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On Professor Brauer's paper: Versuch einer Characteristik der Gattungen der Notacanthen. 1882.

By *C. R. Osten Sacken.*

A comparative critical survey and a better definition of the genera of the larger families of the diptera is one of the most urgent wants in the present condition of dipterology. The publication of Dr. Brauer's work on the Notacantha was therefore hailed by me with a feeling of eager anticipation. Upon a rapid persual, I concluded to publish a short Addendum to it, a few notices on some little known genera, which I had seen in collections. But upon a closer study, I was sorry to find, that my remarks gradually turned into criticisms, and that the only alternative I had to choose from was either to preserve an absolute silence, or to publish a detailed critical review. Upon reflection, I determined to follow the latter course.

The families Stratiomyidae, Tabanidae, Xylophagidae, Acanthomeridae and Leptidae form a natural group, which has several characters in common: three pulvilli, total absence of macrochetae, and smooth legs, deprived of those bristles and spines, that distinguish the Asilidae, and, in a lesser degree, most of the Bombylidae and Therevidae. The tibiae especially are smooth, and when we see the genus Rüppelia Wied. figured with some bristles along the tibiae, we may conclude with a high degree of probability, that this genus does not belong in the circle of relationship in question. (Compare Note I). The femora are in some rare cases spinose (Subula), or dentate (Acanthomera). — The principal families in that group are well-marked enough; but there are, alongside of them, many forms of transition, so-called synthetic types, which render the exact definition of the families very difficult, and still more so, a linear arrangement. In preparing my Catalogue of N.-Am. Diptera (1878), I followed the arrangement proposed by Loew in the Mono-

graphs of N.-Am. Dipt. Vol. I, 1862, with a single slight modification.[1]) As a catalogue-maker I was not called upon to reform, and therefore I adopted that distribution, although in several points I dissented from it. Since then, I have matured my views on the subject; and I will avail myself of the present opportunity in order to state my objections; they refer to the family Xylophagidae.

I. In the first place I object against the juxtaposition of *Subula* and *Xylophagus* in the same ultimate subdivision. Since Meigen, in the infancy of dipterology, united both forms in the same genus, routine and nothing else seems to have induced authors to keep them together. What have they in common? Head, trophi, palpi, antennae, thorax, abdomen, legs, venation, offer differences that are obvious, and some of them are even of a higher order than mere generic differences. In Subula, the prosternal plate, intervening between the front coxae and the anterior thoracic orifice is large; the front coxae short; the abdominal segments but little extensile; in these characters Subula is like the Beridina. In Xylophagus, on the contrary, the prosternal plate is small, and hence, the interval between the front coxae and the anterior thoracic orifice is short; the front coxae are inserted very near the head; they are long, cylindrical and very movable; the abdominal segments are loosely joined, with extensile connecting membranes; all these characters are those of the group Tabanidae-Leptidae. A conclusive proof of this double relationship is afforded by the metamorphosis of these genera: the larva of Subula resembles those of some Stratiomyidae and its pupa remains within the larva-skin; the larva of Xylophagus is more like that of the Tabanidae, and its pupa throws off the larva-skin.[2]) — Still another argument in the same direction is supplied by the anatomy of those diptera. In Dr. Brandt's Vergl. anat. Unters. über d. Nervensystem d. Dipteren (Horae Soc. Ent. Rossicae XV, p. 97) I find that the formula for the distribution of the cephalic, thoracic and abdominal ganglia is the same for Xylophagus cinctus and Leptis tringaria and scolopacea (2, 2, 6); while it is different in the Tabanidae (2, 1, 4—6) and Stratiomyidae (2, 1, 4). — The nervous systems of Xylophagus and Leptis, as represented on the plates (fig. 11, 27, 28)

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[1]) Loew's section *Coenomyina* I retained as a separate family. Mr. Beling's discovery of the larva of Coenomyia has proved since, that its relationship to Xylophagus is much closer than I had supposed.

[2]) Dr. Brauer is aware of the differences between Xylophagus and Subula, but he does not attach to them the same importance as I do. (See his p. 5 at the bottom and passim).

are remarkably alike; the difference consists in the double commissures connecting the nervous knots in Leptis, a difference certainly less important than the above mentioned resemblance.

II. My other objection against Dr. Loew's conception of the family Xylophagidae refers to the location in it of the genus *Arthropeas*. In the notes to my Catal. N.-Am. Dipt. 1878 (page 223) an insect is described which I referred provisionally to the genus Arthropeas. It has the body of a Leptid (Symphoromyia), with the antennae of a Coenomyia. It will probably form a new genus, because besides the differences in the venation noticed by me in the description, it has no spurs on the front tibiae, while such spurs are distinct in Arthropsibirica. [1]) There can be no doubt of the relationship of that species to Arthropeas on on side, and of its belonging to the Leptidae on the other. A different species (from Washington Territory), apparently of the same genus, was recently communicated to me by Dr. Williston in New Haven; unfortunately it reached me in fragments. — The genus *Glutops* belongs in the same group, and as it was discovered during the preparation of my Catalogue, I felt at liberty to dispose of it, and laid claim by means of it to the position of the whole group among the Leptidae. (See Note II).

To sum up: of the components of Loew's Xylophagidae, *Subula* alone, in my opinion, must remain among the Notacantha. It may be placed among the Beridina, until its relationship is cleared up. *Arthropeas*, *Glutops* and my nov. gen. must be connected with the Leptidae. *Xylophagus* and *Coenomyia* would form the stock of the reformed family Xylophagidae, which must be brought in nearer connection with the Leptidae, and not with the Notacantha.

When I said above, that routine, and nothing else, seems to have kept Subula and Xylophagus united in the same ultimate subdivision, I should have excepted two authors: Latreille and Westwood.

In judging of Latreille's conception of the Notacantha, authors seem to have overlooked a rather important circumstance: Xylophagus, in Latreille's meaning, is equivalent to our Subula only. Our Xylophagus is his Pachystomus. (See Note III). As soon as we examine his system in the light of this interpretation, we find that it agrees with the distribution which I am proposing, and that my reformed Xylophagidae, that is the Xylophagidae minus Subula, are

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[1]) I obtained this and other details on Arthropeas sibirica through the kindness of Dr. Karsch in Berlin; his data confirm me in the belief that Arthropeas must be referred to the Leptidae.

nearly equivalent to his Sicarii (Coenomyia, Chiromyza, Pachystomus), which he puts among the Tanystoma.

Latreille's Notacantha (Fam. Naturelles, 1825, p. 493), consist of two tribes: Stratiomyidae and Xylophagi. The latter contains: Hermetia, Xylophagus Latr., which is our Subula, Beris and Cyphomyia. In other words, his Notacantha are our Stratiomyidae, plus Subula only.

In his Tanystoma the order is this: I Tabanidae (plus Acanthomeridae); II Sicarii (Coenomyia, Chiromyza, Pachystomus, that is, our Xylophagus); IV Leptidae.

As Tribe III he has the Midasii, which, in a later work (Regne Animal), he placed elsewhere.

The other author who recognized the true importance of the differences between Xylophagus and Subula is Westwood. He placed the former among the Coenomyidae (= Sicarii Latr.); for the latter, with Beris and Actina, he forms the family Beridae. Both families he considers as Notacantha.

My grouping of the families and genera in question agrees therefore, in the main, with that of Latreille. The only novelty is the location of Arthropeas and Glutops, two genera which were unknown to that author.

Dr. Brauer's Xylophagidae, considered as a section of the Notacantha, contain the following genera:

93. Coenomyia; 94. Heterostomus Bigot; 95. (?) Lagarus Phil. 96. Arthropeas; 97. Glutops; 98. Xylophagus; 99. Pachystomus; 100. Antidoxion; 101. Rhachicerus; 102. (?) Macroceromys Bigot. 103. Subula.

From this list *Lagarus*, *Pachystomus* and *Antidoxion* must be struck out; Lagarus is a Chiromyza with an open discal cell; Pachystomus is the same as Xylophagus, and Antidoxion the same as Rhachicerus.

It appears from this list that Dr. Brauer agrees, in the main, with Loew's view. But he goes farther than Loew; he not only coordinates Xylophagidae and Stratiomyidae; he subordinates them as sections of the same family Notacantha, as distinguished from his Tanystoma (Tabanidae + Leptidae; see Brauer l. c. p. 43).

He is led to this division by a character which he believes to have discovered and to which he attributes an unusual importance: the direction of the posterior branch of the fork of the third vein. Whenever this branch reaches the margin of the wing before the middle of the distance between the apex and the tip of the fourth vein, the genus belongs to the Notacantha; whenever the end of that branch

lies beyond that middle, the genus must be referred to the Tanystoma. (l. c. p. 3, lines 6—8 from bottom).

I have shown above that my views on the limitation and position of the Xylophagidae are different from these; and for this reason, I cannot attach to the alleged new character the same value as Dr. B. does. If he places Arthropeas and Glutops among the Notacantha, in virtue of that character, and in spite of their organization, why does he not bring in some Leptidae also: Symphoromyia crassicornis has the same venation as Arthropeas; and in Spania the posterior branch of the fork ends in the apex of the wing, as it does in Subula. Why is Spania left by him among the Tanystoma, and not Glutops? What becomes of his division of the Cyclocera in Notacantha and Tanystoma (see his p. 43), if the only character upon which this division is based is so insufficient?

In passing now to a detailed examination of Dr. B.'s paper, I will begin with the few addenda, that I intended to give.

Rosapha and *Tinda*. (page 8). I possess specimens of Rosapha bicolor Bigot from the Philippine Islands. The antennae are inserted below the middle of the profile, and the genus may be inserted in the dichotomic table as follows:

b) Endgriffel schmal, streifenförmig, microscopisch behaart, aber nicht seitlich gefiedert; Schildchen mit vier kleinen Dörnchen von beinahe gleicher Grösse; kleine Querader fehlend. *Tinda.*

c) Endgriffel lang, federartig, d. h. dicht zweizeilig gefiedert; Schildchen mit vier grossen, länglich-conischen Dornen, das mittlere Paar grösser; kleine Querader vorhanden. *Rosapha.*

In describing *Tinda*, in my Enumeration of the Diptera of the Malay Archipelago etc. (Annali del Museo Civico etc. Genova Vol. XVI, p. 393—492). I speak of a „much less distinct fringe on one side only". This refers to the microscopic pubescence on the surface of the lamel, and the term fringe is not appropriate. Both Tinda and Rosapha have contiguous eyes in the male sex; the former is described and beautifully figured in Walker's Ins. Saunders. Diptera, Tab. III, f. 3, under the (preoccupied) name of *Biastes*.

Toxocera (p. 33). I have seen in Mr. Bigot's collection the original type of T. limbinervis Macq. D. E. Suppl. IV, 45. It is the same as Endmeta marginata (F.) Wied. Mr. Bigot pointed out this identity to me. In Suppl. III, p. 16 Macquart had described the same

species under its true name, E. marginata! The genus Toxocera may therefore be struck out.

Myxosargus (p. 21). The specimens of this new genus, described by Dr. B. are from Mexico. I possess one from Dallas, Texas (collected by Boll); its antennae are altogether black; the middle tibiae are also dark, but the distal third is yellow; the grayish cross-bands on the wings are connected at the costa. Nevertheless, I do not doubt that it is the same species. I would not compare the antennae to those of Chordonota as Dr. B. does (Linn. Entomol. XI, Tab. III, f. 13). I remember seeing specimens of Myxosargus in Mr. v. Roeder's and Prof. Bellardi's collections.

Hylorus (p. 16, No. 99 and p. 32). I have seen Hylorus Krausei in Mr. Bigot's collection. It struck me by its resemblance to Chiromyza, from which it differs in having the third vein furcate. Mr. Bigot goes too far, I think, when he unites it with Chiromyza (Annales etc. 1879, 185).

Lagarus (p. 17, No. 110). Among the notes which I took in Mr. Bigot's collection, several years ago, I find one which say's that Lagarus is a Chiromyza with an open discal cell.

Macroceromys Bigot (p. 17, No. 118). Dr. B. says about the antennae: „ausser den zwei kurzen cylindrischen Basalgliedern, sollen 10—11 undeutlich geschiedene längere Geisselglieder vorhanden sein." This is not correct. Both in the Ann. S. E. Fr. 1877, Bull. p. LXXIII and l. c. 1879, p. 187, Mr. Bigot describes the whole antenna as counting 10—11 joints; and this agrees with the antenna of Subula, to which Mr. Bigot compares Macroceromys. The description of the wings is exactly applicable to Subula, and in that light, there is nothing incomprehensible in it. Macroceromys, which I remember seing in Mr. Bigots collection differs from Subula by its elongated antennae. There is no room for a comparison with Rhachicerus.

Antidoxion and *Rhachicerus* (p. 17). They are characterized thus:

Antennae pectinate, the single joints bearing branches *Antidoxion.*

Antennae moniliform or serrate, without branches *Rhachicerus.*

This is so far incorrect as one half of the North-American species of Rhachicerus have the antennae pectinate as much as a Ctenophora! Compare my Western Diptera, p. 212, where an analytical table of

the described species is given, based on the structure of the antennae. In my Enumer. Dipt. Malay Archip. p. 21 I have shown there is no reason to separate Antidoxion from Rhachicerus, and I based this statement upon the comparison of an Antidoxion from Sumatra with an undescribed Rhachicerus from Brazil, which I have seen in the Museum in Vienna. The identity of both genera has beeen recognized by Gerstaecker at the time of the publication of Antidoxion (Entomol. Bericht 1863, p. 410).

Rhachicerus (Syn. Antidoxion) shows a very stricking character in the deep emargination of the eye, on the frontal side; only a slight vestige of such an emargination is visible in Subula. Besides N.- and S.-America and the Malay Archipelago, Rhachicerus has also been found in Europe (Spain; see Loew, Beschr. Eur. Dipt. I, 24).

Solva (p. 17 at bottom). There is no doubt about the identity of Solva with Subula; compare my statements in Enum. etc. p. 19, based on Mr. Walker's types in London.

Exochostoma. The locality is given as: „Nord-Amerika, Süd-Europa" (p. 13, No. 17, also p. 31). The species from North America, although not otherweise indicated, is of course the *E. caloceps* Bigot, Ann. S. E. Fr. 1879, p. 217. I have not seen this species, but the description makes me suspect that it is an Odontomyia with an unarmed scutellum (like O. nigrirostris Lw.). Exochostoma is described as having two spines on the scutellum, a coloring of an entirely different character etc.

On p. 12 (No. 58, NB.) Dr. B. says: „Die Gatt. Exochostoma Macq. unterscheidet sich von den Verwandten dieser Gruppe durch die einfach bleibende dritte Längsader." Again on p. 27, line 7 from bottom: „dritte Längsader am Ende einfach, daher zweite Submarginalquerader fehlend." This statement is apparently based upon Macquart's figure, notoriously bad as they are. The description says explicitly: „deux sousmarginales: première assez longue et étroite; deuxième petite, apicale, éloignée de la marginale," which, of course implies a fork on the third vein. Had Dr. B. any other source of information about the venation of this genus?

Arthropeas (p. 17, No. 112, 113). Here again, Dr. B.'s statement disagrees from those found elsewhere, and yet, this disagreement is not alluded to, nor explained. Arthropeas is placed (l. c.) among the genera with an open anal cell. According to Loew's figure in the Stett. Ent. Z. 1850, Tab. I, fig. 46, the only published source of

information we have about the anal cell of this genus, it is closed. In describing my *A. leptis* (Catal. N. Am. Dipt. p. 224) I stated explicitly that it differs in that particular from the species described by Loew.

Anisophysa Macq. This name appears in the alphabetical list of the genera (p. 30), with the addition „(Pachygastrina?)". It is not preceded by a number, as are the other genera, and it is not found in the dichotomical table, nor in the synoptic one (p. 26—27). Now Anisophysa Macq. S. à B. II, 544 was introduced for Piophila scutellaris Fall. Meig, now called Scatella scutellaris. (Compare Schiner's Fauna Austr. II, p. 184). How does it happen to figure among the Notacantha? Apparently because in Loew's Monogr. N. Am. Dipt. I, p. 18, at the end of the genera, referred to the Pachygastrina, Dr. B. found the words: „perhaps also Phyllophora Macq. and *Anisophysa* Macq." If he had thought it worth while to take Macquart in hand, it would have easily occured to him that. in the above quoted passage Anisophysa is merely a lapsus calami for *Diphysa*.

Diphysa (p. 16). Dr. B. says it was quite arbitrary („ganz willkührlich") that Loew and I took this genus for the same as Exaireta Schiner. This statement, as well as those as the bottom of p. 14, are based upon a misconception. — Diphysa belongs in the number of those genera which Macquart, as he frequently did, established a priori, without seeing the specimens, merely upon the data suggested by Wiedemann. When in the course of time he came across specimens of such genera, he frequently did not recognize them and described them for a second time under a different name. To any one, accustomed to handle Macquart's writings, such instances are familiar. In the present case, the note in Wied. A. Z. II, 619 at the bottom, about *Xylophagus spiniger* and *rufipalpis* gave occasion to the creation of the genus Diphysa. As Wiedemann compared the venation of those two species to that of Beris (overlooking that Xyl. spiniger has five posterior cells, and Beris only four), Macquart attributes four posterior cells to his Diphysa. At the same time, and owing to the wrong statement about the venation, he did not recognize a specimen of Xyl. spiniger which had before him, and described it as a new species, Beris Servillei, on the same page with Diphysa, duly noticing that it is a Beris with the exceptional number of five posterior cells (Comp. Macq. D. E. I, 1, p. 172). Thus Diphysa became a purely maginary genus, based upon a mistake and not represented by any typical species. Later, as if not knowing what to do with Diphysa,

Macquart forced two heterogeneous species into it (Suppl. I and IV). For this reason, in my Catal. N. Am. Dipt. p. 44, I placed *Diphysa ex parte*, as a synonym to Exaireta. — Under such circumstances, the name Diphysa be better dropped, the more so as the name was preoccupied when Macquart used it (Acalepha 1834).

This history of Diphysa is explained at length in Nowicki's „Beitr. z. Kenntn. d. Dipt. Neuseelands 1875", only the author is not positive enough about the occasion of Macquart's blunder, which was, Wiedemann's erroneous comparison of the venation of his Xyl. spiniger (five post. cells) with that of Beris (four p. c.).

Thorasena (p. 10). This is a parallel case to that of Diphysa, only the developments are still more curious. Macquart (D. E. I, 177) established this genus a priori, merely on the strength of Wiedemann's data concerning Hermetia pectoralis Wied. A. Z. II, 26; but this time he went so far as to construct the figure of a fly which he had never seen (l. c. Tab. 21, f. 3 and 3a, the head). Any one who carefully compares the quoted passages and figures will perceive, that whatever Macquart gives us, is based on Wiedemann's statements. Thus „Stirn mitten vertieft" is translated „front enfoncé au milieu", the figure however represents a vertex deeply excavated between the eyes. Dr. B., with an eye upon the figure translates Macquart's french back into german thus (p. 10, No. 34):

„Scheitel tief eingesattelt, zwischen den Augen concav", which is rather remote from Wiedemann's: „Stirn mitten vertieft." I doubt whether Macquart or Dr. B. would have recognized Hermetia pectoralis W. in this imaginary *Thorasena*, with its Asilus-like vertex.

Pachystomus. I do not understand why this genus is treated as a distinct form (p. 3, line 16 from bottom; p. 17, No. 115; p. 30, line 9 from top) although it is stated on p. 32, line 2 from bottom that it is the same as Xyl. cinctus, and also explained p. 17, No. 115 that the genus was based on a specimen of Xylophagus cinctus with injured antennae? Nevertheless on p. 17 (No. 115) Pachystomus is introduced into the dichotomic table, as having three-jointed antennae! I will recall here that Mr. Bigot (Ann. S. E. 1879, p. 184) gave the same explanation of the origin of Pachystomus.

Artemita (p. 7, No. 10). Four scutellar spines are attributed to it, in agreement with Walker, List etc. V, 61, where two species are named as belonging here: Clitellaria Halala (Honduras) and Clitell. Amenides (sine patria). But in the description of Clitell. Amenides

Walker, List etc. III, 523, the scutellum is described as having only two spines. Nevertheless, on p. 30, Dr. B. calls Clitell. Amenides the type of the genus Artemita and gives it South America for patria. Now has C. Amenides two or four spines? And from what source did Dr. B. derive the information about the locality of that species?

Hermetia (p. 10, No. 36). The terminal lamel of the antennae is described as „keulen- oder spindelförmig, flach, bandartig" (flat, ribbonshaped), in contrast to the antenna of Lagenosoma, which is „dicht zweizeilig gefiedert" (with a dense fringe of hairs on each side). This description of the terminal portion of the antenna of Hermetia is not correct; it looks like a flat, ribbonshaped lamel, but a closer examination shows that it has the same structure as that of Lagenosoma; a central, more or less broad rib, with a vane of closely packed hair-like appendages on each side. This structure becomes especially apparent when we hold the antenna between ourselves and the light. My comparison of the antennae of Rosapha and Hermetia, quoted by Dr. B. in another passage (p. 8), is based upon this structure; but in Rosapha the feather is much less dense.

Massicyta (p. 10, No. 35). „Bei Massicyta soll der Hinterleib keulenförmig sein;" and on p. 25 „? Massicyta Wk., schlecht characterisirt." — In making these statements Dr. B. does not quote, and seems to have overlooked, the figure appended to M. Walker's description, which was drawn by Prof. Westwood and removes all doubt about the shape of the abdomen of Massicyta, as well as about the identity of that genus with Lagenosoma.

Engonia, *Thylacosoma* and *Lagenosoma* are merely new names for Negritomyia Bigot, Ruba Wk. and Massicyta Wk., as Dr. B. himself acknowledges (p. 20, 21, 25). I do not think that the publication of new generic names can be justified, when there is no doubt concerning the old ones. In this instance Negritomyia has been sufficiently characterized; Ruba is so peculiar, that it would have been difficult, even for a Walker, to make it unrecognizable. And if Dr. B. had any doubts about Massicyta, it was, as I have shown above, not the fault of Mr. Walker's.

Drasteria n. gen. (p. 22); *Compsosoma* n. gen. (p. 23). In publishing the new genera, found among Dr. Schiner's posthumous papers, Dr. B. should at least have verified whether the names are not

preoccupied. There is a Drasteria Hübn. Lepid., and Compsosoma Serville, Coleopt.

Engonia aurata n. sp. Amboina (p. 20). It is the same as Clitellaria festinans Walk. J. Pr. Lin. Soc. IV, 95 (Celebes). It is a striking, easily recognizable species, and should be called Negritomyia festinans Wk.

Ephippium spinigerum Dolesch. (p. 20) is quoted as as probable synonym of *E. maculipenne* Macq., on the strength of Doleschall's type-specimen in the Vienna Museum. — The specimens described by Doleschall were from Java; what Dr. B. takes for the type-specimen is, according to his own statement, from Amboina; it may be therefore an author's type, but it cannot be the type of Doleschall's description. If Dr. B. compares that specimen with the description, he will find that the specimen has the legs partly black, partly yellowish and that the spines of the scutellum are yellowish or reddish towards the tip. Doleshalls description speaks of altogether black legs and spines. The specimen in the Vienna Museum, although an author's type, is a wrongly named specimen. It is E. maculipenne Macq., originally described from the Philippine Islands, but also occurring in Amboina, the fauna of which is closely allied to that of the Philippines. — The true E. spinigerum Dol. (Java) is the same as *E. bilineatum* Fab. (bivittatum Wied.), as Mr. v. d. Wulp has shown (Sumatra Exp. 14). This is confirmed by the original colored drawing of Doleshall's, now in my possession, which is marked in his own handwriting: Ephippium spinigerum, Java; and in the same handwriting, but added later; Clitellaria bivittata Wied. — I will notice, by the way, that the *bivittata* in Wied. II, 46, is merely a *lapsus calami* for *bilineata.*

In another instance (Laphria tristis Dolesch.; in O. Sack. Enumer. etc. 41) I have drawn attention to a similar mistake, committed by Schiner, on the strength of these so-called types of Doleschall; they cannot be used without a previous comparison with the descriptions.

Cyanauges, Antissa. On p. 5, 15, 31 the genus *Antissa* Walker is declared a synonym of Cyanauges Philippi, on the strength of a typical specimen of Antissa cuprea Walk., which exists in the Vienna Museum and which „answers completely the generic characters of Cyanauges" („welches ganz auf die Gattungsbeschreibung von Cyanauges passt"), only the antennae are more obtuse, and the compound third joint not incrassate near the base. In virtue of this agreement, on p. 29, No. 78 Cyanauges and No. 79 Antissa (why these two num-

bers, as soon as the genera are synonyms?) are placed next to the Berinae, in the group VII *Transitoriae* Brauer, characterized by spurs on the middle tibiae. The same is repeated in the introductory chapter (p. 5): „Dagegen gehört *Antissa* Walker, synonym mit *Cyanauges* Philippi, entschieden in die Nähe der Berinen und ist durch die gespornten Mittelschienen sehr verwandt mit Acanthomyia." Finally, on p. 15 (No. 92) Cyanauges is more fully characterized, and I take Philippi's work in hand to compare the characters. I find for Brauer's „eyes hairy", oculi glabri; for „middle tibiae with a distinct apical spur", tibiae inermes; for „abdomen with five or six segments", abdomen quadriannulatum; for „scutellum with 10 to 12 spines", scutellum 6 vel potius 8 dentatum. This is what Dr. B. calls a complete agreement! Happening to know that Rondani described Cyanauges valdiviana independently, two years before Philippi, from a specimen sent by the latter, I compare his description (Archivio per la Zool. Vol. VII, fasc. I, Modena 1863) and find that, like Philippi, he describes the scutellum as having 8 spines and the eyes as glabrous. Now I turn to Walker's Antissa and find (List V, p. 63) scutellum spinis quatuor minimis and not ten or twelve, as Dr. B. has it. The question arises whether what Dr. B. calls Mr. Walker's type is a type of Ant. cuprea at all? From Mr. Walker even types must be received with caution; timeo et dona ferentem. Antissa cuprea Wk. is from Western Australia; Dr. B.'s type from Cape York, two regions separated by 20—25 degrees of latitude and about as many of longitude.

This is not all. From the statements in Dr. B.'s paper one would suppose that he knows Cyanauges merely from Dr. Philippi's description. And yet, as I open Dr. Schiner's Diptera of the Novara p. 54, I find that Dr. Schiner described Cyanauges ruficornis n. sp. male and female, the specimens of which exist, of course, in the Museum in Vienna. In a note, appended to his description, Dr. Schiner expresses his views on the genus in general. In agreement with Philippi, he says that the scutellum has 6 or 8 spines and the abdomen four segments. He adds that it may be related to Antissa Wk., but that the synonymy cannot be assumed, as M. Walker's description speaks of only four scutellar spines etc.

Has Dr. B. read this passage? Has he seen and compared those specimens?

Here are some minor corrections:

Cacosis (p. 30). The type of this genus is Sargus niger Wied. (comp. Wk. Ins. Saund. 83, Tab. III. f. 1) and not Sargus vespertilio

Wied. as Dr. Brauer gives it. The latter seems to be of a different genus, as Dr. Schiner (Novara, 67) refers it to Chrysochlora.

Euryneura Schin. (p. 11, No. 44). The typical species belongs to S. America, and not to Mexico, as Dr. B. has it. That a mexican species belongs in the same genus was merely a supposition of Schiner's (Novara, p. 56).

Exodonta Bellardi (p. 31) will not be found in that author's writings and should be quoted Exodonta Rondani; Bellardi merely proposed the name in litteris.

Acanthina (p. 7, line 7 from bottom). Besides Ceylon, this genus occurs in the Malay Archipelago and in the Philippine Islands. (Compare my Enumeration etc. p. 23).

Anacanthella Macq. (p. 29) is placed among the species with seven abdominal segments (Section VIII, Beridinae), while Macquart distinctly says that although allied to Beris („voisin"), the genus has only five abdominal segments.

Chlorisops Rondani (p. 16, 29, 30, 32) is borrowed from Schiner; Rondani has *Chorisops*.

For *Calcochaetis* Bigot (p. 8 and 30) read *Calochaetis* (Ann. S. E. 1877); Calcochaetis was a misprint in Ann. etc. 1879.

Chrysochlora (p. 9, 28 and 30) is Latreille, not Macquart;

Chloromyia (p. 31, No. 68) is not O. S., but Duncan, Mag. Zool. and Bot 1837.

Lophoteles (p. 32) „Insel Radak, Persischer Meerbusen". The island Radak belongs to the Marshall group in the Pacific, as is correctly stated on p. 9 (No. 21).

On p. 22, line 11 from bottom, for „Neu-Holland", read Island Tonga Tabu.

On p. 16, No. 101 I read: „*Exaireta Straznickii* and *analis* Nowicki from the Auckland Islands, not New-Zealand." No clue whatever is given concerning the reason for this correction, as Dr. Nowicki had these species from New Zealand. I venture the following supposition: a number of diptera, described by Dr. Schiner as from Auckland, New Zealand, are erroneously enumerated in Dr. Nowicki's

paper (pag. 5) as being from the Auckland Islands. A confused recollection of this fact may have induced Dr. Brauer to state exactly the reverse about a species to which his statement has no application [1]).

I close with some general remarks on the construction of Dr. B.'s dichotomic tables.

We have no right to quarrel with an author for giving us less than we expected; but we have a right to expect him to give us what he promises. The title of the paper, translated literally promises us: An attempt at a characterization of the genera of Notacantha. What the paper gives us is not a characterization, but merely a meagre dichotomic table of the genera. The characters used in that table are by no means the leading ones; on the contrary, subordinate characters often occupy the first place, and the important ones are not mentioned at all.

On p. 16 (No. 98) the difference between *Beris* and *Hadrestia* Thomson is stated as follows:

Scutellum with six spines *Beris*.

Scutellum with eight spines *Hadrestia*.

Now it is very well known that in Beris the number of spines on the scutelum is variable, even in the same species; for this reason Beris must be characterised as having not six, but from four to eight spines (comp. Loew, on Beris, Stett. Ent. Z. 1846, p. 219 or Schiner's Fauna Austr. I. p. 23, line 7 from bottom). The statement of the differential character between Beris and Hadrestia is thus reduced to nothing; at the same time a better character, which, judging from Dr. Thompson's description, exists in the antennae, is not noticed by Dr. B.

Again on the same pag. 16 the differential character between Beris and Actina may be reduced to this:

97 (100). Of the veins issuing from the discal and second basal cells, the first, second and fourth are present; the third is wanting or rudimentary etc.

100 (97). The four veins issuing from the discal and second basal cells are present; . . . . the third is often abbreviated, not reaching the margin.

The whole difference between there two sentences lies in the difference between the words „rudimentary" and „abbreviated". Any

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[1]) I find that since my writing the above, Prof. Mik has made the same criticism in the Wien. Ent. Zeit. July 1882.

one, aquainted with Actina tibialis knows that in this species the third vein, issuing from the discal cell, varies in length. In a specimen which I have before me, that vein is rudimentary on the left wing; on the right it is merely abbreviated and reaches the middle of the distance between the discal cell and the margin. A much more important and available difference between the two genera consists in the structure of the palpi, well developed in Actina and rudimentary in Beris; but this character is not noticed in Dr. B.'s paper.

In the same way the remarkable emargination of the eyes, a very striking character of Rhachicerus, is not mentioned at all.

I do not think therefore that the title of the paper: Characterization etc. is justified by its contents. — That the characters of Arthropeas and Rhachicerus, as introduced in the dichotomic table, are the reverse of what they actually are, has been shown above; also that the characters of Macroceromys, Hermetia, Exochostoma and Anacanthella are inaccurately described.

Another defect of Dr. B's table is that the genera known to the author by sight, are not clearly discriminated from those which he knows merely from descriptions. For instance he speaks as positively about Exochostoma and Thorasena, as if he had specimens in view; while in reality, as I have shown, he has not even read Macquart's descriptions and has merely borrowed his facts from that author's very inferior plates. Such defects, once discovered spread a haze of doubt over the whole paper, and render it unsatisfactory as a source of positive information.

A third defect is that references are almost altogether omitted. Papers of this kind are not written for the few who know all about the subject in question, but for the many who know little or nothing. For readers of the latter class Dr. B.'s paper will be full of puzzles, and they will uselessly consume a great deal of time in hunting for the references which the author should have given. For instance, why not add the references to the list of more than one hundred genera, placed at the end of the paper. (The list is otherwise complete and Hirtea Scop. is the only name I miss in it.) The frequent quotation of references, as I know from experience, is useful in more than one way; the author himself in verifying them, often finds occasion to rectify his statements. If Dr. B. had used this precaution he would have avoided good many of the mistakes which he committed.

I regret very much to have been obliged, in this instance, to criticize one for whom I have the profoundest esteem as a man, as well as a zealous and talented entomologist; that in adopting this course, I had no other aim in view but the interest of truth, I need scarcely add.

As a postscript I will add a remark concerning a passage not in Dr. B.'s Notacantha, but in his paper on the venation of the diptera (vergl. Untersuch. d. Flügelgeäders etc.) contained in the same fascicle.

Page 40, at bottom, it is said: „Lampromyia Macq. gehört durch ihre zwei Haftlappen und das Geäder wohl hierher zu den Asiliden und nicht zu den Leptiden, und zwar in die nächste Verwandtschaft zu Leptynoma sericea Westw. (Africa), Trans. Ent. Soc. Lond 1876." This inference is again a little too hasty. Soon after the appearance of Prof. Westwood's paper, he had the kindness to inform me, upon my inquiry, that Leptynoma has three pulvilli, and that, for this reason, it would be better placed among the Leptidae. Chaetotactic characters would perhaps prove decisive in this question. If those two genera are Asilidae, they would probably have some thoracic macrochetae; as Leptidae, they would have none. *Lampromyia* has none, and *Leptynoma* probably neither.

NOTES.

I. In speaking of Ruppelia and Bolbomyia (l. c. p. 18, top) and the doubts of Loew concerning the relationship of the latter to Thereva, Dr. B. says: „Es scheint der Zweifel über die systematische Stellung in der Unkenntniss über die Fühler der Thereviden zu liegen (siehe Mik, Verh. Z. B. Ges. 1881, p. 321." — In the quoted passage, Prof. Mik draws attention to the peculiar structure of the antennae of Thereva, which look 4-jointed, as a distinct suture cuts off the base of the 3^{d} joint. But a similar suture exists at the base of the third joint of many Bombylidae and, in my opinion, is one of the indications of the relationship of the two families. That Loew, the author of a monograph of the Therevidae, should have been ignorant of this character is somewhat improbable! — I inquired for Ruppelia in the Museum at Frankfort; unfortunately the type does not exist any more in the collection.

II. Even Loew was struck by the resemblance of Arthropeas sibirica to an Atherix („nach Färbung und Form fast wie zur Verwandtschaft von Atherix ibis gehörig"; Stett. Ent. Z. 1850, p. 304); but the structure of the antennae decided him to place Arthropeas among the Notacantha! At the time when Glutops was discovered, my talented friend Mr. E. Burgess in Boston pointed out its relationship to Arthropeas. Although fully accepting this view, I said in a letter to him „that I felt inclined to subordinate the structure of the antennae of Arthropeas to its general habitus, which is undoubtedly that of a Leptid; as there was no more reason for regarding this organ as so

constant in the Leptidae, than in the Xylophagidae, Stratiomyidae etc." (Compare Burgess on Glutops in the Proc. Boston Soc. N. H. 1878, p. 321.) — In my Catalogue, I left Arthropeas in the place which Loew had assigned to it, because I did not feel prepared to introduce the reform I am proposing now. And thus an inconsistency arose with regard to Glutops, which I placed at the end of the Leptidae, quite far from Arthropeas.

III. Latreille, in defining his Xylophagus, had principally *Subula*, and especially *S. maculata* in view; he figures it, and mentions its habits (Genera Crust. et Ins. IV, p. 272; Tab. XVI, f. 9, 10). The true Xylophagi he had either not studied attentively, or, still more probably he may have known them from descriptions and figures only. A close scrutiny of Latreille's wording is, in this respect, convincing. Compare, l. c., the defination of his Xylophagus: Antennae ad apicem attenuato-acuminatae etc. agrees with Subula maculata, and not with Xylophagus, not even with Subula varia. — Palpi articulo primo manifesto crassiore; cellula stigmosa non fuscata; both characters are not applicable to our Xylophagus. The only reference to the true Xylophagus I find in the description of the venation, because here he had figures to compare. In speaking of the posterior cells, he says imperfectae, aut illarum tertia solum, *Xyl. maculatus*, conclusa. In quoting the figures of Xylophagus cinctus, especially Schellenberg's he is very naturally struck by its resemblance to his Pachystomus (Rhagio) syrphoides Panzer („Rhagioni syrphoides dom. Panzer prima fronte, simillimus!"). He would certainly not have spoken in that way of a figure, if he had had a specimen to compare; and in such a case it would not have escaped his keen eye that his Pachystomus is nothing but a Xylophagus with brooken antennae!

Observe that in the Genera (1809) Latreille places Pachystomus among the Leptidae; in the Familles naturelles he forms for Pachystomus and Coenomyia the family *Sicarii*, which he places near the Leptidae; and this is the exactly the arrangement which I am proposing to restore.

IV. I would not express a final opinion on the other genera of doubtful position belonging to the same groups, without again having examined specimens; they are beyond my reach at present. What I have to say now is merely based on recollections.

Heterostomus, *Rhachicerus*, *Electra*, *Chrysothemis* will probably remain among the Xylophagidae.

Coenura Bigot, according to Dr. B. belongs to the Pangonina (l. c. p. 4). This same opinion was expressed by Dr. Gerstaecker at the time of the publication of Coenura (Entomol. Bericht 1857, p. 203). It may be correct; but the telescope-like elongation of the abdomen of the female is not a character of the Pangonina. Dr. Schiner who could compare specimens of Coenura said that the relationship with Arthropeas could hardly be called in doubt (Novara 75). I felt inclined to follow this view, when I saw Coenura in Mr. Bigot's collection; hence my opinion, as expressed in the Catal. N. Am. Dipt. Note 48. I am much less positive now, and would leave Coenura among the Xylophagidae. Nevertheless I would observe that the statement of Philippi about the motions of Coenura (hovering, suddenly flying away, and returning to the same spot; Verh. Z. B. Ges. 1865, p. 726) calls to mind the behaviour of a Tabanid, rather than the slow motions of a Coenomyia. At any rate, I agree with Dr. B. in removing Coenura from among the Notacantha.

Hylorus and *Lagarus* belong in the same group with Chiromyza (as I have already shown above). But whether Loew (Monogr. N. Am. Dipt. I, 17) and Dr. B. (l. c. p. 29) are right in placing them in the vicinity of the Beridina, I do not know. Latreille (Fam. Nat.) has Chiromyza among his Sicarii (which answer my Xylophagidae); the extensile abdomen of the female of Chiromyza favors that view. On the other hand, the venation (veins crowded towards the anterior margin, short praefurca etc.) is more like that of the Stratiomyidae. Chiromyza may be a synthetic type of a high order, intermediate between Tanystoma and Notacantha. This would explain its simultaneous occurence in Australia and S. America. Similar forms of transition *Tanyderus* (Tipulidae) and *Apiocera* (Asilidae) also occur in those two continents.

Although I discovered Bolbomyia in the United States many years ago (see Loew, Cent. II, 5) I do not remember it and have nothing to say about it.

About the Acanthomeridae I agree with Dr. Brauer, that they are more related to the Tabanidae and Leptidae, than to the Stratiomyidae.