggests as a possibility, was not available since this had already been eliminated as type of *Urogonimus*. *D. clavatum* (as is now evident) was also not available, since this is type of *Hirudinella*.

In the description of *D. globiporum* (a distome of fish) we find expressions which fully agree with Rudolphi's reference to *Sphaerostoma globiporum*: "*Pori globosi apertura orbiculari, anticus exiguus, ventralis major, in junioribus prominulus, saepe sub animalculi motibus maximam partem protrusus vel prolapsus.*" Hence we are justified in selecting this species as type.

Further, earlier authors not infrequently selected a species which they raised to generic rank, taking the name of that species as generic name. In a number of cases in order to prevent tautonomy (as *Trutta trutta*), the Latin or Greek specific name was translated into Greek or Latin as a generic name, or another name of the same meaning was selected, the old specific name was made generic while the new or the specific name *vulgaris* was introduced. In such cases it is customary, whenever this is possible, to select as type of the genus, that species whose name agrees in form or meaning with the generic name (*Alces alces, Alle alle, Anhinga anhinga, Bison bison, Bos taurus, Buteo buteo, Capra hircus, Cardinalis cardinalis, Coturnix coturnix, Crex crex, Equus caballus, Glis glis, Gulo gulo, Histrionicus histrionicus, Lutra lutra, Meles meles, Ovis aries, Phocaena phocaena, Pipistrellus pipistrellus, Porzana porzana, Puffinus puffinus, Rosmarus rosmarus, Rupicapra rupicapra, Scomber scombrus, Sus scrofa* or *Sus porcus, Sula sula, Tarandus tarandus, Trutta trutta,* etc.) or one with which the specific name *vulgaris* has been used.

Rudolphi had a Latin specific name *globiporum* in the genus *Distoma*; he suggested separating from *Distoma* a genus *Sphaerostoma*; although he gave to the latter a poor diagnosis, Looss will surely admit that this applies — so far as it goes — to *globiporum*. Instead of taking a specific name (*globiporum*) and making it generic so as to give tautonomous combination, like *Trutta trutta*, he followed a not uncommon custom in zoology, especially among early authors, in translating his Latin specific name into a Greek generic name *Sphaerostoma.*

No speculation is needed to understand the case; all that is required is an examination of the diagnosis given by RUDOLPHI, and a knowledge of zoological customs and precedents; and on the basis of these there is no question in my mind but that *globiporum* should be selected as type of *Sphaerostoma.*

It may here be added that, before publishing this case in 1898, it was submitted to two of the most experienced nomenclaturists living, to see if, perchance, they would rule differently from the way I had decided. Both men agreed with me that *Sphaerostoma* was published in such a way that it could not be ignored, and that there was no question but that *globiporum* was the most natural species to select as type.

LOOSS further takes the ground that, according to the law of priority, *Sphaerostoma* RUDOLPHI should be rejected because it is not "recognizably defined or indicated". As seen from the above, I find it necessary to maintain that this genus is recognizably indicated, hence that it is subject to the law of priority. Our positions, therefore, are diametrically opposed.

A curious part of LOOSS' discussion is that he apparently does not see the enormous advantage of dating a genus 1809, when possible, instead of 1899, — thus reducing the chances of a later change of name.

20. The case of *Schisturus* RUDOLPHI 1809.

LOOSS (1899, p. 527—528) considers that we have gone too far in connection with the generic name *Schisturus*, and suggests that if RUDOLPHI, 1819, were accepted as starting point, all such early names would at once be removed from consideration as nomenclatural problems.

LOOSS has evidently misunderstood us. It was our purpose to collect all generic names which in any way came into consideration with the *Fasciolidae.* RUDOLPHI (1819, p. 425) cites *Schisturus* in the synonymy of *Distoma nigroflavum*, and this fact made it obligatory upon us to enter *Schisturus* in our list. Having found the name in this connection, it was necessary to define its status; this we did in no uncertain terms; and it is difficult to see how LOOSS can object to the ruling we made. Many authors might have been inclined to construe Rudolphi's citation in this case as being the work of "the first reviser", and as indicating that *Schisturus* — not being preceded by a mark of interrogation (Rudolphi, 1819, p. 425) — should be construed as a definitely fixed name, to be recognized as generic as soon as *nigroflavum* was taken out of the genus *Distoma*. Foreseeing such a possibility, especially on the part of younger students, we endeavored to inhibit such action until *Schisturus paradoxus* should be shown to be identical with *D. nigroflavum*. If it is ever established that such is the case, no doubt can possibly arise as to the rehabilitation of *Schisturus*, and we see no reason for retracting our words. If the identity is never established, *Schisturus* is not entitled to priority. Personally, I did not then and do not now, see any probability that this synonymy, adopted by Rudolphi, will ever be established; this does not, however, entirely relieve us of the responsibility of considering the name *Schisturus*. We did not attempt to reestablish *Schisturus* as the valid (gültiger) name for *Podocotyle*, but simply indicated it in its proper place as a doubtful synonym, warned against its rehabilitation on insufficient grounds, and indicated the necessity of holding the name in mind. The name is not a nomen nudum; it is accompanied by a diagnosis and four figures, and a type (only) species; its fate hangs on the fate of that species. It is, therefore, not entirely clear to me wherein Looss and I differ in principle in regard to this case.

21. The case of *Brachycoelium* and *Lecithodendrium*.

Looss (1899, p. 611—614) heartily disapproves of the action taken by Hassall and myself in designating *Distoma crassicolle* as type of the genus *Brachycoelium* and asks which name is valid, the insufficiently defined older name (*Brachycoelium*) or the sufficiently diagnosed younger name (*Lecithodendrium*)? He also refers (p. 647) in connection with this case, "to the inconvenience resulting from the mere designations of typical representatives for insufficiently and absolutely undetermined genera" and ends his discussion (p. 614) with the exclamation: "Therefore, care in selecting typical representatives!" From the discussion it would appear that Looss considers that we had designated *D. crassicolle* as type, without due consideration of the factors involved, and Lühe (1899) apparently takes the same view. Under these circumstances, it may be well to examine carefully the exact status of the case.

In 1845, DUJARDIN proposed under *Distoma* the subgenus *Brachycoelium* with the following diagnosis:

"Intestin divisé en deux branches courtes, renflées en massue, et précédé d'un long oesophage filiforme."

No type species was designated, but the following species were placed in *Brachycoelium.*

1. *D. heteroporum* DUJ., 1845. [Examined by DUJARDIN; type of *Pycnoporus* by LOOSS, 1899; probably a *Lecithodendrium* — LOOSS, 1896; to *Lecithodendrium* by STOSSICH, 1899, p. 9.]
2. *D. arrectum* DUJ., 1845. [Examined by DUJARDIN; to *D.* (*Dicrocoelium*) by STOSSICH, 1895; admitted by LÜHE, 1899, p. 536, to be a species inquirenda, yet selected by him as type for *Brachycoelium*; admitted by LOOSS, 1899, p. 614, to be a species inquirenda.]
3. "*D. clavigerum* RUD.", of DUJ., 1845. [Examined by DUJARDIN; admitted by LOOSS, 1894, p. 101, to be a misdetermination and renamed *D. confusum*, the latter taken as type of *Prosotocus* by LOOSS, 1899, p. 616.
4. *D. crassicolle* RUD., 1809. [= *Fasc. salamandrae* FRÖLICH, 1789, renamed; examined by DUJARDIN; erroneously placed in *D.* (*Dicrocoelium*) by STOSSICH, 1889; retained here by PARONA, 1896, pp. 13—16; returned to *Brachycoelium* by STOSSICH, 1897, p. 9; designated type of *Brachycoelium* by STILES and HASSALL, 1898, p. 83; placed in *Lecithodendrium* by STOSSICH, 1899, p. 9, and by LÜHE, 1899, p. 356.]
5. *D. retusum* DUJ., 1845. [Examined by DUJARDIN; to *D.* (*Dicrocoelium*) by —?—; admitted by LOOSS, 1899, p. 614, to be problematic.]

Here we have a subgenus, containing five species, united by a perfectly clear diagnosis, and from DUJARDIN's point of view and from the point of view of his time, forming a more or less natural group. The subgenus is defined fully as clearly as *Dicrocoelium*, *Apoblema*, *Echinostoma*, *Crossodera*, and thousands of other genera and subgenera of its time. It does not appear to be preoccupied or antedated. No grounds are apparent which would justify an author in ignoring it when studying any of the five forms mentioned, or when studying other forms which would fall under the same diagnosis. Natural or unnatural, from the standpoint of the present day, it must be admitted as entitled to recognition; and if any author later than DUJARDIN, 1845, desires to propose another genus for any one of the five species mentioned, or for any other distomes which correspond to the diagnosis given by DUJARDIN, it is incumbent upon the proposer to show wherein his new genus differs from DUJARDIN's *Brachycoelium.* And if any author subsequent to DUJARDIN, 1845, does propose a new genus which corresponds to *Brachycoelium* without showing wherein the two genera differ, it is natural and just to consider the later genus a synonym of the earlier until some one does show a difference between the two either by mentioning a character of generic importance or by redefining DUJARDIN's genus so that such differences will be brought out. It is but natural, and in accordance with the principles of systematic zoology, that in the latter event he shall designate type species for both genera; and in selecting the type for *Brachycoelium*, it is but natural that he shall notice the following facts:

1) None of the five original species bear the name *Brachycoelium* or its equivalent, as specific name;

2) It is not apparent that DUJARDIN (pp. 381—389, or pp. 402—404) had any one species in mind more than any other; although he examined all five forms;

3) DUJARDIN did not give any figures of any of the forms;

4) The diagnoses are all apparently about equally complete;

5) None of the species had ever been selected as type of any other genus.

Hence all other things being equal, any one of these five species might be selected as type. In considering the other elements which enter into the subject it may be noted:

6) The oldest species mentioned are *D. clavigerum* and *D. crassicolle*; of these, *D. clavigerum* is a misdetermination, hence ceteris paribus, *crassicolle* would appear to be less liable to lead to confusion, if taken as type, than would be *D. clavigerum.* If therefore the principle supported by some workers (namely to select the oldest species as type) were followed, *D. crassicolle* would be the type.

7) The first page on which any species is mentioned in connection with *Brachycoelium*, is p. 386, and *D. crassicolle* is that species. With all those systematists who follow strict page-precedence, *crassicolle* would on this account be selected as type.

8) Of the five species mentioned, DUJARDIN refers to figures of two: "*D. clavigerum*" [misdetermination] and *D. crassicolle.* The figures of *D. clavigerum*, it would appear best to leave out of consideration because of the misdetermination, hence *D. crassicolle* remains.

While it is not necessary to rule in favor of *D. crassicolle* because of 6, 7, or 8, still unless reasons can be advanced to show that it would be better to select some other species as type, it is clearly in the interests of harmony to select *D. crassicolle*, for not only is it the only illustrated form (up to 1845) which comes into serious consideration, but such a ruling would be in accordance with the views of that not inconsiderable class of systematists who believe in page-precedence, and also in accordance with the views of those who prefer, if possible, to select the oldest species.

Now let us inquire whether there was any reason for not selecting *D. crassicolle* as type — any reason developed by the writings of later authors.

9) The subgenus had been freely used, both directly and indirectly, by various authors, but none of these writers had designated any type species.

10) Several of the species had been referred to or discussed by various writers, but none had been eliminated as type of any new genus.

11) *D. arrectum* had been placed in *D.* (*Dicrocoelium*) by Stossich, 1895, and so far as our records go, had not been returned to *Brachycoelium*. Accordingly, there does not appear to be any reason for selecting this species, over *D. crassicolle*. This view is rendered even more justified by the fact that *D. arrectum* is problematic.

12) *D. retusum* had been placed in *D.* (*Dicrocoelium*) by — ? — and so far as our records go had not been returned to *Brachycoelium*. Accordingly, it would not appear advisable to select this species over *D. crassicolle*. This view is rendered even more justified by the fact that *D. retusum* is problematic.

It would therefore appear that both *D. arrectum* and *D. retusum* should be eliminated from competition with *D. crassicolle*. For practical reasons, also, to prevent confusion in selecting a misdetermined species, "*D. clavigerum* Rud." of Dujardin, since it might easily happen that some authors would interpret *D. clavigerum* as type, the third species of the list was eliminated. There remain now species 1 and 4, *heteroporum* and *crassicolle*.

If *heteroporum* were selected, we should have gone quite contrary to the view of three sets of nomenclaturists: those who believe in page-precedence; those who prefer to select the oldest species; those who prefer to select a type which has a definite reference to an illustration. If reasons were apparent for not following the views of these men, in this particular case, I should not have hesitated an instance in selecting *D. heteroporum* instead of *D. crassicolle*. No reason seemed apparent for our not conceding page-precedence, hence we selected *D. crassicolle* as type.

But to turn to another phase of the question: we made our selection without knowing of Looss' *Lecithodendrium.* The question therefore arises, would our selection have been different if we had known of that genus? To decide this point we must turn to Looss, 1896.

In 1896, Looss proposed a genus "auquel on pourrait peut-être reserver le nom *Lecithodendrium*" to contain the species: *D. glandulosum, D. hirsutum, D. chefrenianum, D. pyramidum, D. obtusum, D. sphaerula, D. ascidia* Beneden (= *D. lagena* Brandes nec Rud.), *D. ascidioides*, and probably also *D.* (*Brachycoelium*) *heteroporum.*

Regarding this proposition, it may be noticed: 1) that it was made in the text of an article 250 pages long, and the name is not contained in the index (pp. 251—252); 2) Looss himself did not use the genus in connection with a single species which he included in it (see Looss, 1896, pp. 64—86); 3) no diagnosis was given; 4) no type was designated; 5) the name was even proposed with reserve. In other words, it was purely a matter of luck and chance, if an author examining Looss' superb paper should happen to discover that a genus *Lecithodendrium* had ever been proposed, and even then one might suggest that Looss only said that the name *Lecithodendrium* "might perhaps" be used, or that it was incidently used, as Looss claims Rudolphi used *Sphaerostoma*; Braun in his review of Looss (1896) discovered the name; Stossich (1899) also discovered it; Hassall and I failed to discover it. Further, 6) all of the species Looss included in his *Lecithodendrium* come within the generic diagnosis of *Brachycoelium*, 1845, and since this latter subgenus was mentioned in so many modern papers, there are no grounds for assuming that it was unknown to Looss; 7) yet, Looss did not show wherein his genus differed from Dujardin's *Brachycoelium*, and so far as any thing contained in Looss' paper is concerned, an author would be perfectly justified in suppressing *Lecithodendrium* in favor of *Brachycoelium.*

In other words, in proposing *Lecithodendrium*, Looss failed to do what he should have done to insure his genus, namely, he should have given it a diagnosis, showing wherein *Lecithodendrium* and *Brachycoelium* differed, and he should have designated types for both genera. Having, in addition to these omissions, published the genus in a way (in the text) calculated to aid other workers to overlook it, and having failed to connect the name with a single specific name he intended to consider in connection with it, it would appear that my good friend and colleague is hardly in a position to think it strange if circumstances result in suppressing *Lecithodendrium*. And in order to prevent such cases in the future, I readily join with him in the exclamation, "Deshalb Vorsicht in der Aufstellung von typischen Vertretern" — to which I would add: And be sure to designate the types at the time the genus is originally proposed!

LOOSS (1899, p. 614) admits that "*D. retusum*" is problematic; that "*D. arrectum*" is also not certain; and that "*D. clavigerum*" of DUJARDIN is a misdetermination. Accordingly, for him, the choice of the type of *Brachycoelium* would naturally lie between *D. heteroporum* and *D. crassicolle*, and he remarks that (if HASSALL and I had not already selected *D. crassicolle*) he "might now [1899, i. e., three years after his *Lecithodendrium* was proposed] very easily select the name *Brachycoelium* for the genus based upon *D. heteroporum*".

I will not lay stress upon the fact that LOOSS in 1896, considered *D. heteroporum* as a probable member of the genus *Lecithodendrium*, and that he would at that time, therefore, not have selected this species as type of *Brachycoelium* since such action would have invalidated his own genus *Lecithodendrium* (hence the inadvisability of designating *heteroporum* as type in either 1896 or 1898 is too self evident to need discussion), but I will turn to another phase of the subject.

Upon examining the literature, it is seen as LOOSS (1899, p. 612) states, that MINOT (1878) and BRAUN (1895, fig. 45, p. 128) have given figures of *D. crassicolle*. As stated by LOOSS, MINOT has given a detailed description of this form; furthermore, as also recognized by LOOSS, an exceedingly important character, not clear from MINOT's paper, is clearly shown in BRAUN's figure, namely, a cirrus pouch is present. In referring to the latter, LOOSS remarks: "It remains, however, a question whether the figure is not made somewhat diagrammatic corresponding to its special purpose in the given place." To this I am constrained to reply that I would be no more inclined to assume that an authority like MAX BRAUN would deliberately draw a diagram of a trematode, insert a cirrus pouch if it were not present, and label it *D. crassicolle*, than I would assume that an authority like LOOSS would deliberately figure organs he did not see in one of his own species. If BRAUN wished to draw a diagram of a trematode which had a cirrus pouch, he would not deliberately select a species which had none, and then insert it from imagination. I have not the honor of the personal acquaintance with Prof. BRAUN as I have with

LOOSS, but from the work and reputation of both men, the fact that they insert a cirrus in any given drawing, diagrammatic or otherwise, and give to that drawing the name of a certain species, is prima facie evidence that I must assume good faith on their part and consider that they saw a cirrus pouch or some structure which they interpreted to be such. It may further be added that a cirrus pouch is described by DUJARDIN, 1845, p. 405 [1]), and is also referred to by PARONA, 1896, p. 15 [2]) in connection with a form taken as synonymous with *D. crassicolle.*

From the above discussion, from which I have endeavored to eliminate the subjective element so far as possible, it will be seen that I maintain that all due care was exercised in selecting *D. crassicolle* as type of *Brachycoelium* in 1896. Unfortunately I was not aware of LOOSS' *Lecithodendrium*, but had I known of it, I should certainly not have selected *D. heteroporum* as type of *Brachycoelium*, since such an action would have been more likely at that time to jeopardize *Lecithodendrium* than would the selection of *D. crassicolle.*

Under the circumstances, *D. crassicolle* was the most natural species to select, for it was not apparent why any other species should be better selected, and in selecting *D. crassicolle* the ruling was made in accordance with the views of those systematists who believe either in page precedence or in selecting the oldest species. While I am not a believer in either of these latter views, still unless in any given case I can show why they should not be followed, I am willing for harmony's sake to adopt them.

In reviewing the entire subject, and giving all due consideration to the views advanced by my friend LOOSS, I can not escape the conclusion that whatever difficulty may arise in this case is due solely, entirely, and absolutely to the manner in which *Lecithodendrium* was proposed in 1896, and to the fact that LOOSS failed at that time to fulfill the conditions he would have fulfilled, had he not felt it necessary to admit (see LOOSS, 1899, p. 523): "That also in other specialities of zoology similar practical difficulties arise is not impossible, still I have no judgement in regard to the matter."

After this review of the case, it will hardly be necessary to discuss in detail the views which LÜHE, 1899, p. 536, has advanced,

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1) "Penis assez mince, replié dans un réceptacle peu volumineux, courbé en avant et appliqué au côté droit de la ventouse".

2) "Cirro non bene distinto et racchiuso in borsa ovale".

since it is apparent that when he claims priority for *Lecithodendrium*, 1896, over *Brachycoelium*, 1845, and that when he maintains that in selecting a type for *Brachycoelium* "a necessary prerequisite would be that the species [in his discussion *D. heteroporum*] should not be placed in *Lecithodendrium*," he has argued the case without due consideration of the numerous points involved. Furthermore, since he overlooks the universally recognized rule that after a type has once been designated, no one can change to another type without showing that at the time of designation the species in question was not available as type. His referring to the case also, as a "nomenclatural doctorate-question" shows plainly that the principles involved, and the necessity and broad application of those principles have, for the moment, escaped his memory.

22. The case of *Campula, Opisthorchis,* and *Brachycladium.*

In Note 48, the opinion was expressed that *Campula* was generically identical with *Opisthorchis*, hence the latter was made a synonym of the former. Braun (1898) and Looss (1899) differ from this opinion, Looss giving his reasons for the position he takes.

In connection with the subject at issue, the following points may be noticed: Cobbold in 1859 proposed *Campula* for a distome found in *Phocaena communis*, possessing digestive caeca which "instead of displaying the dendritic character of the Fascioles, offer a peculiar zigzag-like form". In 1878, he determined certain worms from *Platanista gangetica* as identical with his *Campula oblonga*, but, concluding that the genus was not well founded, named them *Distoma campula*. Looss admits "*Distoma campula*" as a typical *Opisthorchis*, but believes that "*Campula oblonga*" is an entirely different species. He also lays considerable stress on the fact that Cobbold himself rejected his own genus *Campula* — a point which to my mind is of no consequence whatever, since Cobbold no longer possessed any rights over *Campula* different from the rights possessed by other authors.

Looss then argues that Cobbold's *Campula oblonga* is generically identical with *Distoma palliatum*, for which he now erects a new genus, *Brachycladium*, despite his own assertion that this species is congeneric with a genus (*Campula*) already proposed by Cobbold.

In his argument that *Campula* is not congeneric with *Opisthorchis*, Looss has indeed, as must be frankly admitted, put forward an exceedingly plausible case, an argument which has undoubtedly already carried conviction to many persons, and for which I express a very high appreciation. Admitting for the moment that he is in the right, and that I and HASSALL are in error, has LOOSS been justified in rejecting *Campula* which he declares is not identical with *Opisthorchis*, but which he asserts is identical with his new genus *Brachycladium*? It would indeed appear that in making out such a strong case against us, he has not followed up his success by keeping his own ruling relative to *Campula* entirely free from criticism.

Now, let us examine the exact status of *Campula*. That *Campula* was not described by COBBOLD in 1859 in so exact a manner as LOOSS describes his genera, may be admitted without question. That COBBOLD in 1878 determined specimens from *Platanista gangetica* as identical with his *Campula oblonga* of 1859 from *Phocaena communis*, and that he figured them quite clearly, may, without any injustice to anyone, be construed as a later effort to more definitely fix the genus *Campula* or rather the species *C. oblonga*. Now it apparently did not occur to LOOSS that COBBOLD might have had two species before him in 1859. It appears quite certain, however, that the specimens of *Campula oblonga* collected by HASSALL from *Phocaena communis*, and determined by COBBOLD, contained two species. One of these species was in my possession when I asserted with HASSALL that *Campula* could not be separated at present from *Opisthorchis*. That specimen was an *Opisthorchis*. It has unfortunately been lost during my two years absence on foreign service. Since my return another specimen has been found and mounted, and that is unquestionably generically identical with *Distoma palliatum* hence a *Brachycladium*.

Accordingly, it would appear that the case is not quite so clear as would seem from LOOSS' argument. Both his and our positions are open to criticism. Our position is weakened because of the unfortunate loss of the auto-type (specimen determined by the author of *Campula oblonga* as identical with his species), the specimen upon which we made our assertion, hence my inability to prove the correctness of our study by a drawing of that specimen. Furthermore, it is weakened by the fact that, as LOOSS has pointed out, COBBOLD's description applies in reality more closely to *Brachycladium* than it does to *Opisthorchis*. Finally, and most important of all, the fact that at the time I examined COBBOLD's specimen, and, indeed, until a very short time ago, I was under the impression that I had before me an original of 1859, whereas I had only an auto-type collected some years later.

Looss' proposition is weakened by the fact that he deliberately proposed *Brachycladium* for the genus in which he claimed *Campula oblonga* belonged; furthermore by the fact that Cobbold later determined two species from *Phocaena communis* as members of his species *Campula oblonga.*

The case of *Campula*, *Opisthorchis*, and *Brachycladium*, has thus become somewhat complicated. Nevertheles, it is clear what should be done. The first point is, that since I was laboring under a misapprehension in 1898 in supposing that I was dealing with an original 1859 specimen of *Campula oblonga*, and in reality therefore based my statements upon an erroneous premise, the ruling that *Opisthorchis* is synonymous with *Campula* must be rejected unless it can be supported by the production of an unquestionably original specimen of 1859. Since this can not at present be done, I recede from the ruling and acknowledge that there is at present no reason for assuming that *Campula* and *Opisthorchis* are congeneric. In other words, I accept Looss' view unreservedly in this particular, and admit the ruling of 1898 to be rendered valueless by its erroneous premise.

I am unable however to accept Looss' (1899) view that *Campula* 1859 is to be rejected. He himself admits it to be congeneric with *Brachycladium*, 1899, hence the latter name must naturally be suppressed in favor of the former. Accordingly *Opisthorchis* Blanchard, 1895, should be reinstated, and *Brachycladium* falls as a synonym of *Campula* [1]).

23. The date borne by a publication is to be assumed to be correct, until it is proved to be incorrect.

Many journals are supposed to be issued on certain specified dates and they bear the dates in question upon their cover. Yet circumstances frequently result in delaying the publication by a day or a few months. In this way it occasionally arises, that one paper

1) Since writing the above, a paper has appeared by Braun (1900) in which he comes to exactly the same conclusions relative to re-establishing *Opisthorchis* and suppressing *Brachycladium.* — Looss, 1901, p. 209, also accepts *Campula.*

apparently antedates another although it does not do so actually. In such instances, the ruling of the A. O. U. Code is that the date given on the paper shall be assumed to be correct unless it can be proved to be incorrect. If proved to be incorrect, the actual dates are taken in preference to the dates borne by the publications.

A case which falls under this general rule has recently occurred in helminthological writings. Two papers, one by Looss and the other by LÜHE, happen to bear the same date, December 28, 1899. Under ordinary circumstances, these dates would be accepted. It so happens, however, that both authors have proposed new generic names for the same genera, and it therefore becomes necessary to rule that one paper shall be given priority over the other. The natural tendency would be to rule in favor of Looss' paper, since it is a more extensive publication, more carefully prepared, more clearly written; it contains both the designation of types and full diagnoses, and in many cases illustrations of the genera. If it were impossible to show that LÜHE's paper has any prior claim, it would be natural to prefer Looss' publication. It so happens, however, that from the evidence at hand, the date on each paper is incorrect. In reply to a letter to Professor SPENGEL, the editor of the Zoologische Jahrbücher, asking whether a copy of this paper was recorded in the library of the Zoological Institute at Giessen on December 28, and thus open to the public on that date, word has been received that there was a delay of several days in its issuance. If a single copy could be shown to have been registered on December 28 in any public library of the world, open to scientific workers, that date could be accepted. The fact that SPENGEL is editor of the Zoolog. Jahrbücher would not have invalidated the date, in case his Institute were the only one which had it on that day, for any person could have consulted it in that public institution. Such proof, however, cannot be submitted. A letter has been received from Dr. LÜHE dated March 19, 1900, stating that he has communicated with the publishers of both the Zoologischer Anzeiger and the Zoologische Jahrbücher, relative to the point at hand, and that according to their statements, LÜHE's paper was distributed on December 29, Looss' on December 30[1]). LÜHE further states that BRAUN accepts

1) "Sicher ist jedenfalls, dass das Heft der Zoolog. Jahrbücher erst am 30. December zum Versandt gelangt ist, die Nummer des Zoolog. Anzeigers dagegen am 29. December. Diese Angaben rühren von den beiden Verlegern her (FISCHER bezw. ENGELMANN)."

these dates as correct. Prof. SPENGEL writes me that LOOSS' paper was not distributed until January 4, 1900. In view of the conflicting evidence, and in view of the unfortunate circumstance connected with changing the date of LOOSS' genera and species to 1900, it appears best to adopt the dates decided upon by BRAUN and LÜHE. It is therefore necessary to give LÜHE's paper priority by one day over LOOSS' [1]) paper.

The question might be raised that both SPENGEL and CARUS, as editors of the two journals, might have received copies of the journals earlier than other persons, and hence should not be taken into consideration; that, on the contrary it should be required that some other person should have the journal and should have received it through a book-seller. These points, which as a matter of fact have been raised by one of my colleagues, cannot be recognized as free from objection. So far as the private libraries of these two editors are concerned, the point may be acknowledged as applying both to CARUS and to SPENGEL. So far, however, as the library of the Zoological Institute of either Giessen or Leipzig is concerned, the fact that SPENGEL is connected with the University of Giessen, CARUS with the University of Leipzig, cannot be interpreted as depriving these two universities of the privileges enjoyed by other public institutions of learning. If any public library in either Giessen or Leipzig could show a record that it had received a copy of LOOSS' paper on December 28, I should unhesitatingly adopt that date.

1) LOOSS in two papers just published, objects to this ruling. Nearly all of the questions which he raises in connection with priority, date of publication, nomenclature etc., have been discussed in detail in connection with other groups, so that it seems hardly necessary to repeat all the arguments here. I agree with LOOSS fully that it does seem unjust to give LÜHE's paper priority over his own, but precedents of this nature have been established on a basis which eliminates the subjective element as far as possible. A paper is not "published" until it is open to the public. No other circumstances need be taken into consideration in establishing its date. Suppose for instance that both LOOSS and LÜHE's papers were printed on the same day, and bore the same date but that the entire edition of LOOSS' article had been accidentally burned — such a case is possible — LOOSS' present arguments would hold as well under those conditions as under the present. LÜHE's paper, by the statement of LOOSS' publishers was open to the public earlier than LOOSS'. The date of publication on the papers on both cases is incorrect. All of these points may be found by studying the history of nomenclature.

Relative to the claim that the work should be obtained through book-sellers, it must be replied that this point cannot be admitted. Many Governments publish documents of different kinds and distribute them *gratis*. If these documents are given to libraries which are open to the public, the publications are open to the public just as truly as if they had been obtained by purchase. The point at issue is to show a work can be consulted by the public, not whether it has been sold. Any other ruling than this would invalidate thousands of names.

Zoological Laboratory, Bureau of Animal Industry
U. S. Department of Agriculture,
March 15, 1901.
