

breitung besitzen müßte. Mir ist es nicht gelungen, diese Pflanze zu finden. Nach meinen Beobachtungen sind *E. montanum* × *obscurum*, *E. montanum* × *parviflorum* und *E. parviflorum* × *roseum* in N. als die häufigsten Epilobienbastarde anzusehen. In Becks Flora fehlt der erstgenannte Bastard, der zweite wird mit 1, der dritte mit 2 Standorten angegeben. Wenn auch durch meine Ausführungen die Kenntnis von der Verbreitung der Epilobienbastarde erweitert wurde, abschließend kann sie noch immer nicht sein. Es bleibt der Zukunft vorbehalten, durch neue Funde neue Erkenntnisse reifen zu lassen. Die Epilobienbastarde seien der Aufmerksamkeit der Floristen empfohlen.

## Dr. Stiles' "American referendum" on three propositions in Nomenclature.

By **Franz Poche**, Vienna,

Chairman of the "Nomenklaturkommission des Verbandes Deutschsprachlicher Entomologen-Vereine".

(Eingelaufen am 19. X. 1929.)

The explicit aim of the propositions in question<sup>1</sup> is to *reduce the number of changes in nomenclature* without any prejudice to the law of priority and (prop. III) to *abolish the liberum veto* in the Commission on Nomenclature (v. Poche, 1919b, p. 75–77, 85, 99 and 127). — 1929b, p. 1559 Dr. Stiles, Secretary to the International Commission on Zoological Nomenclature, says: "On basis of the appended documents, and supported by numerous conferences with zoologists in various specialities, I conclude that the opinion of the overwhelming majority of the zoologists in the United States can be fairly summarized about as follows". He then gives such a summary under 15 points [a)–o)].

At the outset the Secretary's summary, being largely based on uncontrollable conferences, is in the same measure *uncontrollable* too and is thus *a priori* very feeble evidence. Add to this that in such conferences it is easy to lead persons not well versed or not interested

<sup>1</sup> Dr. Stiles consistently calls them Dr. Poche's propositions. While I do not object to this as a short appellation for them, it is somewhat misleading. For I cannot and never did claim the exclusive authorship of them, various other zoologists, notably Ganglbauer and Hendel, also having collaborated in formulating them. Further, they were not brought in by me alone, but by more than five hundred other zoologists as well. I may, however, fairly be called the chief champion of these propositions, as Dr. Stiles does 1929a, p. 147.

in nomenclature by suggestive questioning to give the answers as desired. Further, some of the points in the Secretary's summary, notably *l*–*o*), are so complicated and refer to matters so little known to most zoologists, that it is *obvious that they cannot possibly represent* "the opinion of the overwhelming majority of the zoologists" in the U. S. And: "*Nihil probat qui nimium probat*", nothing proves he who proves too much, viz. more than *can* be true.—I will now take up seriatim those points that need a comment.

*a*) Dr. Stiles claims that the propositions were studied carefully by certain committees on nomenclature in the U. S. and that not one of these approves of them.—To this I state that such committees were created—and apparently at Dr. Stiles' instigation—for the *sole purpose* of reporting on the proposals in question (and of course in the sense desired by him). For Cort, 1927, p. 227 says of the committee on Nomenclature of the American Society of Parasitologists (then under the presidency of Dr. Stiles): "The committee was not continued" (i.e. after it had fulfilled that purpose), while a year before this committee evidently did not yet exist (see —, 1926). And it is very unlikely that I should actually have hit upon the *only* such case.—It is further apparent from the report of said committee (Hall, Schwartz, 1927), that the latter *has not seen my paper* (1919b) *giving the full reasons* for the propositions. For while I (p. 91) explicitly say that the fact that an author does not name species does *not* constitute an offence against the principles of binary nomenclature, Hall, Schwartz, p. 228 erroneously put the contrary construction on proposition I; and likewise they claim *without any comment* that generic names of polynomial authors may be recognized under the code, while I (p. 85–88) had proven that this is *not* the case. That under *such* circumstances no adequate study of the propositions and no fair report on them was possible, is self-evident. And this was in Washington, the great centre of American biological science!

*b*) For anybody knowing "how things are done" even in matters of science, it is not at all surprising that Prof. Stiles in his governmental position and by the means resorted to by him [see below sub *d*)–*e*), *h*) and *m*)] has succeeded in inducing "Governmental zoological Bureaus and Divisions, scientific societies, and zoological staffs of universities, colleges, and museums" of his country to take "formal action" against the propositions in question.

*d*)–*e*) "A referendum in regard to these propositions has been conducted" (by Dr. Stiles—see his printed circular). The votes

show 9 for, and 546 against proposition I, 4 for, and 553 against proposition II, 4 for, and 562 against proposition III.—Every fair-dealing person will admit that there are not in the U. S. (nor of course in any other country of the world) anything like 550 persons capable of passing an independent judgment upon propositions II and III. Further, the point noted sub *b*) comes in equally here too. And above all note how the Secretary conducted the said referendum. The most pernicious trait introduced by him into it is *his abuse of national feeling* to induce his countrymen to vote against the propositions. For it leaves room for no other interpretation when he repeatedly in his printed circular of April 11, 1927 speaks of "American objections" to Dr. Poche's propositions and in the voting sheets submits to his compatriots the alternative of concurring with A. "Dr. Poche's propositions" OR B. "American objections," while he at the same time carefully omits any mention of the fact that at the last (1913) meeting, to which he refers, these propositions had been signed by about 550 zoologists of all nations, among them numerous Americans! Lastly, it is obvious that the persons involved sub *a*) and *b*) are again included here. Moreover, Dr. Stiles in the circular mentioned grossly *misleads his readers by statements which are contrary to the objective facts very well known to him* [see below sub *h*) and *m*)].—In full accord therewith experience shows the judgment of our American colleagues, when not misled by wrong assertions, to be very different from what it would appear from the Secretary's statistics (see e.g. Curtis, 1912, p. 933 f. for an *unanimous* proposition by the American Society of Zoologists, Central Branch, which goes even somewhat farther than proposition III, and Kingsley, 1912). Were it otherwise, these statistics would betray a very regrettable discrepancy between their views and those of the great majority of the zoologists of the rest of the world.

f) The liberum veto in the assembled Commission on Nomenclature is here defended as conducive to stability of the Rules, while proposition III "is viewed in the United States not only as jeopardizing stability but as actually inviting instability".—Of course a liberum veto is always conducive to stability; none the less it has long since very justly been universally rejected by parliamentary theory and practice, where stability certainly is at least as important as in our Rules. For *that stability is bought far too dearly* at the price of progress and of the capability of action. As proposition III simply abolishes the liberum veto, it no more jeopardizes stability or even invites instability than the generally recognized parliamentary majority

principle, the less so as it provides for a majority at two separate votings. If, however, the Americans, in spite of all this, really think the liberum veto so beneficial a principle, let them first introduce it into the Congress of the U. S. and into the law-making bodies of at least some of their single states.

g) Here it is claimed that questions of nomenclature should be decided by the Commission on Nomenclature and *not* by the International Congress.—This goes totally beyond the mark. For *never* has the Commission had that power nor has it ever been claimed for it. As Dr. Stiles, 1905, p. 9 very rightly says: "The commission considers it [viz. a nomenclatural proposition submitted to it] and makes recommendation to the congress, which has the final decision in the matter"; and he moreover *fully approves* of this mode of procedure. See likewise Blanchard, 1905, p. 12. The aim of proposition III simply is to prevent that the Congress be deprived of the opportunity to take action on nomenclatural propositions or even to hear of them, however well-founded and necessary they may be, because *one* member of the assembled commission votes against them.

h) Here it is asserted that the present policy (i.e. the liberum veto) "was established at the Cambridge (1898) and Berlin (1901) Congresses" and "has been accepted by the succeeding Congresses of 1904, 1907, 1910, and 1913".—Dr. Stiles's own representation of the mode of procedure in his commission [v. sub g)] is the best proof that *at least up to 1905 there was no trace of a liberum veto* and that all claims to the contrary *later* raised by him (also in his circular of April 11, 1927) are based either on irrelevant facts (e.g. utterances or actions of single individuals, which in part would appear to have been gross transgressions of their competence by a General Secretary and a President of earlier Congresses) or on assertions which he has subsequently deemed expedient to make though himself best knowing their falsity. I have conclusively shown this 1919b, p. 127–137;<sup>1</sup> 1927e;<sup>1</sup> 1929a, p. 1548–1551, and also, that the liberum veto was neither adopted by any of the succeeding Congresses.

i) "We hold that to raise 'elimination' to the status of a 'rule', as is proposed in proposition II, would be to reopen a *res judicata*; it would abrogate the agreement reached in 1907, namely, *the application of the objective Law of priority to genotype determinations*".—This

<sup>1</sup> Upon oral information since received, contrary to the printed documents, I no longer maintain that the Commission did submit its Report to the Cambridge (1898) Congress.

sort of reasoning *would obviously apply with equal force to every alteration of the Rules*, every provision of which naturally represents a *res judicata* as long as it stands. More especially it clearly applies to the very "agreement" it is intended to defend. For by the latter a *res judicata* in 1901 was reopened, viz. the recognition (not by name, but in fact) of previous elimination as a rule (Art. 30), and the agreement reached at that date abrogated. Further the "agreement" of 1907, i.e. *the arbitrary designation of types* (irrespective of previous elimination) is directly *opposed to the Law of Priority*, while it is the method of elimination that is in full accord with the latter (see Allen, 1906a, p. 778 f.; Coquillet, 1907b; Poche, 1919b, p. 111 f.; 1929a, p. 1541; Fejérváry, 1923, p. 166).

j) Dr. Stiles says: An abrogation of the 1907 agreement "would invalidate much constructive work that has been accomplished, and throw nomenclature into serious confusion; it would also tend to confine the work of genotype determinations to relatively few specialists located at the great library centres". Under the arbitrary designation of types "many publications can be omitted from consideration as their authors are known not to have worked on the genotype basis".—Quite on the contrary to the two first-quoted assertions, for which not a shadow of evidence is given, it is just the 1907 agreement which, *if considered as legal* [see below sub b)] and hence accepted, *invalidates a great part of the constructive work* of restricting the meaning of generic names and of thereby often also automatically fixing types that has been done upon the principle of elimination during a century and a half of our science, and *causes dreadful confusion in nomenclature* (v. Allen, 1907e, p. 41 ff.; Williston, 1907; Poche, 1919b, p. 113–115; 1929a, p. 1541–1543; Barnes and Lindsey, 1922, p. 89). The abrogation of the 1907 agreement will, to be sure, "invalidate much constructive work" of that sort which transfers the name *Turdus* to *Planesticus* and calls *Turdus Arceuthornis* (Oberholser, 1921, p. 105), forces us to call *Alca Pinguinus* and *Plautus Alca* (Allen, 1907 e, p. 43), transfers the names *Tenthredo* to *Allantus* and *Ichneumon* to *Ephialtes*, makes *Pimpla* a synonym of *Ichneumon* and coins new names for *Tenthredo* and *Pimpla* with all the corresponding overthrow of supergeneric and specific names (v. Rohwer, 1910, p. 117; Viereck, 1914, p. 52 [cf. p. 57]; cf. Poche, 1917, p. 44 f. and 49–51). Moreover, it is said agreement which confines the determination of types to a few great library centres, while elimination can in general be applied on the basis of readily accessible catalogues, records etc. (v. Jordan, 1907, p. 468;

Allen, 1912, p. 808; Poche, 1929a, p. 1544 f.). This far outweighs the last-mentioned point brought forward by Dr. Stiles in favour of the former method. For a very large part of the publications absolutely necessary under it are only to be found in the great library centres; and their lack can nowise be compensated by the possibility of omitting other publications from consideration.

k) Here rather vaguely exception is taken to the definition of elimination given in proposition II.—Full reasons for all points of that definition had been given by me 1919b, p. 102–107. Besides, every point of it can easily be altered if this is deemed advisable. It was just the *lack* of a sufficient definition of “elimination” which led to the unsatisfactory results with it in the U. S. hinted at in the Secretarys next sentence.

l) Here it is said: The 1907 agreement “appeals to us as a workable adjustment of the different viewpoints of the ‘eliminationists’, of the ‘1st species’ advocates and others. We stand by that agreement which we accepted in good faith and on basis of which we have been working for 20 years.” We “are persuaded that a number of signers of the POCHÉ propositions attached their names without full knowledge of the history of the subject”.—The “‘1st species’ advocates and others” were always only a small minority (except for a short time in the American Ornithologists’ Union) and their arguments either totally invalid or greatly outweighed by the numerous and very serious disadvantages of that method (v. Poche, 1912 j, p. 38–43, 45–52 and 59–61 and the literature there quoted). They thus afford a very poor excuse for upsetting the method of elimination so long and widely used and so firmly established in our science. The next argument is in effect the same as that already advanced sub i); I need thus only refer to what I have said there. I will only add that the provision embodying the “agreement” in question [Art. 30 (g)] was *illegally*<sup>1</sup> brought before the Congress by the Secretary, its author, was thus illegally introduced

<sup>1</sup> No proposition for change in the Rules may be brought before the Congress unless it has been in the hands of the Commission on Nomenclature at least a year before the meeting of the Congress. The proposition in question was, however, only proposed to the Commission immediately before or at the Congress, as Dr. Stiles (1911b) himself admits. Neither can it possibly be construed as a “modification” of his proposition of 1905 for the alteration of Art. 30. For there previous elimination (albeit in a more restricted sense than hitherto) is explicitly recognized as *excluding* the respective species from being selected as type. The 1907 “agreement” is thus in the most important point at issue just the contrary of his proposition of 1905.

into the Rules and is hence *invalid* even from a strictly formal point of view (see Poche, 1914c, p. 40-43; Stichel, 1928, p. 150). And finally: Do the signers of the "American objections" really think that among themselves there were *not* "a number... without full knowledge of the history of the subject"? [Sections *m*) and *h*) afford the most cogent proof that there *were*.] Can that argument then fairly be used against the propositions in question?

*m*) Dr. Stiles says (and similarly in his circular of April 11, 1927): "Proposition I was a subject of controversy for more than half a century. Under the International Rules as interpreted by the Commission it became a *res judicata*."—Quite on the contrary there was *never the slightest doubt* or controversy about the essential point of proposition I, viz. the meaning of the term binary nomenclature, till Dr. Stiles, 1910a, p. 48 ff. *against his better knowledge*—as obvious from his earlier papers (in: Stiles & Carus, 1898, p. 18; 1905, p. 11 and 24; in: Stiles and Hassall, 1905a, p. 9)—bestowed upon us an interpretation of this term which is as absolutely novel as wrong (v. Mathews, 1911, p. 1 f.; Lönnberg, 1914c; Poche, 1919b, p. 86-90; 1929a, p. 1534-1537; Bather, 1924). The *only* controversial question was, whether such names of non-binary authors as conformed to the rules of the binary nomenclature were to be accepted. This has—and very justly so—been unreservedly decided in the negative by the International Rules ever since their beginning<sup>1</sup>, and this decision is fully endorsed in proposition I. Cf. Poche, 1927c, p. 229-231.

*n*) This paragraph is admittedly fictitious. I will therefore only say that the main part of it applies with equal force to the "1907

<sup>1</sup> This decision is so well founded theoretically and practically (v. Poche, 1919b, p. 94-98) and has received so widespread approval that any *open* attack upon it was doomed to certain failure. It did however *not* suit the special wishes of the active and influential American Ornithologists' Union. *Here apparently lies the reason* for Dr. Stiles' procedure referred to above, and also why Stejneger, 1924, seconded him. The latter further introduced (p. 1, 7, 16 [cf. Poche, 1927c, p. 82, 132 and 225]) the confusion of the two totally distinct questions pointed out above, which confusion Dr. Stiles in his turn very skilfully uses in his referendum to mislead his unsuspecting readers. I have op. c. *carefully refuted Stejnegers paper point for point* and shown that it is based on an almost endless series of wrong statements of fact, contradictions within the paper itself, arbitrary and in part demonstrably wrong assumptions, logical faults, intentional suppressions of important paragraphs in the literature, respectively facts, from which the incorrectness of assertions made by Dr. Stejneger is obvious, and last but not least on an absolute disregard of all the cogent proofs brought forward by various authors of the falsity of the interpretation recently advocated by Dr. Stiles.

agreement" [cf. sub *i*)] and in fact to any and every change in the Rules, and that here again matters are so represented as though the propositions in question were brought in by me alone, while in fact they were brought in by hundreds of zoologists. Further, I certainly never claimed and never imagined to be able "to organize a local majority [at the Congress] *pro* or *contra* according to my views", the sole influence which I may possess being the strength of my arguments. Was it not rather the Secretary who had this opportunity at the Boston Congress (1907) and amply availed himself of it? Finally, I must again absolutely repudiate the bringing in of the point of nationality into purely scientific questions.

o) Here the complaint is raised of the Commission's time being occupied "by reconsideration and reconsideration of questions which it has already definitely decided", and it is affirmed that these three subjects "have long ago been settled by the Congress and by the Commission".—As far as there may be *any* foundation for the said complaint, the Commission has only itself to blame. For it has in its majority assented to the Secretary's disobeying the explicit order of the Congress, overthrowing—and moreover in an illegal manner—the longestablished method of elimination, and promulgating a totally novel and false interpretation of the so well-known term "binary nomenclature", or has at least suffered him to do so. And that, although the first two points had—with the full approval of the Secretary—long ago been settled by the Congress and absolute unanimity (including the Secretary!) had always reigned as to the third [see above sub *g*)–*i*), *l*) and *m*)]. Besides, it is of course *out of the question to debar any point in science from future consideration* by an autocratic or bureaucratic "already decided", regardless of any and all arguments advanced and experiences gained.

As to the Secretary's comments on the first list (of 1913) of supporters of the propositions in question, it was obviously to be expected that after the lapse of fourteen years (and the greatest war of history) considerable changes should have occurred, and that further the Secretary would by means of a gross misrepresentation of the facts and an appeal to nationalism [see above sub *d*)–*e*), *h*) and *m*)] be able to incite some of his countrymen to revoke in whole or in part their approval of the said propositions.

The Secretary further strongly emphasizes that the voting sheets in his referendum (contrary to mine) show whether the voter favoured my views or the American objections to them (thus giving a seemingly *objective* representation of the opinions of the voters). Would they



really give this, that would surely be a valuable asset. To attain this end the voting sheets would, however, have had to be submitted to the voters either without any further comment or with an *impartial* statement of the arguments *for and against* the propositions in question. Instead of this the Secretary's covering letters are in fact, if not in form, only an ill-disguised polemic against them, which even *resorts to totally wrong statements as to actual facts* [sub 2. and 4.; cf. above sub *h*) and *m*)]. The effect of this on the value and weight of the Secretary's referendum needs no comment.

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## Aus der Nordtiroler Libellenfauna.

Von **Fritz Prenn** (Kufstein).

(Mit 6 Textabbildungen.)

(Eingelaufen am 3. VII. 1929.)

### 3. Zur Biologie von *Leucorrhinia dubia* (Vand.).

Die Gattung *Leucorrhinia* (Britt.) tritt in der Umgebung von Kufstein nur in der Art *dubia* regelmäßig auf. Zweimal gelang es mir auch, ein ♂ von *L. pectoralis* (Charp.) zu erbeuten. *L. rubicunda* (L.), die Ausserer für die Innsbrucker Gegend als selten anführt, habe ich hier nie beobachtet. *L. dubia* fliegt hauptsächlich an Torfmooren (Maistaller Moor), in denen die Larven ihre Entwicklung durchmachen, zeigt sich aber auch an Wassergräben und Tümpeln, jedoch viel seltener. Zumeist sind es ♂♂, die sich an solchen Stellen vorübergehend aufhalten; doch habe ich auch ♀♀ dort gesehen, aber nie bei der Eiablage, obgleich die Möglichkeit derselben wohl nicht ausgeschlossen ist.

Die Imagines erscheinen in der ersten Hälfte des Mai (früheste Beobachtung am 4. Mai 1927) und zeigen sich zu Ende dieses Monats am häufigsten. In der zweiten Hälfte des Juni nimmt ihre Zahl bedeutend ab und Mitte Juli gelingt es nur mehr selten, ein Stück zu beobachten. In günstigen Jahren kann man um den 25. Mai herum an schönen Vormittagen oft massenhaft ausschlüpfende Leucorrhinien sehen. Allenthalben steigen die Nymphen an den Stengeln des Fieberklees (*Menyanthes trifoliata* L.) empor, um sich in Libellen zu verwandeln. Da hängt ein frisch geschlüpftes Tier an die eben verlassene Larvenhaut geklammert, dort taumelt ein anderes mit rauchgrauen Perlmutterflügeln unsicher dahin und dazwischen flitzen und blitzen

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Jahr/Year: 1930

Band/Volume: [79](#)

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Artikel/Article: [Dr. Stiles' "American referendum" on three propositions in Nomenclature. 273-283](#)