To Professor J. M. Aldrich,
with the sincere regards of the author
Heidelberg, April 10 1904.

C. F. Oster-Sackau.

MY LIFE WORK
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RECORD OF MY LIFE WORK IN ENTOMOLOGY

^{Carl Robert}
C. R. OSTEN SACKEN
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A. L. MELANDER
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The fossil Diptera enclosed in the lumps of Amber abundantly found on the Baltic coast of Prussia have always been one of the favorite objects of Loew's study. His first essay: "Ueber den Bernstein und die Bernsteinfauna," Berlin, 1850, was but an imperfect attempt to bring these fragments of an extinct fauna into agreement with the systematic distribution of recent Diptera. His lecture, "Ueber die Dipteren-Fauna des Bernsteins," 1860, an English translation of which was published by me in Silliman's Journal in 1864, contains an abstract of results, and very interesting generalizations on the same subject. The dream of his life was the publication of a general work on Amber Diptera which he had conceived on a large scale, for which he had drawn a number of plates, and must have prepared a great deal of letter-press. This intended publication has never taken place. My long intercourse with Loew has enabled me, in the above-quoted chapter, "to throw some light on this otherwise very obscure subject."

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This "Special Report" is likewise alluded to in my "Introduction" (p. 8 in the middle). It contains an account of the state in which I found the collection in Guben, and of what I have done in preparing it for its transfer to the United States; it provides

besides a multitude of miscellaneous notes on the localities of the specimens, on the meaning of the labels and etiquettes of different shape and color occurring in it, and on the collectors who have contributed these specimens.

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until his Death in 1879 96

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- XV Prof. Fr. Hermann Loew. An Obituary Notice by C. St.
[Carus Sterne] 99

Translated by me from the *Deutsche Entomologische Zeitschrift* of 1879. *Carus Sterne* is a pseudonym of the well-known author Dr. Ernst Krause, a scholar of Loew, and later his political friend.

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In my "Introduction" (p. 21) I have referred to this Chapter in the following terms: "After twenty-nine years of relations with him, I possess in his voluminous correspondence a source of information which enables me to accomplish this task better than anybody else, and I am convinced that I have fulfilled it with impartiality." To this Chapter a full-length portrait of Loew is appended (the first ever published).

- XVII Philipp C. Zeller as a Dipterologist 137

"In Zeller, the lepidopterist, the ideal of a perfect dipterologist was lost," is the motto of this Chapter. Zeller was, after Rob. Desvoidy, one of the few who put in practice the notion that dipterology is the science, not of *pinned* Diptera only, but also of Diptera *in life*. The two articles on Diptera by Zeller in Oken's *Isis*, 1840 and 1842, filling one hundred and thirteen quarto double-column pages, "are perhaps the most delightful reading on Diptera in existence." The most attractive department in these papers is the *biological*, "consisting in accounts of the habits and of the demeanor of flies," "noticing peculiarities in the behavior of almost every species," etc. Such is the well-deserved praise which I have bestowed upon Zeller. And yet his publication on Diptera, buried as it is in the ponderous volumes of Oken's *Isis* "of sixty years ago, has remained almost unknown to the majority of dipterologists."

My Chapter on Zeller, besides a short biographical notice, contains an account of the beneficial influence he had exercised

upon Loew during the eight years (1860-1868) when they were colleagues in the Gymnasium at Meseritz, an account principally based on extracts from Loew's letters to me.

XVIII Notice of Johann Wilhelm Zetterstedt (1785-1874) . . 142

Among the many services rendered to dipterology by Zeller, a special mention must be made of the faithful assistance he afforded to Zetterstedt during his long and persevering career, culminating in the publication of *fourteen* volumes on Scandinavian dipterology. The dedication which Zetterstedt placed at the head of the *first* volume of this work was a well-deserved compliment to Zeller: "Zellero, inter Lepidopterologos et Dipterologos Europae magni, spectati et cari nominis viro!"

Loew had always treated Zetterstedt with studied disregard, and had declined all correspondence with him. Zetterstedt gave a proof of his magnanimity when in 1860, at the age of seventy-five, as if in forgiveness of the past, he dedicated his fourteenth and last volume to Loew, in the following terms: "Viro amplissimo, Doctori H. Loew, etc. Dipterologo nostri aevi celeberrimo, hunc sui operis tomum dicavit J. W. Zetterstedt."

XIX Camillo Rondani and his Relations with Loew . . . 144

This Chapter is based on my own impressions during my meeting with Rondani in Parma (in 1873), on the materials I gathered in the existing literature, on my correspondence with Loew, and on *three letters of Rondani* to Loew, written in 1846, which opened their correspondence, and which were found among Loew's papers. This Chapter offers a new instance of the "*sans façons*" manner of Loew in his relations with his dipterological colleagues. At the same time it shows that Rondani, as early as 1845, had called attention to the difference between *macrochaetae* and ordinary bristles, and had introduced this new term for them. Although, in his later publications, he did not give any further development to this suggestion, it became the germ of the method of *Chaetotaxy* which plays at present such an important rôle in dipterology. Loew, to whom Rondani had sent his paper of 1845, did not appreciate its contents, and began two years later the publication of his "European Asilidae" (1847-1849) without making any use of the chaetotactic characters which, nevertheless, are indispensable for the proper classification of this family.

XX Notes on Chaetotaxy, complementary to my previous Publications on the same Subject . . . 153

These notes contain an account of the origin and progress of Chaetotaxy, and a proposal for its improvement.

XXI An Account of the Breach between Loew and Schiner . 158

As the estrangement between these two leading dipterologists has had some importance in the history of dipterology, I have

in this Chapter given a detailed account of its origin and progress. In my "Introduction" (p. 20) there is an allusion to this very regrettable dissension.

XXII	Brauer and Mik	164
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Both Brauer and Mik have been on the best terms with me until the publication of my article: "On Prof. Brauer's paper: Versuch einer Charakteristik der Gattungen der Notacanthen" (*Berl. Ent. Zeit.*, 1882), which provoked Brauer's ire. After this publication Mik found himself in an embarrassing position between Brauer and me. He wished of course to remain on good terms with the very sensitive Custodian of the Entomological Museum of the Imperial and Royal Academy of Sciences in Vienna, but at the same time, in his views, he was in most cases in accordance with me. In his capacity of a reviewer and critic of dipterological publications Mik began to sustain most energetically Brauer's innovations in the classification of Diptera. As I fully understood Mik's perplexities, I bore these indirect criticisms against me with perfect composure. When my turn came to criticize Mik rather severely (*Berl. Ent. Zeit.*, 1896, p. 323), he took it in good part, and we remained friends up to his premature death. The purpose of the Chapter "Brauer and Mik" is to point out the incongruities in Mik's critical notices in the *Wien. Ent. Monats.* which arose from this, for him, awkward situation. At the same time this Chapter contains a detailed characterization of both of these dipterologists and of their work.

XXIII	Notice on the Circumstances of the Publication of two principal Works (1830, 1863) of Robineau-Desvoidy	180
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My "Introduction" (p. 16, line 12 from bottom) contains an allusion to an almost forgotten document of which I had made use while inquiring into the circumstances of R.-Desvoidy's first publication (in 1830): it is the *Report* upon it, drawn up by the Committee of the Academy of Sciences in Paris in 1826, and which authorized its publication. The *second* of R.-Desvoidy's works (dated 1863), having been published after his death by persons not familiar with the subject, contains some errors which it was necessary to point out in order to make the work serviceable in future.

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Part Third of this "Record" contains the List of my entomological publications. It is already prepared in manuscript, and will be published as soon as possible.

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PART FIRST

INTRODUCTION

THIS "Introduction" was first printed as a separate pamphlet by the Cambridge (England) University Press, and was dated January 30, 1901. This *second* edition is a reprint of the first, and the principal change introduced consists in the mode of reference to the *Chapters* on various subjects which, instead of being distributed among the items of the "List" of my publications (an arrangement that I had announced in the Prospectus of my "Record" published in June, 1901), appear now *collected* in *Part II*. The reason for this change will be found explained in the Preface of that Part.

The items of the "List" of my publications, which will form *Part Third* of this "Record," are generally referred to in Parts I and II by the mere quotation of the *number and date* of the "List," which are printed in *heavy-faced type*. By means of this arrangement the papers required will be easily found, and the cumbersome repetition of their headings avoided.

My entomological career may be divided into *three* distinct periods, dependent on my places of residence during each of them.

I WORK IN ST. PETERSBURG (1854-1856)

Born in St. Petersburg on the twenty-first of August, 1828, I began at the early age of eleven to take an interest in entomology. It was during a temporary residence with my mother in Baden-Baden (1838-1839) that I met a young Russian, Mr. Schatilloff, who gave me my first instruction in collecting Coleoptera.

Joseph Nikolaievitch Schatilloff (1824-1890), a Russian nobleman, four years older than myself, had been initiated into the study of entomology by the Italian naturalist and traveller *Gaetano Osculati*, in whose villa near Monza in Lombardy he and his father spent the autumn of 1838. (The Diptera collected by Osculati in South America have been described by Rondani in the *Nuov. Ann. Sci. Nat.*, Bologna, 1850.) Schatilloff devoted himself later to general zoölogy, and especially

to ornithology, following a most honorable career of many-sided usefulness, principally in Southern Russia. His biography and portrait were published in the Collection of Biographies of Russian Naturalists, edited by Prof. A. Bogdanoff, at the expense of the *Imperial Moscow Society of Natural Sciences*, in three quarto volumes (1888-1891), with 38 plates containing 342 portraits. Schatilloff's portrait is in Vol. II, Plate VII; his biography is in Vol. II.

Later on, in St. Petersburg, where I received my education and entered (in 1849) into the service of the Imperial Foreign Office, I made collections in all orders of insects except *Lepidoptera*. The results of this work of mine, so far as published, are represented by the first three numbers of my "List," namely:—

1 (1854). An article in the *Stett. Ent. Zeit.* in which I set forth some ideas on a new classification of the *Tipulidae*, which ideas I carried out in later years (1859, 1868, etc.).

2 (1857). Another paper in the *Stett. Ent. Zeit.* refers to my discovery, in the environs of St. Petersburg, of the long-lost and misunderstood *Tipula (Trochobola) annulata* Linné.

3 (1857). A pamphlet of one hundred and sixty-six pages in Russian, which I had left in manuscript when I departed for the United States in 1856, and which for that reason was published during my absence. This work contains a general survey of the insect fauna of the environs of St. Petersburg so far as known at that time, with a *List of species* of the different orders (except *Lepidoptera*), geographical and historical accounts, etc. This publication brought to a natural conclusion the *first period* of my entomological career.

In 1856 I was appointed Secretary of Legation in Washington, and started for my destination in the first days of April of that year. During my journey, which lasted two months, I made the acquaintance of the principal entomologists and zoölogists in the cities I visited. At Königsberg, Prussia, I met Dr. H. A. Hagen, and formed an acquaintance which ripened later into a lifelong friendship, and became of great importance for American entomology. (A notice of Dr. Hagen will be found in Part II, Chapter XI.) Some of the German entomologists—Klug and Ruthe (Berlin), Kiesenwetter (Dresden), Schiner, Brauer, Kolenati, etc. (Vienna), C. A. Dohrn (Stettin)—I had already met during

an earlier journey in 1852-1853. In Berlin I had a long and interesting interview with *Alexander von Humboldt* in his house, but our conversation had little to do with entomology. Although eighty-seven years old at that time, and much bent by age, he was remarkably active, both physically and mentally.

In the course of my further journey, I visited *Winnertz* at Crefeld (with whom I had corresponded before), *Baron de Selys Longchamps*, *Candèze*, and others in Belgium, *van der Wulp*, *Snellen van Vollenhoven*, etc., in Holland. In London I made (and in part renewed) acquaintance with the principal entomologists, some of whom I remembered having met before during a visit to England in 1852 (*Westwood*, *Stainton*, *Francis Walker*, *Adam White*, etc.). In the beginning of June I embarked at Liverpool on the steamer "Arabia," and landed in New York after a passage of thirteen days.

II WORK IN THE UNITED STATES (1856-1877)

The *second* period of my entomological career embraces the *twenty-one years* of my residence in the United States (1856-1877), during which, until 1862, I was Secretary of the Russian Legation in Washington; in that year I was appointed Consul General of Russia in New York, which thus became my residence between 1862 and 1871. I resigned my post in 1871 and made several journeys to Europe and back, until, in the autumn of 1873 (this time as a private citizen), I again settled in the United States, where I remained till 1877. These twenty-one years were, as regards entomology, principally devoted, in collaboration with Dr. *H. Loew*, to the task of working up the *Diptera of North America* north of the Isthmus of Panama. A great deal of my time, as will be seen, was spent in acting as a purveyor of material for Loew to work upon, and as a translator and editor of his manuscripts.

Hermann Loew, born (1807) at Weissenfels, Prussia, was Professor in Posen about 1840, Director of the "Realschule" in Meseritz (Posen) from 1850 to May, 1863, and finally lived in retirement at Guben (Saxon Prussia) from 1868 up to the year of his death. A translation of a sympathetic biography of Loew, by his political friend Dr. *Ernst Krause* (known in literature under the pseudonym of

Carus Sterne), will be found in the present "Record," Part II, Chapter XV. In Chapter XVI I have given a *Characterization of Loew as a Dipterologist*.

A complete list of Loew's publications on the dipterous fauna of America will be found in my "Catalogue of North American Diptera," edit. 2, 1878, p. xxxiv-xxxvi.

Dr. H. Loew, with whom I had had a short correspondence previously (about 1850-1851) concerning *Trochobola annulata* Linné, which was soon interrupted, showed a great eagerness to renew it when he heard of my appointment to the United States. I readily fell in with his wishes, in the hope of utilizing his well-known working-power in the interest of American dipterology. After a rather long, *quasi*-tentative correspondence, during the first years of my residence in America, about the mode and conditions of our proposed co-operation, we came to the following agreement: I promised to send to Loew as much material as I could, on the condition, however, that he should consider the collection thus gradually accumulating in his hands not as his property, but *as a trust*. My purpose was, by this means, to form a collection of North American Diptera containing the type-specimens described by Loew, as well as specimens determined by him from earlier authors, and, as the case might be, an abundant supply of as yet undescribed and undetermined specimens. Such a collection I expected, sooner or later, to be brought back to the United States, in order to form a solid foundation for the further study of the American fauna. This scheme enabled me to receive without stint the numerous contributions in collections and specimens which were, *most generously*, put at my disposal by different collectors during my long residence in the United States. As will be shown in the sequel, this scheme came to a successful conclusion, principally in consequence of the generous intervention of Prof. Louis Agassiz. In 1877 this collection, containing (according to Loew's estimation) about 1350 species described by himself, 330 species described by earlier authors, and a large number of undescribed species, forming a total of about 3000 species, came back to the United States and was safely housed in the *Museum of Comparative Zoölogy* in Cambridge, Mass. At the same time Loew received from the Museum a liberal remuneration for his work on the col-

lection. (A more detailed account of the whole transaction will be found in Part II, Chapter XII.)

This is not all. During my long residence in the United States I had formed a collection of Diptera for *my own use*. It contained:—

(1) All the types of the new species described in my own monographs, and in my other papers of this period (*Tipulidae brevipalpi*, *Tabanidae*, *Cecidomyiidae*, *Western Diptera*, etc.).

(2) Numerous *duplicates*, many of them *numbered*, of the species I had sent to Loew for the purpose of having them described. Such specimens, not having crossed the ocean *twice*, and not having been so much handled as the *typical* specimens of the collection in Loew's hands, were for these reasons in a much better state of preservation. Of this *my own* collection I made *a present* to the same Museum in Cambridge, Mass., as a token of my gratitude to Professor Agassiz for his generous assistance in the realization of my principal scheme. During the two winters which I spent in Cambridge, Mass. (1873–1875), I incorporated in this collection the dipterological materials accumulated in the Museum before 1873, principally by Mr. P. R. Uhler, and thus formed a collection which is now kept separate from the other collection, and which affords a useful supplement to it.¹ A considerable collection which I had formed of *deformations* of plants (as galls of *Cynipidae*, *Cecidomyiidae*, *Trypetidae*, etc.), fungoid excrescences, and also vegetable hypertrophies, was included in this gift.

Thus far, for clearness' sake, I have anticipated events, and now I return to June, 1856 (the date of my landing in New York), for the purpose of giving a more circumstantial account of *my* share in the common work, between Loew and myself, on the dipterous fauna of North America. My first task was the compilation of a *Catalogue of all the previously described species of the Diptera of North America*. It was accepted for publication by the *Smithsonian Institution*, and was the *third* of the long series of entomological works since published by that Institution (compare my "List," 4, 1858).

¹ On this collection I made a detailed *Report* to the Trustees of the Museum. Compare 44 (1875) of my "List."

The *first* publication of the Smithsonian Institution on the subject of entomology was Prof. *Louis Agassiz's* article: "On the Classification of Insects from Embryological Data" (*Smiths. Contributions to Knowledge*, Vol. II, 1851, 28 pages). The *second* publication of the same class was Dr. *F. E. Melsheimer's* "Catalogue of the Coleoptera of the United States," revised by S. S. Haldeman and J. L. Leconte. (Smiths. Instit. 1853.)

Twenty years later, and almost simultaneously with the installation of the two above-mentioned typical collections of Diptera in the Museum in Cambridge, Mass., my entomological work in the United States was concluded by the publication of a *second*, this time *critical* Catalogue, likewise published by the Smithsonian Institution, representing the state of North American dipterology at that time, and showing the progress achieved (see my "List," 61, 1878). This *second* Catalogue has the advantage of being represented by the two type-collections deposited in the Museum of Cambridge, Mass.

Eighty-four years ago, Wiedemann, in his *Zoologisches Magazin*, 1817, Preface, p. 3, said: "It was an excellent idea of the highly respected Count Hoffmannsegg to found a *normal museum* of natural history, in which, as much as possible, every species, properly authenticated by the severest test of criticism, should be deposited as original *type*, and made accessible to any worker for the comparison or identification of species, doubtful or supposed to be new." (I have referred to this passage before, in my Catalogue, edit. 2, 1878, Preface, p. viii.) The *Museum of Comparative Zoölogy* in Cambridge, Mass., and the *National Museum* in Washington, D. C., have, at present, fully realized this idea of Hoffmannsegg.

In consequence of my arrangement with Loew, as I have said above, my principal efforts after 1856 consisted in procuring him materials to describe, and in translating and editing his manuscripts. It required some abnegation on my part to impose upon myself this laborious (and comparatively subordinate) part of a middle-man, occupied as I otherwise was with my official and social duties.¹ Ever since the beginning of my entomological career I have felt a decided preference for observations on the living subject, for questions connected with classification, and for the study of entomological literature. And I always had a marked repugnance

¹ About my *social* duties I shall merely state that, in Washington, they were necessarily connected with my diplomatic position, and that in New York my visiting-list contained over *one hundred* houses where I had been invited to dine.

for merely descriptive work, and submitted to it (*à mon corps défendant*) only when it was unavoidable. Thus, in consequence of my agreement with Loew, I could hope to obtain excellent descriptions of the collections brought together by me, and, at the same time, pursue my own favorite studies independently. For the latter, I had reserved for myself the *Tipulidae brevipalpi*, and later, the *Tabanidae*. Both families have been treated monographically by me.

Besides my principal works (my two *Catalogues*, the papers on *Tipulidae*, *Tabanidae*, *Cecidomyiidae*, *Cynipidae*, the *Western Diptera*, and the translating and editing of the three volumes of Loew's *Monographs*) I wrote, during my residence in the United States, a considerable number of papers on other entomological subjects. All these are enumerated in my "List."

I also attempted to be useful in corresponding with American entomologists, and in naming Diptera for them. As a *memento* of this correspondence, I have before me 617 letters received from 99 American correspondents between 1856 and 1872.

It would have been a grateful task to me, and an interesting contribution to the history of the development of entomological studies in the United States and Canada, to prepare an account of this correspondence and to recall the names of many worthy observers of nature (often unknown to fame) who took part in it. Such an account would have been foreign to the immediate purpose of the present publication, and would have occupied too much space in it, but circumstances permitting, I may, some day, devote a separate publication to it. [This was written before 1898; since then my "circumstances" have made it impossible to realize this project.]

During the winter of 1857-1858 I visited Cuba (where I spent five weeks) and returned by way of New Orleans, Montgomery, Ala., Savannah, and Florida. Although the season was not the best for collecting, I brought many specimens home, especially from Florida. In Havana I made the acquaintance of the excellent Prof. *Felipe Poey*, who at that time already looked old, though he lived for thirty-five years longer. It must have been on my way home to Washington that I paid a visit in Columbia, S. C. (the exact date I do not remember), to *Christoph Zimmermann*, a German coleopterist, whose name may be found in Hagen's *Bibliotheca* as author of papers on *Zabrus*, *Amara*, etc. (1830-1832). He most positively

refused to have anything to do with Diptera, although he might have been very useful to me by contributing specimens from South Carolina.¹

As I said above, in 1871 I resigned my post of Consul General of Russia in New York, and remained a part of the following two years in Europe. In September, 1873, I returned to the United States, and spent the interval between this year and 1875 principally in working at the Diptera of the Museum of Comparative Zoölogy in Cambridge, Mass. (where I had deposited my collections during my absence), and in preparing materials for the projected new edition of my Catalogue of North American Diptera. Between December, 1875, and September, 1876, I made a journey to California, the Sierra Nevada, and the Rocky Mountains, whence I brought back considerable collections. A portion of the collection of Californian Diptera was worked up by me in my *Western Diptera*, 53 (1877), after which publication I bade farewell (and this time for good) to the United States, and sailed in June for Europe.

One of the first duties I had to fulfil, after my return to Europe, was to go to Guben, the residence of Loew, and to secure the shipment to the United States of the large collection of North American Diptera which, for the last twenty years, had been accumulating in his hands; an arduous and risky business for me to undertake, but one which was nevertheless successfully accomplished. The whole collection arrived safely at Boston (in the autumn of 1877), and, as I have related above, was deposited in the Museum in Cambridge, Mass. With this transfer, and the almost simultaneous publication of the *second* edition of my Catalogue of North American Diptera (1878), the principal object of my entomological labor in the United States was fulfilled. An outline of the transactions which have led to these results has been given by me in this "Introduction"; a detailed account will be found in this "Record," Part II, Chapter XII.

The two men who principally influenced the American period of my entomological career were *Loew*, and *S. F. Baird* of the

¹ In Loew's "Centuries" some species are marked: *Carolina* (Zimmermann). The specimens must have been found by Loew in other collections and not among the materials which he received from me.

Smithsonian Institution. A considerable portion of my "Record" of this period has, for this reason, been devoted to *Loew*, and it remains for me to pay a tribute of heartfelt gratitude and admiration to *Baird*, to whose encouragement, support, and example I owe a considerable share of my success. I have often thought that if I were asked to name the man who, in my opinion, was morally nearest to *perfection*, I should unhesitatingly name *Spencer F. Baird* (1823-1887). I was therefore glad to find in the volume of the History of the First Half Century of the Smithsonian Institution (Washington, 1897) a biography of Baird (p. 157-200) by G. B. Goode, which proves that the very exalted opinion I had of him was fully shared by many distinguished men who were nearest to him. In this work (p. 157) he is characterized as follows: —

"He was one of those rare men, perhaps more frequently met with in the New World than elsewhere, who give the impression of being able to succeed in whatever they undertake. Although he chose to be a naturalist, and of necessity became an administrator, no one who knew him could doubt that he would have been equally eminent as a lawyer, physician, mechanic, historian, business man, soldier, or statesman."

The secret of Baird's influence lay in the immediate effect of his character upon those who came in contact with him; in his force of will, his rare simplicity and directness of manner, combined with the utmost kindness and evident rectitude of purpose, to which were added an extraordinary executive capacity and the tact of selecting proper men for the work he intended for them.¹

III WORK IN EUROPE (PRINCIPALLY IN HEIDELBERG) SINCE 1877

For my European residence I selected Heidelberg, where I made up my mind to spend the rest of my life. As I had left *all* my entomological collections in the United States, I made the acquisition of the remains of Professor Zeller's collection of European Diptera. Celebrated as a specialist in Microlepidoptera, *Zeller* had

¹ An excellent obituary notice of Professor Baird was published in the *New York Nation*, 1887, No. 1170.

also done some excellent work in Diptera (compare my notice about *Zeller as a Dipterologist*, Part II, Chapter XVII). This collection afforded me the necessary material in specimens for study and comparison.

While in the United States, my principal task had consisted, as I have shown, in working up, in collaboration with Loew, the Diptera of the North American fauna. Nevertheless, my favorite study was always in the direction of *classification*. My very first publication, in 1854, foreshadowed the improved arrangement of the *Tipulidae*, which I more or less brought to maturity (1859–1869) before leaving America. Once settled in Europe, I considered my task on North American Diptera as concluded, and attempted to extend my horizon by the study of Asiatic, African, and Australian forms. Preparatory to these new studies, I began by compiling a *Catalogue of all the previously described Diptera*¹ from all parts of the world, excluding Europe (together with Siberia and Central Asia, which I considered as belonging, zoologically, to Europe) and North America north of Panama. This task took me several months; but, once done, it proved an immense resource for my future descriptive work. I published descriptions of Diptera from the *Malay Archipelago*, from *New Guinea*, from the *Philippine Islands*, and from *Central America*, 77 (1881–1882), 83 (1882), 111 (1886–1887). I also developed my classification of the *Tipulidae*, by describing new forms peculiar to recently explored regions, 115 (1886) and 118 (1887).

Although in preparing these publications I acquired a good deal of morphological knowledge, I soon became aware that, owing to the very imperfect state of the classification in most of the families of Diptera, and to the old routine methods of description, this kind of promiscuous descriptive work did not satisfy my entomological conscience. As I expressed it (*Biol. Centr.-Amer., Diptera*, 111 (1886), p. 75): "The mere describer of a limited collection who attempts at the same time to improve the classification may be compared to a traveller who, having his own road to make, finds

¹ As this *Catalogue* is incomplete, and was prepared merely for my own use, it was never intended for publication, and remains in manuscript. Nevertheless, it may still prove useful in future for controlling an independent publication of the same kind.

his progress slow and his road bad." This feeling of dissatisfaction led me to look out for new methods of description, based upon the discovery of new descriptive characters, and thus I hit upon my *System of Chaetotaxy*, 80 (1881), 102 (1884), the usefulness of which has been fully recognized since.

The same intention, to introduce new descriptive characters, led me to publish my papers: *Suggestions*, etc., 128 (1891), *On the Characters of the three Divisions*, etc., 130 (1892), and *On the atavic Index-Characters*, etc., 133 (1894). The second, and principal of these papers, *Berl. Ent. Zeit.*, 1892: *On the Characters of the three Divisions of Diptera: Nemocera vera, Nemocera anomala, and Eremochaeta*, contains two important innovations:—

(1) The attempts of Loew, Brauer, and others to obliterate the distance separating the *Nemocera* from the *Brachycera*, as indicated by Latreille (Macquart merely supplied the name for the second group), by means of what they took for *transitional forms*, are proved to rest upon a fallacy. "*No transitional forms have, as yet, been discovered between these groups, either in the living or in the fossil faunas. We do not yet know a single dipteran, the position of which, between the two groups, can be called in doubt*" (*loc. cit.*, p. 421).¹ For this reason, in order to maintain the separation between the *Nemocera* and *Brachycera*, instead of the two suborders of Diptera proposed by Brauer (*Orthorrhapha* and *Cyclorrhapha*) I introduced three: *Orthorrhapha Nemocera*, *Orthorrhapha Brachycera*, and *Cyclorrhapha Athericera*, thus combining the nomenclature of Latreille derived from the antennae, with that of Brauer derived from the mode of transformation.

This step of mine was not exactly an *innovation*; it was rather a *restoration*. After the lapse of a century since Latreille's earliest publications, the classification of the Diptera not only had not advanced in its principal feature, but had run the risk of a revolution which would have driven it back behind Latreille's position. *The prevention of such a retrogression is perhaps the principal service I rendered to dipterology in my work on its classification.*

¹ Transitional forms *must* of course have existed at some previous geological age but such forms have not yet been discovered.

In my "Historical and critical remarks concerning Loew's first volume of the 'Monographs of North American Diptera,' etc.," in this "Record," Part II, Chapter VII, I have reproduced a passage of his letter to me, dated July 25, 1860, in which he formally expressed his opinion that the *coalescence* of Nemocera and Brachycera was an accomplished fact, and that he would have introduced this innovation in the new Catalogue of North America Diptera, if he had been the author of it!

(2) The other innovation, introduced by me in my 130 (1892), was to show that, within the three suborders, the existing *families* of Diptera could be easily arranged into larger groups, which I called *Divisions*, but which, since the appearance of Prof. J. H. Comstock's Manual, etc. (1895), I prefer with him to call *Superfamilies*. I called attention to characters which the families belonging to such superfamilies have in common; characters of which most had heretofore been entirely overlooked or neglected. The importance, for instance, of the character derived from the structure of the *head* in the male, which I called *holoptic*, had never been sufficiently appreciated before, so much so that there was no special term even to designate this structure. The *total absence* of this character in my superfamily *Nemocera vera*, combined with several other characters borrowed from different parts of the body, as well as from the early stages, justifies the separation of this Division from the superfamily *Nemocera anomala*, so called because I placed in it a number of anomalous, ancestral forms, not having any apparent connection between them. At the same time, there is no doubt that both superfamilies belong to the same suborder *Orthorrhapha Nemocera*.

Within the suborder *Orthorrhapha Brachycera* I have formed, for the families *Stratiomyidae*, *Tabanidae*, *Acanthomeridae*, and *Leptidae* (including the *Xylophagidae*), the superfamily *Eremochaeta*, characterized by the predominance of holoptic heads in the male sex, and by the remarkable instability in the structure of the antennae, a character which I have called "*morphological restlessness*." A conspicuous peculiarity of this superfamily is the *total absence of macrochaetae*, from which I have derived its name. Other characters are taken from the legs, provided, in most cases, with *three pulvilli*; from the wings, which in most cases have *five* posterior cells; and from the shape of the more or less developed

thoracic squamae, which do not exist in the *Nemocera*, etc. The *larvae* belong to a peculiar type, called the *long-headed larvae* (Marno).

An important feature of my superfamily *Eremochaeta* is the suppression of the old-fashioned family *Xylophagidae*. I showed that, from the very beginning, it was founded on the erroneous notion of a close affinity between the genera *Xylophagus* and *Subula*. It was a spurious family, not founded on any distinctly defined concept, and therefore a stumbling-block for a future development of the classification. Routine, and nothing else, had kept up this arrangement so long. When I found it reproduced in Brauer's paper on the *Notacantha* (1882), I took occasion to state my objections in the *Berl. Ent. Zeit.*, 1882, p. 364-366, 89 (1882), where a detailed historical statement is given. The components of the old-fashioned *Xylophagidae* (including, probably, even *Chironmyza*) must be absorbed in an enlarged concept of the family *Leptidae*. Since the discovery of many transitional forms in North America, Chili, and Australia, such a result is unavoidable, although a great deal remains yet to be cleared up.¹

In continuing my researches after 1892, I proposed, five years later in the *Berl. Ent. Zeit.*, 1897, the introduction of three new superfamilies to complete the subdivision of the *Orthorrhapha Brachycera*, 158 (1897). My new scheme was principally founded upon the contrast between *aërial* and *pedestrian* Diptera, a contrast the chief features of which had been already foreshadowed by me in my *Essay on Chaetotaxy* (102 (1884), p. 500-501), and to which I have given a further development in the above-quoted paper in the *Berl. Ent. Zeit.*, 1897, p. 366-367. In the present "Record," Part II, Chapter XXIV, I have given an elaborate essay on the influence of the two modes of life-habit on the organization of *all* the families of Diptera. Besides the superfamily (1) *Eremochaeta*, adopted by me in 1892, the three new ones, introduced in 1897, were (2) *Mydaiidae*, for this single family, which I consider as a relic of an

¹ I cannot, of course, reproduce here the numerous innovations introduced in my 130 (1892), for instance in the grouping of the families within the superfamily *Nemocera vera*, and must refer the reader to the paper itself, or to the account of it given in the "List."

earlier geological period; (3) *Tromoptera*, for the prevailingly *aërial* Diptera (*Nemestrinidae*, *Cyrtidae*, *Bombyliidae*, *Therevidae*, *Scenopinidae*); and (4) *Energopoda* for the *pedestrian* Diptera (*Asilidae*, *Dolichopodidae*, *Empidae*, *Lonchopteridae*, *Phoridae*). Objections have been raised against the juxtaposition of the *Asilidae* and the *Dolichopodidae*, etc., in the same superfamily. I believe that this juxtaposition is sufficiently justified by the structure of the head, of the antennae, and of the genital organs in this superfamily as conceived by me, and that the connection between the *Asilidae* and the rest of the families comprised in it is found in the Mydraid-like and ancestral section *Apiocerina*, the different forms of which occur in Australia, Chili, and some parts of North America, which are countries abounding in ancestral forms.

I feel convinced that the new distribution of the *Orthorrhapha Brachycera* which I have proposed is better than the previous one. But at the same time I am far from expecting, or even desiring, that it should be immediately accepted in handbooks of dipterology, or introduced into collections of Diptera. My feeling about it is distinctly expressed in the last passage of my *Preliminary Notice*, etc. (*Berl. Ent. Zeit.*, 1896, p. 373), which says: "My paper is a *ballon d'essai*, which I launch, attentive to the course it will follow!" Much remains to be done in anatomical and biological research before the final introduction of my new arrangement into dipterological practice can take place; and Dr. D. Sharp, in his *Insects* (London, 1899, p. 456-457), has set a good example in not adopting this arrangement, but in giving a short account of it, and calling the attention of advanced students to it.

The views concerning *Zoölogical Classification* and the *Nomenclature* connected with it which I have gradually developed during my studies may be summed up as follows: I understand the office of both to be merely *mnemonie*: they represent a scheme for the arrangement of animal forms tending to make the affinities existing between them tangible and easier to remember. I expressed this opinion for the first time in the *Berl. Ent. Zeit.*, 1895, p. 160, 144 (1895) in the following terms: "The true end of classification is an *easier survey of affinities*, a temporary aid to memory. In space and time all divisions become convergent and finally confluent." Classification is not necessarily permanent. If zoölogists had existed in different geological epochs they would have introduced different arrangements, to answer the requirements of their

times. Modern classifications have been modified in consequence of newly discovered recent or fossil forms. If we knew in all its details the evolution of the successive forms of living beings, classification would have been replaced by the *history of that evolution*. But as our knowledge of this history is only very fragmentary, the practical task of the classifier of the present time consists in contriving a scheme of arrangement to contain the *recent fauna* and the fragments of the *fossil fauna*, so far as known.

In the "Life and Letters of T. H. Huxley," by his son L. Huxley, London, 1900, Vol. I, p. 312, I find the following passage about fossil remains: "What a wonderful assemblage of beasts there seems to have been in South America! I suspect if we could find them all they would make the classification of Mammalia into a horrid mess."

Ever since I began, more than fifty years ago, to reflect upon the true aim of classification, I have recognized *evolution* as a logical postulate, a *sine quâ non* of organized life. Darwin's work of 1859 gave a new impulse to my thinking, and I gradually came to consider the evolution of organic forms through geological ages as the *Growth of the tree of Life* (to use the expression of Genesis iii. 22), whose branches and branchlets, under the ambient influences which prevailed under each of the successive epochs, were subjected to the regimen of *adaptation*. Natural selection, according to my view, has been the result (or *effect*) of adaptation, and not the *cause* of evolution. I have compared the evolution of organic forms to the *growth of a tree of life*. But, as the French say, "comparaison n'est pas raison." What *is* Life? On this question I adhere, at least in the present state of our knowledge, to the humble avowal of Professor Du Bois-Reymond: "Ignorabimus!" (Du Bois-Reymond, "Ueber die Grenzen des naturwissenschaftlichen Erkennens." Leipzig, 1872, p. 33.)

Under different conditions of temperature, land, water, and air, during geological ages, a multiplicity of terrestrial, aquatic, and aerial organizations has been evolved. Among this variety of influences, a special mode of life, *parasitism*, in its various forms, became a potent agency in producing *aberrant* organizations. In the class of insects, it is due to parasitism (and especially to the *internal parasite*) that such specialized forms as *Rhipiptera* and the

Meloidae (among the Coleoptera), *Mantispa* (among the Neuroptera), *Cyrtidae*, *Conopidae*, *Pipuneulidae*, *Oestridae*, *Tachinidae*, *Phoridae*, even *Anthrax*, *Bombylius*, and others (among the Diptera), and numerous similar instances among the Hymenoptera exist. *Commensalism* likewise produces aberrant forms, like the *Pselaphidae*, *Paussidae*, and *Lomechusa strumosa* (*Staphylinidae*) among the Coleoptera, and it is difficult to draw the line between commensalism and some forms of parasitism in the widest acceptation of the term.

In the course of my new studies I came gradually to abandon descriptive dipterology. Since 1890 especially, I undertook a more thorough study of the earlier literature, which has been much neglected by most authors. I found that valuable hints contained in the writings of *Latreille*, *Lyonet*, *Rob.-Desvoidy*, etc., had been overlooked or insufficiently studied, and that, by means of a study of earlier authors, neglected characters introduced by them might be rediscovered, and the synonymy of genera and species, the systematic nomenclature, and the terminology of organs might be improved. Thus a series of papers, classificatory, critical, historical, and bibliographical, came to be published.

These papers either bore a general character, like that already quoted: *On the Characters of the three Divisions*, etc., 130 (1892); or they treated of single families and groups, like the *Blepharoceridae*, 60 (1878), 124 (1891), 144 (1895); *Psychodidae*, 146 (1895), 150 (1895); *Nemestrinidae*, 162 (1897); *Apiocerina*, 93 (1883), 109 (1886), 123 (1891). A similar paper on *Cyrtidae*, which was nearly finished in 1895, is still unpublished. The much-debated question on the right spelling of *Mydas* or *Midas* was critically settled, 149 (1895). Similar questions on the terminology of certain organs were historically investigated in the papers: *On the Terms Tegula, Squama, Alula*, etc., 154 (1896); *On the Terms Calyptrata and Acalyptrata*, 157 (1897); *Zur Geschichte der sogenannten Brustgräte*, etc., 135 (1893). Criticisms appeared of publications of contemporary authors, *Brauer*, 89 (1882), 136 (1893), 137 (1893); *Mik*, 122 (1890), 126 (1891), 156 (1897); *Williston*, 127 (1891); also bibliographical papers, like the Lists of the publications of *Rondani*, 79 (1881), 98 (1884), 106 (1885); and *Loew*, 104 (1884); a critical appreciation of the relative value of the two editions of Loew's "Posener Dip-

teren" of 1840, **153 (1896)**. An investigation of the circumstances under which *Rob.-Desvoidy's* two successive works on the *Myodaires* were published is incorporated in the present "Record" (Part II, Chapter XXIII). My account of the publication of the first of these works, the *Myodaires*, is based upon an apparently forgotten and most interesting document, the *Report* on *Rob.-Desvoidy's* manuscript, drawn up by the Committee of the Academy of Sciences in Paris in 1826, and signed by *Latreille*, *Duméril*, *Blainville*, and *Cuvier*, the latter as Perpetual Secretary of the Academy at that time.

Lately, I have published the following three *critical* papers:—

(1) Notiz über die Erstlingsarbeit Duméril's, etc., in the *Verh. zool.-bot. Gesellsch.*, Wien, 1900, p. 450–451, **166** of my "List."

(2) Notice on the synonymy of *Anopheles maculipennis*, in the *Ent. Monthly Mag.*, London, 1900, p. 281–282, **167** of my "List."

(3) On the new nomenclature of the family *Cecidomyiæ*, *Ent. Monthly Mag.*, London, 1901, p. 40–43, **168** of my "List."

I have attempted to justify the usefulness of such apparently laborious and tedious critical, historical, and bibliographical work in the following passage of my above-quoted paper: *On the Terms Calyprata and Acalyprata*, **157 (1897)**: "A celebrated French painter, I believe it was *Ingres*, used to say: 'Le dessin est la probité de l'art.' So it may be said: 'Literature is probity in science.' If during the last six or seven years I have spent a considerable amount of time in tedious researches in entomological literature, I had some reason for considering such researches as my special duty. I have the advantage of possessing a rather complete dipterological library, over the contents of which, by dint of indexes, extracts, and cross-references, I have acquired a certain (although still very insufficient) mastery. Another advantage which I enjoy consists in an almost absolute freedom in the disposal of my time. Under such favorable circumstances, it is much easier for me than it would be for others to fulfil some duties of drudgery, indispensable, among the deluge of literature, for maintaining a *decent level of scientific probity*. And I believe that my labor is not lost, so long as I am helping others to maintain that level."

During the last twelve years, many investigations were begun by me in different directions, and inchoate papers kept back for the sake of emendation, when ill-health, the result of overwork, interfering with my projects, reminded me of the necessity of bringing my entomological career to an appropriate conclusion. This conclusion I offer in the present "Record," and in the commented "List" of my writings ready for publication. My successive papers on the so-called *Oxen-born bees* of the Ancients (*Bugonia*, 133, 142, 147 (1893-1895)), although they concern folk-lore, philology, and even theology, rather than entomology, have been included in the "List," because they contain a history of the geographical distribution and of the very remarkable wanderings of a common dipteron, *Eristalis tenax*, the drone-fly.

The historical and critical paragraphs which I have introduced in this "Record" will be accepted, I hope, as a useful contribution to the history of dipterology. *Criticism* has always been distasteful to me on account of the personal *animus* which is almost unavoidably connected with it. In rare instances only have I published criticisms, or rejoinders, on the spur of the moment. In a most provoking case, when Professor Brauer published his "Offenes Schreiben, als Antwort auf Herrn Baron Osten-Sacken's *Critical Review*," etc. (1883), which was nothing but an unjust and undignified effervescence against me, I did not make use of an excellent occasion for a retort, which an egregious error in that pamphlet offered me. I waited for fourteen years, but then the pointing out of that *lapsus* became unavoidable (*Berl. Ent. Zeit.*, 1897, p. 148-149; 162 (1897)). Many criticisms, which I have kept *in petto* for years, have found their place in this "Record" as a mere matter of history. Entomological literature would become intolerable if it were considered every one's duty to criticize and contradict in print the many errors which continually rise to the surface. But I hold it as useful that authors, who have some right to believe themselves competent, should engage occasionally in the work of purifying the literature of their domain. If such a rule were generally adopted, superficial writers would be more mindful of the Russian proverb: "*Whatever is written with the pen, cannot be hewn out with the axe.*" And the conviction that, sooner or later, justice

must be done, would act as an efficient check upon many useless publications.

An historical account of a branch of science is not complete without some characterization of the workers who have been active in it. I have therefore attempted to introduce into my account characterizations of entomologists in regard to their qualification for scientific work — in the measure, of course, of my capacity. Drawing portraits means introducing the psychological element into history. *Talent* is a gift of nature, and does not, for that reason, constitute in itself a merit; the merit lies in the *character* which makes talent fruitful. The duty of a fair critic is to sum up the scientific progress resulting from the action of a given character upon a given talent.

The *beginnings* of a worker in science are often symptomatic of his future work. The celebrated contemporary Dutch painter *Joseph Israels* is reported to have said about beginners *in art*: "The born artist places his ideal so high, that he never imagines he has reached it. A beginner who is satisfied with his work arrives at nothing."¹ The same is true of science, and there are many instances of entomologists who, with a decided talent, have been led astray by conceit and ambition. This "*Rule of Beginnings*," as I would call it, is in most cases infallible.

What is needed in entomology, as well as in every other branch of science, is co-operation among honest workers; and an effective co-operation is impossible without a mutual recognition and appreciation of *character*. "As we are, so we associate. The good, by affinity, seek the good; the vile, by affinity, seek the vile" (Emerson, Divinity College Address, 1838).

It will be noticed that in exercising the duties of criticism I have been particularly careful to render justice to meritorious authors who have been misunderstood and decried through the malevolence,² or neglected through the carelessness, of their contemporaries and successors. As such, I would especially mention

¹ This passage is borrowed from an article about the painter *Israels* in the *Frankfurter Zeitung*, February 11, 1897.

² "Expressions malveillantes." *Rob.-Desvoidy, Ann. Soc. Ent. France*, 1846, p. 351.

Rob.-Desvoidy, Rondani, Zeller, Schiner, and also, in a certain measure, *Haliday*.

I could not omit giving an account of my controversies with Professor *Brauer*; but, avoiding useless detail, I have introduced all the references necessary for a critical study of them in the future. It is an historical account *sine ira et studio*. In fact, it will be seen that, in most instances, Brauer is made to be his own critic by a mere verbal reproduction and juxtaposition of his own utterances.

It has been an equally distasteful task for me to point out my friend *Loew's* persistent and systematic disregard of his brother dipterologists; it constituted a dark and to me an almost inexplicable feature of his character. The only secret motive of this idiosyncrasy that I can surmise is the consciousness of Loew that Nature had formed him for something better than describing Diptera, which was probably true. But that is not a sufficient excuse for the eccentric treatment which Loew was in the habit of dealing out to dipterologists like *Zetterstedt, Winnertz, Rondani, Schiner*, and myself. The facts are given in Part II of this "Record," for *Zetterstedt*, in Chapter XVIII; for *Winnertz*, in Chapter VI; for *Rondani*, in Chapter XIX; for *Schiner*, in Chapter XXI; and for myself, in Chapter II, an account of Loew's treatment of my Monograph of the *Tipulidae brevipalpi*, and in Chapter XII, a general account of my co-operation with Loew, and of his sudden rupture of it in 1875. Even *Haliday*, Loew's most intimate correspondent, was on more than one occasion slighted by him, as will be shown in my account of their relations (in Part II, Chapter VIII, *Haliday* and *Loew*).

In my personal intercourse with Loew, I used to express to him very frankly my disapproval of his treatment of some authors. He received such reproaches with pleasant raillery, and passed to other subjects of conversation. On one occasion Loew acknowledged to me with remarkable candor his propensity to *jealousy*. I had bred several new species of North American *Trypetae* from galls, and intended to publish an account of them with illustrations, but I deemed it prudent to ascertain Loew's opinion beforehand on this project. He answered (March, 1862): "You ask me in your letter

whether it would be agreeable to me if you published descriptions of several North American *Trypetae*. Of course it would be unbecoming for me, in such a case, to propose any conditions to you. But your frank question I answer with an equally frank confession of a foible of mine, that, in regard to this particular family, I am *a little jealous* of the publications of others. Besides, we might easily come into conflict in our publications, as I have again, just now, described *nine* North American species.”¹

This last pretext was of course futile, but Loew had given, a short time before (between 1858 and 1862), a striking illustration of his jealousy concerning *Trypetae* in his extraordinary behavior towards *Schiner* on the occasion of the publication of the volume: “Die europäischen Bohrfliegen (*Trypetidae*).” The details of this incident, reproduced in Part II, Chapter XXI, already quoted above, show to what incredible extremes Loew’s “foible” could provoke him.

Jealousy among the passions has a higher standing than *envy*. *Jealousy* presupposes possession; *envy* on the contrary betrays the want of it. Loew had every reason to be proud of his *possession*. *Envious* people (and especially envious entomologists) generally have but little to show.

In regard to Loew I state here once for all that, while condemning his injustice in the strongest terms, I do not mean to impugn his personal character as a man of truth and honor. Loew, without any question, was a very superior man, *far superior to me in natural ability, as well as in learning*. His colossal labors stand for him; what I have done has been to place his work in a better light, and to give it a more distinct definition, which was due to history. But I have treated Loew’s unceremoniousness towards his colleagues with an equal unceremoniousness. If I, as a younger entomological colleague of his, represent him now as I knew him as a dipter-

¹ “Sie fragen mich in Ihrem Briefe, ob es mir lieb wäre, wenn Sie etliche N. Amer. Trypeten beschrieben. Es versteht sich dass es mir nicht zukommt Ihnen in dieser Beziehung je eine Bedingung zu machen. Doch will ich Ihnen auf die offene Frage ebenso offen meine Schwäche bekennen, dass ich gerade in dieser Familie auf die Publicationen anderer etwas *neidischer* Natur bin. Auch könnten wir in unseren Publicationen leicht zusammentreffen, da ich bereits wieder *neun* N. Amer. Arten beschrieben habe.”

ologist, with the defects of his character and *the palpable limitations of his entomological talent*, I mean to render him a better service than I should by feeble attempts at concealment and attenuation. After twenty-nine years of relations with him, I possess in his voluminous correspondence a source of information which enables me to accomplish this task better than anybody else, and I am convinced that I have fulfilled it with impartiality.

The greatest difficulty, however, which I have had to overcome is that of speaking of myself. In a personal record like the present it is not easy to avoid the semblance of egotism. But I feel that, after the lapse of nearly half a century (for the beginning of my entomological career dates as far back as that), I have some right to look upon my former self as upon a different person. Not having had any training as a professional zoölogist, I have always considered myself a mere *dilettante*, and as such have always avoided going beyond my depth. My only purpose in working was to satisfy a natural wish to turn to some account my moderate abilities, to which fate had originally given a different, and to me distasteful, direction. Sir *M. Grant Duff* (in an article on the Cretan question, *Contemporary Review*, April, 1897) says: "I once introduced an eminent Englishman to *Jules Simon* as a politician and an entomologist. '*La politique*,' was the reply, '*c'est un peu de l'entomologie*,' and so it is." When I found myself figuring in both capacities (between 1871 and 1873), I gave up politics for entomology, and have never regretted it since.

Ed. Perris wrote to *Mulsant* in the ominous year 1848: "La république des lettres et le socialisme des plantes sont les seuls auxquels je m'attache, et plus je m'aperçois que les hommes ne valent pas grand'chose, plus je me raccroche aux plantes et aux bêtes, aux bêtes surtout, si dociles à leur instinct, si fidèles à leur destinée. — Les gouvernements changent, mes occupations se multiplient; mes goûts ne se modifient pas. . . . Je me trouve parfaitement bien d'échapper aux agitations dont les événements politiques semblent avoir fait une nécessité à la plupart des hommes." (Quoted by *Laboulbène*, in his obituary of *E. Perris*, in the *Ann. Soc. Ent. France*, 1879, p. 375.)

I can say that in my new career which I embraced *seriously* rather late in life (in 1877, at the age of nearly fifty) I have done my best, and have succeeded sufficiently to satisfy my conscience. Honest criticism, tending to amend, or even to reverse, any of the

dicta given forth by me, will give me pleasure, as showing progress in the cause of truth which I have always striven to serve. *Of one peculiarity of my work I feel certain: the best part of it is that which has assisted and stimulated the work of others, and I am conscious at the same time that that part of my work is the largest.* This will, I trust, suffice to condone all the faults which I may have committed. I feel nevertheless impelled to introduce here, in extenuation of my shortcomings, a brief account of the difficulties I have had to contend with besides my absolute want of training as a professional zoölogist, and my imperfect knowledge of the English language. I have never had the good fortune to reside for any sufficient length of time in a place in which, or within a reasonable distance of which, there was a large dipterological collection. In St. Petersburg, in my time (that is, before 1856), there was no dipterological collection, public or private, to guide my first steps. The museum of the Academy of Sciences possessed a valuable but small set of Diptera labelled by Meigen, and that was all. In the United States, some remains of Say's and Harris's types were preserved in Boston; there was no collection either in Washington or in New York, the places in which I resided most of my time between 1856 and 1871. Thus it happened that my first monograph of the *Tipulidæ brevipalpi* (1859) was based upon a collection formed, with very few exceptions, by myself. My further studies were supported by abundant and most liberal contributions from all sides, but for comparison and guidance I had nothing to rely upon but the hasty notes gathered in European museums during my temporary absences from America. Finally, in Heidelberg, where I settled after having left *all* my collections in the United States, and where I made the attempt to extend my studies to the Diptera of all the world, I had to rely for my instruction, in addition to the laborious and ungrateful study of a voluminous and generally unsatisfactory literature, upon rather scanty materials which were only the very incomplete collection of European Diptera which I had acquired from Zeller, and the collections of exotic Diptera which, at different times, had been confided to me for the purpose of description. For more information I had again to rely upon fugitive visits to distant museums (principally in London, Oxford, Berlin, and

Vienna). On such occasions I took notes, as well as I could, and, as I but seldom carried specimens for comparison with me, I had most of the time to rely on my memory. As the last years of my entomological career were principally devoted to generalizations tending to improve the System, it is painful for me now to reflect how much I lost by such isolation and how much more completely my work would have succeeded, if I had been able to illustrate it by numerous examples founded upon the study of exotic forms in museums within easier reach!

The views on science and scientific work in general to which I have adhered during my lifetime have been expressed, more than sixty years ago, by a much more eloquent voice than mine. On December 29, 1835, *Karl Ernst von Baer*, whom I feel honored to recall as a patron and a friend, pronounced an oration before the Academy of Sciences in St. Petersburg, the subject of which was: "Blicke auf die Entwicklung der Wissenschaft" ("Views on the Development of Science"). I cannot do better than close this "Introduction" with a passage from that oration.¹

"If I presume to beg your attention a little longer, it is that we may consider a greater subject, the wonderful Pantheon of Science itself, to which we will now revert. The Builder of this edifice, who tests all the stones before putting them together, so that the building shall not totter, is *Criticism*, — but he is an ever-living Builder, whose power and insight grow with his work. As to the result, therefore, let us be unconcerned! The individual workman must break each stone out of the quarry, and then so fashion it as to fit it for the place intended for it. To no one is it granted to complete any very large portion of this edifice single-handed. 'Genius,' if by that is implied irresponsibility (*'Ungebundenheit'*), has long since lost its credit in the domain of Science. It had better try its luck in the world of Poetry! In the domain of Science, talent alone, coupled with diligence and the power of self-

¹ The original German text of the passage will be found in my paper: "Zur Geschichte der Brustgräte, etc., nebst einer Erinnerung an Karl Ernst von Baer" in the *Berl. Ent. Zeit.*, 1893, p. 373, No. 135 of my "List." The whole "Oration" has been reprinted in Vol. I, p. 75-160, of the collection of miscellaneous publications of Baer, published under the general heading: "*Reden gehalten in wissenschaftlichen Versammlungen*," etc., in three volumes, St. Petersburg, 1864-1873.

control, is of any value. With the poetic temperament a man may earlier grasp the future problems of Science, but the more he contrives to suppress the Poet within him the more successfully will he labor on the edifice itself, however fascinating it may be to soar to those heights for the future attainment of which he ought perhaps to be working on the lowest rung of the ladder."

C. R. OSTEN SACKEN.

HEIDELBERG, BUNSEN STR. 8.
January 30, 1901.

NOTE ON THE RISE OF THE ORDER DIPTERA IN PUBLIC ESTIMATION DURING THE NINETEENTH CENTURY

Diptera during the past century have gradually risen in public estimation, especially among men of science. The superiority of their organization has been recognized by systematists. Observers of living specimens have noticed peculiarities in their behavior which prove a higher development of their faculties than of those of other orders.

Diptera, more than all other insects, show a distinct love of *freedom*, while *Hymenoptera*, with all their perfections, betray *drill*. Owing to their organization Diptera have more control over their motions than any other insects, in consequence of which there is a remarkable stamp of *individuality* in their actions. They can suddenly arrest their flight and poise in the air; they can not only swarm, but *dance* in cadence, or gambol in the air in the most extraordinary manner. It is principally the males who dance, play, and frolic together; during courtship they perform most ludicrous antics. Schiller ("Aesthetische Briefe") said: "The animal, the child, as well as man, *play*: the sense of strength and the higher sense of freedom derived from strength give rise to the joy of playing."

At Trenton Falls, N. Y., I remember watching several male Dolichopods executing a ludicrous *cancan* on the stony ground along the stream. The species was called by Loew *Tachytrechus moechus* ("Monographs North American Diptera," II, p. 112, "Trenton Falls, O. S.").

The following passages, borrowed from the works of different naturalists, have reference to the change of public opinion about

Diptera alluded to above. *Oken* (quoted by *Zeller, Isis*, 1840, p. 10) called them "the beggars among Insects, living in dirt."

Kirby and Spence, "Introduction" (1826), Vol. IV, p. 381, say: "While the last [*Hymenoptera*, including the honey-bee], on account of their wonderful economy and the benefits which by them Providence confers upon mankind, have been justly regarded as the *princes* of the winged insect world, — the former [*Diptera*, or two-winged insects], when we consider the filthy and disgusting habits of their grubs, and the annoyance, both from their numbers and incessant assaults, of them, in their fly-state, may very properly be considered as its *canaille*."

Sixty years later *B. T. Lowne*, in his "Anatomy of the Blow-fly" (1890, p. 25–27), expresses himself very differently.

"The blow-flies belong to the family *Muscidae*, one of the most highly specialized groups of the Diptera, the most highly specialized order of the class Insecta." "Just as all discussion would be futile as to whether a bird or a mammal is the higher type, so it is useless to consider whether the Diptera or the Hymenoptera have the higher organization; but there can be no question as to which of these orders departs most from the more generalized form. The Diptera are far more remarkable in their developmental history, and in the modification of structure which they present in the adult or imago form. In this relation the strong tendency of many to produce their young alive, and the fact that some have a capacious matrix, or uterus, in which the larvae are hatched, or even attain the pupa form, before birth, is not without interest, presenting as it does some analogy with the viviparous character of the mammalia amongst vertebrates — whilst the nest-building instincts are more manifest in Hymenoptera and in birds. It is true that the flies, and more especially the heavy forms, with a comparatively tardy flight, like the blow-fly, have been regarded as 'stupid' — Sprengel called them 'die dummen Fliegen' — and do not excite our sympathy and curiosity to the same extent as the social Hymenoptera; but it is impossible to judge of the intellectual functions of an insect. The manner in which the blow-flies and their near allies, the house-flies, have made themselves at home with man, speaks for their power of adapting themselves to new and varied condi-

tions. They are cunning, wary, and easily alarmed, and except when benumbed with cold or heavy with eggs, know well how to avoid danger. They appear to me far more clever in this respect than the bees and wasps." I borrow this remarkable passage of Mr. Lowne from Mr. C. H. Tyler Townsend's paper in the *Canadian Entomologist*, 1893, Vol. XXV, p. 135.

PART SECOND

TWENTY-FOUR CHAPTERS ON HISTORICAL, BIOGRAPHICAL, CRITICAL, AND PURELY ENTOMOLOGICAL SUBJECTS CONNECTED WITH MY WORK

PREFACE

SINCE the publication (1901) of the Prospectus of my "Record," I have changed my original plan of intercalating "Chapters" on different subjects between the items of the "List" of my papers. It occurred to me that, as my "Chapters" contain a great deal of new historical, biographical, and critical matter of immediate interest, independent of the "List," it would be as well to publish them at once, and in a connected form. The number of "Chapters" has been now reduced to *twenty-four*. This will thus form the *second* part of my "Record."

The *third* part, the "List" itself, being already prepared in manuscript, contains matter enough for publication, even if I should be no more present to superintend it.

As my dipterological career has been intimately connected with that of Loew during the greater part of his lifetime, a considerable proportion of the "Chapters" has been devoted to him. They are both biographical and critical, and their contents have been foreshadowed on pages 19 to 22 of my "Introduction." The concluding passage on p. 21 may be reproduced here, as summing up the scope of the "Chapters" concerning Loew: "If I, as a younger entomological colleague of his, represent him now as I knew him as a dipterologist, with the defects of his character and the palpable limitations of his entomological talent, I mean to render him a better service than I should by feeble attempts

at concealment and attenuation. After twenty-nine years of relations with him, I possess in his voluminous correspondence a source of information which enables me to accomplish this task better than anybody else, and I am convinced that I have fulfilled it with impartiality.”

C. R. O. S.

I ON THE BEGINNINGS OF MY RELATIONS WITH LOEW, WITH SOME REMARKS ON THE GENERAL CHARACTER OF OUR CORRESPONDENCE

My correspondence with Loew began as early as 1850 or 1851, when I was still living in St. Petersburg, but I have not preserved his letters of that time. I sent him a *Limnobia* which I had discovered near St. Petersburg, and which he described as *L. imperialis*, sp. n. It proved afterwards to be the long-lost *Tipula annulata* Linné (compare 2, 1857). The tone of Loew's letters to me not having been encouraging, the correspondence was not continued. In 1856, Loew heard from Dr. Hagen of my appointment as Secretary of the Legation in Washington, whereupon he suddenly displayed the greatest eagerness to renew our correspondence. He wrote me (Mescritz, April 11, 1856) a most urgent letter, expressing the hope that I would send him American Diptera, and complaining of his isolation, principally caused by his quarrel with Dr. C. A. Dohrn, President of the Stettin Entomological Society, who, as Loew contended, “had systematically cut off all the sources of supply of material for work, which he formerly received through the medium of the Society.” Loew, on that occasion, sent me his recently published paper on *Bombylius* (“Neue Beiträge,” 1855) and concluded his letter with this characteristic flourish: “It is a pleasant feeling for me to think that, during the probably tedious Atlantic passage, or perhaps even on American soil, you will cast a glance on the lines of my composition, and recall my request that you should be for me a Count von Hoffmannsegg, while, in truth, I am not immodest enough to compare myself with Master Meigen.”¹

¹ “Es ist mir ein gar angenehmes Gefühl zu denken, dass vielleicht auf der doch etwas langweiligen Seereise, oder gar auf amerikanischem Boden, Ihr Blick einmal auf die von mir verfassten Zeilen fallen und Sie daran erinnern könnte, dass ich Sie

More than *two* years passed after this letter, during which I heard *nothing* from Loew, although I had written him several times, and had sent him a first consignment of Diptera, as well as a copy of my Catalogue of North American Diptera, which I had published in the mean time. The solution of this mystery came in a letter, dated from Meseritz, October 1, 1858. Loew, probably in consequence of some vague association in his mind between the United States and the *Declaration of Independence*, had directed his letters to the Russian Legation in *Philadelphia*, without having noticed that *my* letters were always dated from Washington. After a long interval the letters were *returned* to him by the Post Office, and he had the mortification of sending them back to me with a doleful explanation.

In my "Introduction," p. 4, I said: "After a rather long, *quasi*-tentative correspondence, during the first years of my residence in America, about the mode and conditions of our proposed co-operation, we came to the following agreement," etc. The nature of this agreement, as well as its final result for the benefit of American dipterology, having been explained in the "Introduction," I shall confine myself here to some particulars concerning my epistolary intercourse with Loew.

First of all, I feel bound to acknowledge Loew's conscientiousness and painstaking accuracy in rendering me an account of all the successive consignments of Diptera which he received from me. The species were not only exactly enumerated, but almost every one of them separately discussed. When a species could be recognized among published descriptions, the probability of its exact determination was discussed; new, or probably new, species were noticed as such.

This mode of treatment gave rise to an abundant and mutually useful interchange of ideas. That I was not successful in my attempts to come to an understanding with Loew about the *terminology* of the organs of Diptera, will be shown when I come to

gebeten habe, mir ein Graf von Hoffmannsegg zu sein, wobei ich mich mit Meister Meigen vergleichen zu wollen, wahrhaftig nicht unbescheiden genug bin." — Count Hoffmannsegg (1766-1849), a Saxon nobleman and naturalist, sent Meigen a collection of Diptera from Portugal, but did not publish concerning it himself.

Ueber die Nordamerikanischen Vögel der Gattung *Tatanocera* und *Seydon*, vom Dr. H. Loebe in Pittsburg.

I. Tetanocera.

[illegible][illegible][illegible]

a. Der Thron ist punktiert.
 1) flüchtiges Gewebe, (verwunde neu gep. 2) kitzelnd neu gep.
 b. Der Thron ist gestrichelt.
 1) flüchtiges Gewebe, (verwunde neu gep. 2) kitzelnd neu gep.

II. *bianghi nicot glabresc*
4) *halidua niope*, 5) *composita niope*, 6) *desmodium* *zinn*.
7) *plumosa* *zinn*.

[illegible]

Das ist ein bisschen wie bei einem Koffer, den man mit einem Kofferband zusammenbindet. Man hat einen Koffer, der aus einem Stück Holz ist, und man bindet ihn mit einem Kofferband zusammen. Das ist ein bisschen wie bei einem Koffer, den man mit einem Kofferband zusammenbindet. Man hat einen Koffer, der aus einem Stück Holz ist, und man bindet ihn mit einem Kofferband zusammen.

[illegible]

Verantwortung übernehmen. Die Verantwortung wird nicht in der Zeit, sondern am Ende der Zeit, wenn die Verantwortung nicht mehr zu übernehmen ist, sondern nur noch zu übernehmen ist. Die Verantwortung wird nicht in der Zeit, sondern am Ende der Zeit, wenn die Verantwortung nicht mehr zu übernehmen ist, sondern nur noch zu übernehmen ist.

[illegible][illegible][illegible]

Spec. 2. *Stylipetia*, new sp. The surface is extremely rugose, punctate, with numerous small pits, and a few large, shallow, circular pits. The color is a uniform reddish-brown. The length is 2.5 mm. —

judge Leow as a dipterologist, and not merely as a correspondent (in Chapter XVI).

Some other details about my correspondence with Loew are not without interest, as they illustrate the peculiarities of his character. He usually wrote on a light bluish letter-paper 22.6 by 14.1 centimeters in size. His handwriting was perfect, but exceedingly minute. On a page of that size he would easily place 60 lines, but sometimes much more. For instance, in a letter of March 18, 1859, four pages contain successively 74, 85, 94, and 81 lines. From the very beginning, I had begged him to use *Latin* characters, as less trying to the eyes than the Gothic ones, and he had complied with my request. Loew was evidently proud of his calligraphy, and on two occasions it pleased him to produce masterpieces of it. On a space of 12 centimeters by 14, he wrote out, in 61 lines, the twenty-seven Latin diagnoses of Japanese Diptera, the detailed descriptions of which were afterwards published in the *Wien. Ent. Monats.*, 1858, p. 100-112 (letter of January 8, 1858). Not content with that, about a year later (May, 1859) he sent me the *whole* of two articles: "Die nordamerikanischen Arten . . . Tetanocera," . . . (*Wien. Ent. Monats.*, 1859, p. 289-300); and "Diptera Americana ab Osten-Sackenio collecta," decas prima (in the same periodical, 1860, p. 79-84); that is, *seventeen* printed pages written out on *two* pages, on his usual letter-paper of 22.6 by 14.1 centimeters. The first page contains 132 lines, that is on the average six lines in the space of a centimeter! (I reproduce the first page of this *tour de force* in facsimile.) The motive which makes me insist on this detail is, that the incredible endurance and tenacity exhibited in such a feat is the very quality that enabled Loew to accomplish his colossal and, at the same time, *his so homogeneous and thoroughly finished work* in descriptive dipterology.

The *contents* of the letters also showed some peculiarities which deserve to be noticed, because some of them reappear in Loew's entomological publications. The business part of the letters, as I have said above, left generally but little to be desired. But Loew was in the habit of introducing extraneous matters, discussions, etc., and in such cases his defect of *great prolixity* appeared to his disadvantage. His habit was to write such portions of a letter at

leisure, when he had nothing else to do; he would leave off for several days, or even weeks, so that a letter was only finished a month, or more, after it had been begun. Such letters were wanting in genuineness; they had a note of strain in them by no means pleasant to his correspondent. The same prolixity occurs also in some of Loew's publications. A conspicuous instance is the diatribe of Loew against Bigot, in the *Berl. Ent. Zeit.*, 1858, p. 341-346. The first four pages read like a prize essay on zoölogical classification, written by a scholar at the bidding of his teacher; the last page alone contains criticisms against Bigot. Loew knew very well that Bigot could not read German, and as this composition contains nothing of interest to anybody else, it is difficult to guess for what purpose it was written. The style is the heaviest German, and there is a sentence which begins about the middle of p. 341 ("Im ersten Fall," etc.) and ends on the next page. Both this performance, and the calligraphic *tour de force* which I have reproduced, look like the outpourings of an excess of working-power, and savor of ostentation.

Another specimen of the same prolixity, although in a less degree, appears in Loew's Preface to the second edition of his "Posener Dipteren" (1840), which I have reproduced in the *Berl. Ent. Zeit.*, 1896, p. 282. And the same failing may be noticed in the often too long descriptions of Loew's new species, a failing which becomes intolerable in his "Dipteren-Fauna Südafrika's" (1860). The justification attempted by Loew in the Preface of this work (p. iv) is quite inadequate. The mere *length* of a description is *not* a merit, as any one will find who will try to determine African Diptera of the more difficult families from these bewildering descriptions. No wonder that Loew did not find a publisher for the second volume, which, according to the statement in the Preface (p. vi), was to follow soon after the first.

II ON LOEW'S RECEPTION OF MY FIRST WORK ON TIPULIDAE

It would seem natural that Loew should have taken some interest in the work of his (at that time) young correspondent, concerning a family of Diptera that he knew to be in a chaotic condition.

I anticipated with pleasure some discussions with him about my classification, which, although founded upon very insufficient material, had the merit of originality, and introduced an abundance of characters that had never been used before. The first letter from Loew (January 17, 1860), announcing the reception of my work of 1859, began with an apology for the delay in acknowledging it, which he attributed to the dilatoriness of the bookbinder to whom he had immediately sent it ("Die Saumseligkeit des Buchbinders, welcher mir Ihre *Limnobina* nicht wiederbrachte, die ich ihm gleich bei dem Eintreffen übergab"). "At present," he continued, "I have studied your paper thoroughly ('durchstudirt'). You are perfectly right in complaining of the very unsatisfactory arrangement of the letter-press which interferes very much with the indispensable orientation. Nevertheless, with some good-will, one succeeds in getting through it and is rewarded by the contents ('und freut sich dann des gewonnenen Kernes'). Your work is a real progress in the classification, that perhaps will require modification when the faunas of other parts of the world are taken into consideration. . . . That you have not hesitated to multiply considerably the number of genera gives me pleasure ('freut mich sehr'). I would have even commended you if, in this respect, you had in some cases gone further. You see that your publication has caused me great pleasure. I shall, as much as I can, make use of it for my own studies. It would be expedient to popularize its systematic contents in one of our entomological periodicals. I do not know whether you have already done so in your notice in the *Stett. Ent. Zeit.*,¹ but what I know for certain is, that you would by this means have rendered a great service, especially if you had added an enumeration of as many European species as possible belonging to your new genera. In both respects you would have promoted the more speedy reception of your work."

About six weeks later, Loew (March 11, 1860) found it necessary to renew his assurances. "Your work on *Limnobiæ* I have studied again and again." The result of these studies became apparent when, in the course of the next year, Loew published in the *Wien. Ent. Monats.*, 1861, p. 34, among other Cuban Diptera,

¹ I had published a short notice about my work in the *Stett. Ent. Zeit.*, 1860, p. 87.

a pretended *Rhamphidia*, which does not belong to that genus at all, but is a *Teucholabis*, a new and very characteristic genus introduced by me. Loew, in this case, assuming *a priori* that the species was a *Rhamphidia*, because it had a somewhat elongated proboscis, gave a glance to what I said about this genus, and found, among the generic characters the words: "no stigmatical cross-vein." In his specimens that cross-vein was present. So Loew mentioned this discrepancy at the end of his description: "In reliquis *Rhamphidiis* adhuc descriptis venula transversa stigmaticalis deest." If Loew had really *studied* my work, as he pretended to have done, he should have begun by looking for his species in my analytical table of the genera (p. 203) and would have infallibly reached the genus *Teucholabis*, which is described in detail in the same paper, on p. 222-223. Made impatient by Loew's tergiversations, I must in one of my letters have asked him squarely to communicate to me some remarks about my classification. In an undated letter of 1862 (that is, before the publication of the *Tipulidae* in Schiner's Fauna, Vol. II) I find the following passage apparently in answer to my request: "To give you my opinion about your *Limnobina* I consider as quite unbecoming, as you know them ten times better than I; nevertheless, I shall attempt it. It is unnecessary to repeat that your work has caused me a most heartfelt pleasure. I have, as much as possible, attempted to verify the systematic results obtained by you upon European as well as upon American species accessible to me. I must acknowledge their correctness and admire your perspicacity," etc. Loew evidently thought that I had been fishing for compliments, whereas what I wanted was facts. — After the loss by fire of the manuscript of the intended *second* edition of my Monograph, Loew, evidently to fill up some space in one of the long, rambling letters he used to write me, gave me some advice on the modifications to introduce in some of the Sections, especially in the *Eriopterina* (letter of November 11, 1866). His observations in this case were based not upon my work, but on Schiner's second volume which had appeared in the mean time, so that some of Schiner's errors he attributed to me. "The separation of *Gnophomyia* from *Psiloconopa* can be maintained merely on account of the smallness

of the three terminal joints of the antennae and the peculiar erect pile on the legs of *Gnophomyia*, perhaps also on the aberrant shape of the discal cell." Now the *Gnophomyia* of Schiner's Fauna is *Trimiera* O. S. (as Schiner recognized later in his Novara work), and *Gnophomyia* O. S. (1859) is an entirely different genus. This proves that Loew had not paid the slightest attention to the elaborate paper on *Trimiera* which I had sent him after its publication in 1861. The same cause produced the same confusion in Loew's paper: "Beschreibung einiger afrikanischer Diptera nemocera" (*Berl. Ent. Zeit.*, 1866, p. 55-62), in which, on p. 59, a *Gnophomyia inconspicua*, sp. n., occurs, which is a *Trimiera*. That it was easier for Loew to read German than English is not an excuse; he understood English enough to read Haliday and Walker, and even to attempt to write the first volume of the "Monographs" in English.

The two copies of my work of 1859 which I had sent to Loew after its publication, I purchased after his death from the booksellers in Leipzig¹ who had acquired his library. One of these copies was *bound*, but does not bear any trace of having been handled; not a single pencil-mark showing study; not even the slightest crease, proving that the pages had been turned "again and again," as Loew had assured me. The *other* copy was in a state of pristine purity; that is, *entirely uncut*. Such is one of the amusing episodes of my long relations with Loew which I could not resist reproducing here. That Loew appreciated my system of *Tipulidae* he proved much later when, in 1878, he desired me to introduce it in his work on Amber Diptera (compare my Chapter IX: On Loew's work on Amber Diptera, etc.).

III ROBERT KENNICOTT

As early as 1859, that is, only three years after my arrival in the United States, in my "Monograph of the North American Tipulidac brevipalpi" (7, 1859, p. 200) I expressed my gratitude to

¹ *List und Francke*. I preserve both copies, untouched, as a *memento* in my library.

my friend Robert Kennicott, of Illinois, for placing at my disposal a highly valuable collection of Diptera, made during his travels in the northwestern parts of the Union. Later, he extended his travels much farther north, within the possessions of the Hudson Bay Company. Any dipterologist using the volumes of the "Monographs of North American Diptera" by Loew and myself, or Loew's "Centuries," will often in them meet with the name of Robert Kennicott, as the discoverer of many new species. Kennicott honored me with his particular friendship, and it is astonishing that, with his numerous duties in all branches of natural science, he found time enough to collect Diptera for me.

In order to show the high appreciation in which the authorities in Washington held Kennicott's services, I shall quote passages from official publications in that city. The History of the Half Century of the Smithsonian Institution, Washington, 1897, says about Kennicott: —

"The notable explorations of Robert Kennicott in British America and Alaska were made possible by the coöperation of several private individuals and scientific organizations and the Hudson Bay Company. This intrepid explorer, whose early death was a severe loss to American natural history, spent four years in the North and made the most extensive travels."

An account of Kennicott's earlier explorations is found in the Smithsonian Reports. The Report of 1863, p. 36, says: "The specimens received in 1863, from this [Kennicott's] exploration, filled forty boxes and packages, weighing, in the aggregate, 3,000 pounds. They embrace in the line of natural history thousands of skins of mammals and birds, eggs, nests, skeletons, fishes, insects, fossils, plants, etc. In the line of ethnology are skulls, dresses, weapons, implements, utensils, instruments of the chase, in short, all the requisite material to illustrate the peculiarities of the Esquimaux and different tribes of Indians inhabiting the northwest regions."

It was in the beginning of his career of travel that Kennicott wrote me a very characteristic letter, of which I reproduce extracts to illustrate the hardships he had to endure, and at the same time the easy, good-natured disposition, which makes it

appear as if his principal occupation in the world was to provide me with Diptera!

"FORT SIMPSON, MACKENZIE'S RIVER, H. B. T.

"Nov. 30, 1859.

"I can write little more than an apology for my *not* writing! I fear my success among Diptera has not been brilliant. You must remember, however, that until I arrived here, I had no opportunity for hunting out the best localities, and working steadily at the Diptera alone; and I got here too late for doing much. Still I have a good many flies, besides those sent from the Portage. Of *Culex*, *Tabani* and black flies I get enough too readily. — Next March I go on snow-shoes and with dogs across Slave-Lake to Fort Resolution, that being a much better locality for zoölogical operations generally than Fort Simpson. For Diptera it will be capital. Then you may expect new ones! — Your dear (!) brother dipterologist Walker appears to have received the larger part, if not all of the insects, sent from this region. I hope you will enjoy the task of identifying my specimens with his 'descriptions'!" [In this part of the letter Kennicott introduced an account of his projects for the next two years. His great desire was to reach the Yukon River and Alaska, which, at that time, was still in possession of Russia. He continued:—]

"While wintering at Peel River I hope to make an excursion with an officer in charge to the Esquimaux on the coast. Some Esquimaux came up to Fort Simpson this fall, when I taught them to skin birds well, and they promised me lots of fine specimens. As they expect highly valued articles in return, I doubt not their sending me a lot of things from [illegible] River, where the officer in charge will receive and pay them for me. I find the Esquimaux far more intelligent and good-looking than the Iranians here."

"To Prof. Baird I must refer you for an account of my life here. The packet closes in a few hours, and I have been so lazy as to be obliged now to shorten my correspondence to the greatest possible degree. I have not yet sufficiently elaborated any notes on the country for sending them home, but will try to send something by the spring packet, which reaches Red River in July. I only send to Prof. Baird a few notes on birds and mammals."

"I am having an interesting life of it here, and yet plenty of employment. 'Tis harder work collecting in the North than at home! But I have not had a fair chance yet, and hope for great things next summer. I am getting some *rarae aves* among vertebrates this winter. I dress à la *savage*, camp out for several nights together in the snow, with only two blankets, and eat *tallow* (that is, when I can get it!) like an Esquimaux. We get no fresh meat, and from chewing tough dried *caribou*, the muscles of my jaws are developing extraordinarily!" etc.

Kennicott must have been younger than I, and therefore not yet forty, when he was found dead under a tree (from heart-disease?) in May, 1866, at Nulato, on the Lower Yukon.

"In this dispensation of Providence, science has lost an ardent and successful votary, and the Institution one of the most valued collaborators. It is to him that we owe our introduction to the most important sources of information relative to the fur countries, and it is principally through his exertions that the museum of the city of Chicago, of which he was the director, received its endowment and organization" (Professor Henry, in the *Smithsonian Report*, 1867, p. 42).

Honor to the memory of Robert Kennicott!¹

IV SHORT NOTICES OF BENJAMIN D. WALSH AND HOMER F. BASSETT, CORRESPONDENTS IN MY WORK ON CYNIPIDAE; OF DR. WILLIAM LE BARON (1814-1876), GENEVA, ILLINOIS; AND OF MR. E. FOREMAN, WASHINGTON, D. C.

Benjamin D. Walsh was born at Frome, Somerset, England, on September 21, 1808; he graduated at Trinity College, Cambridge; came to America in 1838, purchased a farm in Illinois, and settled afterwards, in 1851, in Rock Island, Ill., where he died on November 18, 1869, in consequence of a railway accident due to his own imprudence. His wife, whom he had married in England, survived him; he left no children. Farming, a lumber business, and later house building and ownership, were his successive occupations, and brought him a sufficient competence. In politics he was a zealous Radical Republican. He was universally esteemed for his high character, and loved for his unusual good spirits. At the same time he was in many respects an eccentric. He had paid some attention to entomology in his young days, before leaving England. But he took up this science again only twelve years before his death. The amount of work he accomplished, within

¹ I suppose that a detailed biographical notice about Kennicott was published after his decease, although I have not succeeded in finding any among the literature accessible to me. My purpose is merely to put on record my personal gratitude to him.

that short period, is astonishing. He voluntarily discharged the duties of State Entomologist of Illinois during the last years of his life, and as such (Acting State Entomologist) issued his First Annual Report for 1867. His final appointment, with a salary of two thousand dollars per annum, was, after much delay, confirmed by the State Senate but a short time before his death.

My relations with this remarkable man lasted for nearly ten years. Between May 21, 1860, and October 27, 1869 (just three weeks before his death), I kept up a most active and instructive correspondence concerning *Cynipidae* with him. The *sixty-five*, generally very long, letters from him in my possession would alone fill a volume. In these letters he often assumed a joeular, rollicking tone of which the following is a specimen: "By the way, if you have your photograph executed *head and shoulders only*, I wish you would be kind enough to send me a copy. You formerly let me have a photograph of full-length size, but, — to tell you the truth, — I do not take any interest whatever in the sartorial and sutorial questions of what style of breeches and boots you wear." I never had the pleasure of meeting Walsh, but, on my return from California, in August, 1876, I went to Roek Island for the sole purpose of paying a visit to his grave. An obituary notice of him by his, at that time, young friend and collaborator C. V. Riley (who later became the greatest economic entomologist of the century) will be found in the *American Entomologist*, St. Louis, Missouri, Vol. II, December, 1869, p. 65-68. A very good portrait is added to it.

It is a strange coincidence that Walsh, Riley, and Walsh's successor as State Entomologist, Le Baron, all met with a more or less unnatural death: Walsh, from a railway accident, Riley from a fall with his bicycle, and Le Baron from the consequences of a sunstroke.

One of my most worthy correspondents and contributors to my general entomological work was Dr. William Le Baron, of Geneva, Ill. I reproduce two passages from an obituary of him which appeared in a local paper and was sent to me after his death. It worthily reflects his moral merit as a man in the opinion of his

fellow-citizens: "Le Baron was born in Andover, Mass., in 1814; graduated at Harvard Medical College in 1837, was united in happy marriage with Miss Sarah Carr in Roxbury, Mass., in 1841; came West, and settled in Geneva in 1844; was appointed State Entomologist by Governor Palmer in April, 1871, which office he held for four years. In the summer of 1874, the Doctor suffered from a sunstroke from which he never recovered." He died in 1876, after having spent thirty-two years in Geneva. The obituary adds: "Geneva is a better place to live in to-day than it would have been had Dr. Le Baron never been known in it. The influences of the lives of such persons as he was compose the best element in the social and moral atmosphere of a community."

A very interesting, detailed biography of Dr. Le Baron, by Dr. F. W. Goding, appeared in *Entomologica Americana*, Vol. I, p. 122 (1885).

My correspondence with Le Baron lasted from 1859 till 1872. The earnestness of purpose and conscientiousness which characterize his letters fully justify the high reputation which he enjoyed among those who personally knew him.

With Mr. Homer F. Bassett, of Waterbury, Conn., a remarkably keen observer who anticipated Adler's discovery of the dimorphism of the *Cynipidae*, I had a very active and instructive correspondence between the years 1864 and 1867. It was continued, although with interruptions, after my departure for Europe in 1877. I am happy to say, that, while preparing this "Record," I received a letter from Mr. Bassett, dated from Waterbury, July 15, 1898, and informing me that, although two years older than myself, he still hopes to publish descriptions of a large number of new *Cynipidae* discovered by him.

[Since this was written in 1898, *Entomological News*, of Philadelphia, September, 1902, records the sad news of the death of Mr. Bassett, on June 28, 1902, in Waterbury, Conn. A biographical notice and an excellent portrait are appended.]

Among other men of science who have been useful to me in contributing to my studies on *Cynipidae*, I owe a debt of gratitude to Mr. E. Foreman of the United States Patent Office in Wash-

ington, with whom (between 1856 and 1861) I took frequent walks in the environs of that city. He taught me to distinguish the numerous species of oaks occurring in the United States, and procured me many new galls and other vegetable deformations.

My first paper on American galls (14, 1862) was published in the *Proceedings of the Entomological Society of Philadelphia*, at that time a young Society, which had been founded only the year before (compare the following Chapter V).

V A CONTRIBUTION TO THE HISTORY OF THE ENTOMOLOGICAL SOCIETY OF PHILADELPHIA

Residing, as I did, in Washington (up to 1862), I had the opportunity of witnessing the origin and the first beginnings of the Society. To me, a European, these beginnings afforded such a remarkable insight into American energy and enterprise that I am glad to give a short account of them.

There was no entomological society in existence in the United States in 1861. A number of friends of entomology in Philadelphia started the idea of forming such an association, and this undertaking soon found a sympathetic response from other parts of the country. Numerous written communications, donations, and specimens began to flow in after the first meeting of the new association on the 11th of March, 1861. The *Proceedings of the Society*, Vol. I, p. 2-6, contain the account of our first four meetings. The want of funds which impeded at first the progress of the Society was relieved by Dr. Thomas B. Wilson, "one of the earliest and most devoted members" who, "from the very commencement of this Society," gave it a "firm and unwavering support, by contributing to its Library and Cabinet, and by large endowments in money" (*Proc.*, Vol. IV, p. vii, 1865). Dr. Wilson, unfortunately for the Society, died four years after its foundation. The most active member of the Society, however, was its Corresponding Secretary, Mr. Ezra T. Cresson, with whom I kept up at that time an active correspondence. I shall content myself with reproducing passages of his letters, which speak for themselves.

On September 4, 1861, Mr. Cresson wrote: "We do our own printing, as you already know; I am the compositor and also assist in the press-work, and although I have had little or no experience in setting type (I have set the type for *all* the pages of the *Proceedings* thus far), yet be assured that I will do my best to have your paper got up in as neat and scientific a style as possible, although we have but three kinds of type, *Long Primer*, *Minion*, and *Minion Clarendon*.—The next number will appear at the regular time, unless something unforeseen should happen."

September 26, 1861: "Please excuse me for troubling you with a proof of *one single page only*; the reason I send this single page is this. On Monday evening last Dr. Wilson, with his usual liberality, presented the Publication Committee with enough money to purchase a press which will print four pages at once. We now have the press at the room, ready for action, and now as we have sent you the proof of the first *three* pages (which by the way Le Conte has returned corrected) we send the enclosed to make up the fourth, so that we may go on and print them off, as soon as you return this one corrected. By the time this is corrected and returned, *four more* pages will be on their way to you for correction, and so on until the paper is finished. So please return the enclosed as soon as you possibly can. The 25 copies *extra* will, with much pleasure, be forwarded to you as soon as the paper is finished.—If you should have any paper hereafter that may require lithographic plates (such as larvae, etc.), we will bear the expenses of getting them up and printed off, that is if the drawings are sent to us. We have a young artist, a member, who has undertaken to do all the lithographing for us free of cost; all we have to pay for is the press-work and paper.—Now as we have a large press, we are ready for any amount of material!"

The *large press* enabled the Publication Committee to print four pages at once; and yet, the small hand-press, with the type set by Mr. Cresson, from which the single pages of the first issues of the *Proceedings* were struck off, produced work which can stand comparison with the best work of an average typography.

November 12, 1861: "Please find herewith 25 extra-copies of your valuable paper. I hope that you will excuse any errors that

may be found, and also if the general appearance may not be so uniform as books generally are, but please remember that we are not printers by trade, but only greenhorns, and have to learn everything by experience. We hope to improve as we progress. We never dampen our printing paper, because it removes the glossy whiteness. We use the best ink and paper, we only want experience to make a *tip-top* job of it. By doing the printing ourselves we save a great deal. For instance, our first number cost us only about \$11; the second, \$8.50; and the third, \$10. — Total, only \$29.50, say \$30, for a pamphlet of 72 pages and 250 copies! We have already saved nearly \$80 by working the affair ourselves. — We have plenty of material for No. 4 (November and December, 1861), but we have none for No. 5 (January and February, 1862). Can you give us something for this number? — I hope that we will receive the earnest encouragement and co-operation of all entomological authors, for our Journal can hardly fail to be a valuable medium through which to diffuse entomological science."

I have before me *seventy-two* letters of this kind, in the neatest handwriting, which I received from Mr. Cresson between 1861 and 1869; twenty-one of them were written during the year 1862 alone. I was delighted to have my papers published by the Society, for, so far as my knowledge of typography goes, I could not have found elsewhere in the United States a special periodical with such a "*tip-top*" printing as theirs (besides my natural desire to encourage such exemplary scientific zeal). For the first volume (between October, 1861, and February, 1863) I furnished *five* papers, occupying nearly one hundred pages of the volume. Among them are my papers on the larvae of *Mycetophilidae* and on those of some rare *Coleoptera*, both accompanied with plates. My series of papers on *Cynipidae* and several other papers appeared in the same *Proceedings*.

I am glad to have found this opportunity for inserting in my "Record" an expression of my gratitude and appreciation to Mr. Cresson and his associates at that time!

VI WINNERTZ AND LOEW

Loew and Winnertz had been for several years in active correspondence on the subject of *Cecidomyiae*, and Winnertz, as Loew must have known, was preparing a monograph of this family when, to Winnertz's surprise, Loew anticipated him by the publication of a monograph of the same family in his "Dipterologische Beiträge," No. IV, 1850 (*Cecidomyiae*). Loew's prolonged correspondence with Winnertz, and the great amount of information he received from him, are entirely ignored in this work; in fact, the name of Winnertz is not mentioned, except in one or two insignificant passages (as, for instance, on p. 18, about *Sciara tili-cola*). Three years later, Winnertz published *his own* monograph, and the manner in which in this work he refers to Loew's publication, without the slightest reproach or malignant allusion, may serve as a model of dignity and moderation. In my "Introduction" (p. 2) I have mentioned the visit I paid to Winnertz at Crefeld,¹ in 1856, on my way to the United States. I was greatly impressed by his grave and dignified demeanor. He told me that he regretted having been obliged to give up the instructive correspondence with Loew, but that after Loew's behavior towards him he felt repugnant to continue it.

I am not aware that any biographical notice of Winnertz has ever been published. In Hagen's "Bibliotheca" I find that *Johann Winnertz*, merchant at Crefeld, was born in January, 1800. The date of his death is unknown to me. I find him mentioned among the members of the *Stettiner Entomologische Gesellschaft* as late as 1890, but this is evidently an error. Winnertz's collection, after his death, came into the possession of the University of Bonn, where I saw it in June, 1881.

It would be difficult, from the existing data, to form an opinion upon the extent of Loew's plagiarism in this case. That it must have been considerable may be surmised from Loew's own ambiguous statement (*loc. cit.*, p. 2): "Although I cannot boast of having had many opportunities of observing in the open air, I have seen

¹ *Crefeld*, like *Cöln*, is spelled *ad libitum*, with *K* or *C* in German.

things ('ich habe manches gesehen') that had escaped the attention of others, and I have formed opinions on the systematic question, which may perhaps prove useful for its further study." Loew, who was a zealous, one may say an *impetuous*, collector, was by no means an habitual observer of life (Loew as a *collector* I shall discuss in my Chapter XVI, "Characterization of Loew as a Dipterologist"). For this reason we have a right to suspect that the abundant material in observations from life, contained in Loew's monograph, was largely due to unaeknowledged contributions of Winnertz.

As to the "opinions" which Loew pretends to have formed "on the systematic question," he had likewise borrowed them from another author. Loew adopts, in his paper of 1850, the subdivision of the *Cecidomyiidae* which Rondani had introduced ten years earlier, in 1840, a subdivision which the same Loew (in the *Stett. Ent. Zeit.*, 1847, p. 146-147) had at first attempted to disparage in contemptuously calling it "splitting" ("Zersplitterung").¹ There is very little doubt that this complete change of opinion within three years Loew likewise owed to his correspondence with Winnertz.

It sounds like sarcasm, and probably was meant for it (upon the principle of *humanum est odisse quem laeseris*), when Loew, at the very beginning of his monograph of 1850 (p. 2) inserts the following grandiloquent passage: "There is hardly any other science which, like Natural Science, can render as exact an account of the Masters who, during its gradual edification, have contributed their share of work to the different parts of the structure. Hence, the names of Old Masters are more faithfully remembered in this, than in any other science, and the veneration of these names, stamped as they are on each building-stone, has become for posterity a custom which I shall attempt to fulfil here, so far as the literary resources accessible to me will allow."

Crefeld, the residence of Winnertz, is a flourishing town, at a short distance from the Rhine, not far from Düsseldorf, and has now (1900) 106,000 inhabitants. It is an industrial centre for

¹ These facts will be reproduced with more detail in Chapter XIX, "Camillo Rondani and his relations with Loew."

silks and velvets. Winnertz's usual and most abundant collecting-ground was a covered bowling-alley in his garden, one side of which was protected by a glass partition. Upon this glass wall he used to catch a profusion of *minutiae*, like *Chironomus*, *Ceratopogon*, *Mycetophilidae*, and *Cecidomyiae*. His catching-apparatus consisted principally in a glass-tube, the cork of which was moistened with a drop of ether. The specimens were thus brought home in perfect condition, and immediately pinned on silver (or platinum?) wire. For drawing wings, antennae, and other parts of his specimens, Winnertz had constructed a *camera* by which the magnified image of the object was projected on a horizontal, transparent film, upon which the drawing was traced, in its minutest details, with the point of a needle. The drawings were afterwards mounted on a black sheet, and thus became distinctly visible. Besides the published copperplate No. 1 of his work on *Ceratopogon*, which represents the tips of the tarsi and the palpi of some of the species, Winnertz showed me, as far as I remember, other drawings of details which have remained unpublished, as their publication had been considered too expensive. I have made a vain attempt, by means of inquiries at Crefeld and Bonn, to recover those drawings.

Of the *seventy-seven* species of *Ceratopogon*, described by Winnertz, about *two thirds* are new species.

A *unique* instance of Winnertz's painstaking and conscientiousness is the "Tabellarisches Verzeichniss der diagnostischen Verhältnisszahlen" on pages 13 and 14 of his Monograph. The amount of work bestowed upon the micrometric measurements of such a large number of the minutest objects is incredible. Winnertz told me that these two pages cost him more work than the labor of the whole monograph. But Winnertz's genius made him feel that this exertion was not useless. The excellent Schiner paid him a due tribute of admiration, when he said: "Die Masse sind so richtig und zur sicheren Bestimmung so werthvoll, dass ich sie überall anzuführen mich für verpflichtet erachte"¹ (Schiner, Fauna Austriaca, II, p. 575, footnote; paragraph on the genus *Ceratopogon*).

¹ (Translation). "The measurements are so accurate, and so important for the exact determination of the species, that I feel bound always to quote them."

Winnertz's work on *Cecidomyiidae* and *Mycetophilidae* has found many successors, but that on *Ceratopogon* has not. A great deal remains to be done for the classification of this group, so rich in European and exotic forms, some of which are remarkable for the great variety of their general organization, as well as for the manifold modes of their transformation. But it will be difficult for any future worker in this branch to surpass the genius and the conscientiousness of Winnertz!

VII REMARKS, HISTORICAL AND CRITICAL, CONCERNING LOEW'S FIRST VOLUME OF THE MONOGRAPHS OF NORTH AMERICAN DIPTERA, WASHINGTON, 1862

The first volume of the "Monographs of North American Diptera" begins with the chapter *On the terminology of Diptera*, p. xi-xxiv, followed by a *Sketch of the systematic arrangement of Diptera*, p. 1-48. These papers were written by Loew at the special request of Professor Henry (of the Smithsonian Institution), who wanted some kind of Introduction to Dipterology at the head of the intended series of the *Monographs*. This very natural request found Loew utterly unprepared. In a letter to me (July 25, 1860), he said: "I undertook this work with fear and terror ('Furcht und Grauen'), because the existing Systems are not acceptable, and the difficulties of the introduction of a new one can be understood only by those who know the lamentable state of things we are in ('wie alles hier noch im Argen liegt'). Just by such a work one may expose oneself to very disagreeable criticisms! For a long time I have been hesitating whether I should not altogether get rid of this, to me most repulsive, task ('mir sehr widerwärtige Arbeit'). Now, after much effort, the 'Opus' is completed ('das Opus ist nun fertig'). That I have learned much during this work is certain; whether others will learn much from it — is doubtful ('ob auch andere daraus viel lernen werden, zweifelhaft'). As an unmistakable result I have obtained the confirmation of my old convictions: (1) That the venation must be maintained as the chief basis of classification; (2) That the

principal subdivision of the Diptera into *Nemocera* and *Brachycera* must be given up, as soon as we attempt the introduction of a more or less natural system. The first I have accomplished; the last I have left undone, in order not to deviate too much from the accepted arrangement ('um nicht zu sehr von dem Gewohnten abzuweichen')".

Loew was fortunate in having left undone his proposed reform of the system, for, far from being a progress, it would have been a step backwards from what Latreille had established in his "Genera" (1809), half a century before! I have exposed this error in my article in the *Berl. Ent. Zeit.*, 1892, p. 421 (130, 1892) and have also mentioned it in the "Introduction" of this "Record" (p. 11).

The other *conviction* of Loew, that the *venation* should be considered as the chief basis of classification, should likewise be accepted with some caution. It is in consequence of his reliance on this character, taken separately and without regard to the whole organization, that Loew, in 1844, pointed out most unnatural affinities between *Liponeura*, *Macropeza*, *Diamesa*, *Orphnephila* (*Chenesia* Macq.), and *Simulium* (*Stett. Ent. Zeit.*, 1844, p. 121-122). In the next year, 1845 ("Dipterologische Beiträge," No. I) he proposed his subdivision of his *Tipula gallicola* into *Polyneura* (*Psychodidae*) and *Oligoneura* (*Cecidomyiidae*); and, as late as 1860 ("Dipteren-Fauna Südafrika's") he introduced his subdivision of the *Cyrtidae* into two sections, which proved untenable (all these criticisms are developed more in detail in my Chapter XVI: "Characterization of Loew as a Dipterologist").

During this period of his career (1860-1862) Loew seems to have been completely in the dark about the system of classification to adopt. He confesses it candidly in his above-quoted letter (July 25, 1860) and shows it in his publications. In his "Dipteren-Fauna Südafrika's" (1860, Preface, p. v) he acknowledges his perplexity, and adds that his views come nearest to those of Haliday (in Walker's *Ins. Brit. Dipt.*, Vols. I-III, 1851-1856). Nevertheless, Loew placed *Thereva* between *Tubanidae* and *Asilidae*, while Haliday considered *Thereva* as a Bombylid, which is certainly nearer the truth. Both authors placed the *Leptidae* between the *Asilidae* and *Bombyliidae*, which cannot be justified.

In the Monographs, etc., Vol. I (1862), Loew gives up Haliday's guidance, and arranges the families apparently hap-hazard. In the *Nemocera*, Haliday had placed the *Mycetophilidae* (including *Sciara* and *Zygoneura*) next to the *Cecidomyiidae*, which is correct. Loew separated these two families by a long interval, inserting the *Blapharoceridae*, *Psychodidae*, and *Tipulidae* between them! Among his *Cecidomyiidae* we find *Zygoneura* and *Diamesa* Meig.! The latter he considers a synonym of *Lestremia* Macq.! (*loc. cit.*, p. 7).

The *Brachycera* were arranged by Haliday in Walker's work (1856) as follows: *Stratiomyiidae*, *Xylophagidae*, *Tabanidae*, *Aceroceridae* (placed here on account of the three pulvilli), *Asilidae*, *Leptidae*, *Bombyliidae*, *Scenopinidae*, *Empidae*, *Dolichopodidae*, *Lonchopteridae*, *Platypezidae*, *Pipunculidae*, *Syrphidae*, *Conopidae*; after which follows the large division *Muscidae*. Although Haliday correctly placed the *Syrphidae* just before his *Muscidae*, it is strange that he did not recognize the importance of the generalization introduced by Latreille (1825) with his division *Athericera*, embracing both *Syrphidae* and *Muscidae*, and founded not only upon their mode of transformation, but upon characters of the imago which have been overlooked by later authors.

Loew, in the same volume of the Monographs, made an incomprehensibly retrograde step in placing the *Syrphidae* quite far from the *Muscidae*, and in introducing the whole series of the *Lonchopteridae*, *Hybotidae*, *Empidae*, *Tachydromidae*, and *Dolichopodidae* between them! He seems to have been entirely unaware of the existence of the large division *Athericera* Latr. (1825), and when Brauer adopted the division in *Orthorrhapha* and *Cyclorrhapha*, the latter of which coincides with Latreille's *Athericera*, Loew accepted this subdivision as an unexpected novelty! (Compare my "Suggestions towards a Better Grouping," etc., in the *Ent. Monthly Mag.*, 1891, p. 35, 121, 1891.)

In consequence of these arbitrary arrangements in the Monographs, Vol. I (1862), Loew did not escape the "disagreeable criticisms" he was so much afraid of. Schiner published a rather severe one (*Verh. zool.-bot. Gesellsch.*, Wien, 1864, p. 209), although the "System" proposed by himself was not much better. In the Novara work (1868) Schiner was perfectly right in adopting

Haliday's concept of the *Empidæ* which includes the *Hybotidæ* and *Tachydromidæ*.

In the preface to the *second* edition of his earliest work "On the Diptera of the Environs of Posen" (1840), which I have reproduced in the *Berl. Ent. Zeit.*, 1896, p. 282-284, Loew, after pointing out the deficiencies of dipterology of that time, foreshadows a programme of his future work. He says: "I should have been glad to add remarks on the anatomy, biology, and life-habits to the enumeration of the species; but this would have made the defects of the adopted System of Diptera so glaring that one would have felt tempted to propose a new arrangement, an undertaking which is not within my intentions at present, and which would have required many extensive preparatory researches," etc. "However, I shall mention here, as a preliminary result of a considerable number of anatomical researches, that the great importance which Meigen attached to the structure of the antennae and of the wings seems to have been the principal reason why the System introduced by him appears naturally incoherent, as soon as we take the inner organization ('innere Organization') into account. Without denying the true value of these characters, and without refusing them a practical value,¹ it would seem that a closer study of the manifold and easily accessible peculiarities in the structure of the thorax and of the abdomen might have brought about an arrangement in many respects more natural," etc. In spite of such anticipations and promises of a better system, Loew, after an interval of *twenty years*, produced an arrangement which, far from being an improvement, was a deterioration of the existing one! Loew's talent lay in the direction of the particular, *the minute*, and not in that of *generalization*. He felt it quite well himself when he wrote me (November 10, 1862): "*Minutiae* are my specialty, particularly when they fall within the domain of the *Acalyptrata* ('Die Minutien sind so recht mein Fach, besonders wenn sie in das Reich der *Acalypteren* fallen')." Loew's best work is certainly his numerous papers relating to this group.

¹ "Ohne diesen Merkmalen ihren wahren Werth zu nehmen, und ohne ihren praktischen Werth abzuleugnen zu wollen," etc. By *practical value* Loew meant, I suppose, the value of the characters for determining specimens.

VIII HALIDAY AND LOEW

Besides the scanty data on Haliday's life and work, contained in the "Obituary Notice" in the *Ent. Monthly Mag.*, 1870, p. 91, I am not aware of the existence of any more detailed account of this remarkable man and eminent entomologist. I shall attempt, therefore, to supply this want in a certain measure by giving a survey of his relations with Loew, which have exercised a considerable influence on the work of both of them. Haliday and Loew were contemporaries, being born in the same year, 1807, but Haliday's first publication dates from 1831, nine years before Loew's, and he had already published several valuable papers on Diptera when Loew appeared upon the stage.

There are several strange coincidences in the chronology of Diptera: The four great dipterologists of the century were born nearly at the same time: Loew and Haliday in 1807; Zeller and Rondani in 1808. Loew and Rondani died in the same year, 1879, Haliday nine years earlier, 1870; Zeller survived them all, and died in 1883. The first publications on Diptera by Loew, Rondani, and Zeller appeared in the same year, 1840.

I have called attention in the *Berl. Ent. Zeit.*, 1895, p. 168 (144, 1895) to similar coincidences in the dates of the discovery of some remarkable forms of Diptera, as *Scenopinus*, *Orphnephila*, and some of the *Blepharoceridae*.

The interesting article of C. L. Marlatt: "Brief Historical Survey of Entomology" (*Proc. Entom. Soc. Washington*, Vol. IV, No. 2, 1898), contains a chronological table of naturalists, and especially of entomologists, which shows that the years 1807-1809 were particularly prolific of them. Haliday and Rondani, born in 1807 and 1808, should have been added to this list.

My sources of information on the subject of the relations of Loew and Haliday consist in the mentions of Haliday in Loew's letters to me, as well as in the very valuable collection of letters of Haliday to Loew, now in possession of Mr. G. H. Verrall in Newmarket, England, which he most generously loaned me for study. To work up exhaustively this most abundant supply of information on the mutual influence of both entomologists, afforded by this correspondence, is a task beyond my power. I shall keep within the bounds of my present unambitious purpose.

The earliest letter contained in Mr. Verrall's collection is dated from Clifden, near Belfast (Ireland), September 27, 1847. It begins

as follows: "In the course of this summer, your letter, bearing date the 16th of March previous, came to my hands, along with other matters sent me, through London, from the Stettin Society. It gratified me much to receive this mark of consideration from such a distinguished naturalist, and one I look up to as the first living authority on all things concerning that order [Diptera] to the study of which I am most inclined, as far as my materials extend. I am glad to find that my little contribution of specimens has been received with such friendly readiness, as I ventured to expect from your liberal temper; and I return you my best thanks for the very interesting and useful series of specimens, which you have had the goodness to transmit to me, containing many new and quite (to me) unknown species, and what I value not less, the authentic determination of important species." It appears from this passage that it was Haliday who opened the correspondence by sending to Loew a collection of specimens, that Loew had answered him in March, 1847, and that the first letter of Verrall's collection, dated from Clifden, was the *second* letter of the correspondence.

The rest of the long letter from Clifden shows Haliday's earnest desire to develop in Ireland an interest in scientific entomology, and to establish the necessary conformity between it and the continental literature in regard to terminology and nomenclature (including synonymy). To the letter are added manuscript lists of the Irish species of *Hydromyzidae*, *Dolichopodidae*, and of the genera *Scatopse* and *Tipula*; a list of supposed synonymies of Irish Diptera; a paragraph on *Orphnephila*, with tracings of an, at that time, unpublished plate with twenty-four figures; another paragraph on *Clunio marinus*, with tracings of the figures which were published later (1855) in the *Natural History Review*, and many incidental remarks and notices on genera and species of Diptera. A box with insects accompanied the letter, the catalogue of which is also crowded with synonymies and notices. One of these contains a very remarkable reference to the new subdivision of the *Psychodidae*, introduced by Haliday in Curtis's "Brit. Entom. Diptera," plate 745 (dated June 1, 1839); I shall refer to it in my Chapter, "Loew as a Dipterologist."

This letter of Haliday's with its annexes shows an immense

amount of work, set forth in a modest, inquiring, almost hesitating manner. It ends with the words: "And trusting that you will kindly excuse the length and desultory style of this hurried letter (encouraged by your permission to address you in English), I remain, etc."

In writing such a letter Haliday may be supposed to have expected a commensurate reply. The beginning of Haliday's next letter (dated from London, December 11, 1847) does not show that this expectation had been realized: "My last letter to you, although delayed in consequence of the change of Dr. Schaum's plans (who originally intended to have himself conveyed it to you when he returned to Germany) has, however, as I understand from Mr. Dohrn, been transmitted to you, and I am in hopes that, when time allows, you will favor me with the remarks of which you have given me hopes, on some of the species which have been referred to in the letters which have passed." Haliday gives an account of his visit to the British Museum and to the Linnean collection (in Burlington House). About the latter, he says: "Difficulties (which I will not here mention in detail) exist and involve great loss of time in the determination of the species, and unfortunately and unaccountably the latter half of the Order Diptera, from *Musca* 63 (11th Ed. Systema Naturae) are wanting. If I cannot find time and the requisite apparatus for going through the *entire*, I hope to determine *some* of the *debated* species." Haliday gives here some instances of his identifications and continues: "But I hope you will see more about this subject hereafter in the Stettin Journal." (The article referred to was published, in German translation, in the *Stett. Ent. Zeit.*, 1851, p. 131-145) *Apropos* of the *Stett. Ent. Zeit.*, Haliday notices in the number received by him Loew's article on Ruthe's publication on Diptera in Oken's *Isis* of 1831 (Loew, *Stett. Ent. Zeit.*, 1847, p. 66-71). With his usual candor, he at once acknowledges the right of priority of the name *Thaumalea* Ruthe over his *Orphnephila*, although they were *apparently* published in the same year, 1831 (I shall revert to this subject farther on). The letter ends with the words: "Believe me, with much desire to cultivate and enjoy your further correspondence and more intimate acquaintance, yours," etc.

Another letter from Haliday, of much later date (Paris, Hôtel de l'Empire, February 9, 1862), contains some facts of importance. I reproduce fragments of it:—

"I left London with my friends on Tuesday morning, reached Amiens that evening, spent the next day with Dr. Dours [hymenopterologist], and reached Paris by 11½ P.M. The next day I called on Dr. Sichel,— then of course to the Museum. . . . The next day at Dr. Sichel's (Hymenoptera), Saturday again at the Museum. In the parts of Meigen's collection to which I confined my attention (*Muscidae Acalypt.*) . . . the species represented are *few* beyond all my previous suppositions. It is evident that he derived more from v. Winthem's and Wiedemann's collections than from his own; or that the duplicates of which his heirs disposed to Mr. Winnertz included a material part of the whole. Yet even the little remaining I found to take much time to muster and survey; the more so from their imperfect condition. I first went through *Helomyza-Heteromyza* with your tables; then *Ephydra* and *Hydrellia*, hastily. The results I have copied out for you (*vide postscript*),¹ expecting to set out to-morrow. At present, however, the aspect of the weather, snow and frost . . . induces us to pause for one or for two days, before we prepare to encounter Mont Cenis. I shall therefore, I expect, have one more morning at the Museum. . . . I have not had a sight of Mr. Bigot till this afternoon." [In a footnote Haliday adds:] "Bigot has got from Corsica the most remarkable Nemocerous Dipteron I have ever seen." [which evidently refers to the Blepharocerid *Apistomyia* Bigot, published in the same year]. "I feel much obliged by the effort you made, under the instant pressure of business, to write to me. . . . I am glad to know the *Discomyza* arrived safe. Finding that the volume of Curtis's 'Farm Insects' might go . . . more safely through my booksellers, I have entrusted it to them . . . and hope it will reach you speedily. I shall receive with real gratification the present you propose for me of your 'South African Diptera.' . . . I expect to find his [Bellardi's] collection of Piedmontese Diptera very interesting. . . . He has been collecting long enough to furnish a good provincial collection. — There I must conclude. Ever yours sincerely," etc.

I shall give in the sequel further extracts from Haliday's letters concerning Loew's first volume of the "Monographs of North American Diptera," which appeared in 1862.

These few extracts from Haliday's correspondence afford much of interest, not only from the biographical, but also from the scien-

¹ A long, very painstaking enumeration of species, with numerous remarks and synonymies.

tific point of view. My often reiterated thesis that "beginnings are generally symptomatic of the beginner" has seldom been more strikingly illustrated than by Haliday. His *first* publication on Diptera was that on *Orphnephila* (1831), perhaps the most refractory genus of the Order. Haliday gave a description of it (with figures) which, in its completeness, is far in advance of most of the generic descriptions in the same Order before his time. With his characteristic indifference for notoriety, he seems to have acquired, some time afterwards, many new facts on the same subject without publishing them immediately. This appears from his paper, "Notes on Irish Diptera," published *a quarter of a century later* (*Natural History Review*, Proceedings, Vol. III, 1856, p. 73, Plate II), in which he submits to the Dublin University Zoological and Botanical Association "some notes and sketches of the internal anatomy of both sexes of *Orphnephila*, made many years since." A long time previous to this publication, in the letter quoted above (September 27, 1847), Haliday had communicated to Loew the drawing of a plate containing twenty-six figures of the internal and external anatomy of *Orphnephila*, only a portion of which were published in the plate of 1856, and the rest are unpublished to this day (I cannot make a detailed comparison, because I have before me only a manuscript extract of the paper of 1856, and no plate). This laborious piece of work is introduced in Haliday's letter to Loew in the following modest manner: "I send also tracings of some of the details of *Orphnephila*, which may enable your practised eye to pronounce on its affinities. Rondani, after seeing the insect, continued to be of opinion that it should form a distinct family; but this gives no information as to its true place, nor does he seem to me to be particular as to the series of his families, contenting himself with the definition of them," etc. I find that Loew, fifteen years later, in the "Monographs," Vol. I, p. 6 (1862), placed *Orphnephila* at the end of the *Chironomidae*, and added: "It differs from all other *Chironomidae* by the veins of the wings running without attenuation to, and the costal vein being continued round, the posterior border. If we do not establish a separate family for it, its proper place will be here, but as an anomalous genus." If Loew had offered the same solu-

tion to Haliday in 1847, I doubt if the latter would have been satisfied with it!

In the next letter to Loew (December 11, 1847, of which I have already given an extract above), Haliday took up the subject of *Orphnephila* again, and said: "I have (I doubt not) succeeded, in what I had long sought in vain, to find the larva of *Thaumalea*, though, just about to leave home on a journey, I had to leave it to the care of a non-entomological friend, and fear it will not be reared. It is half-aquatic, with anterior and posterior spiracles only, and though furnished with very singular, numerous, gill-like appendages in a double row down the back, seems to be not very distant from the larvae of *Ceratopogon*," etc.¹ This mention of the larva of *Orphnephila* has never been published, and Haliday himself does not refer to it, either in Walker's "Ins. Brit. Dipt.," Vol. III (1856), or in his "On Some Remaining Blanks," etc. (1857). There is no doubt that, later, he recognized in it the larva of the Psychodid *Ulomyia*, the description of which, as given in Walker, *loc. cit.*, Vol. II, p. 254, agrees with the statement made in Haliday's letter.

That Haliday, in the above-quoted passage of his letter, calls the fly *Thaumalea* (and not *Orphnephila*) is again a fine trait of his character. When he published his first paper on *Orphnephila*, he was not aware of the contemporaneous publication of Ruthe. As soon as he knew of it he acknowledged (in the same letter of December 11) its right of priority, because the number of the volume of the *Zoological Journal* containing Haliday's paper, although dated 1831, was not actually published until 1833, "the expenses of the Journal being greater than the returns, or from some other causes." Later, however, it was proved that the name *Thaumalea* was preoccupied, and Haliday's name was therefore retained.

The behavior of Haliday in the matter of *Orphnephila* affords an epitome of his whole scientific career: an intense craving for completeness and perfection, quite disinterested, because shy of

¹ This comparison with *Ceratopogon* is not appropriate, because the larvae of the latter genus are polymorphous, and were very little known at that time. Haliday, as appears in a footnote in the same letter, refers to the *Ceratopogon* larvae with spatulate appendages on the back, which have been described by Dufour (1845) and by Perris (1846).

publicity; a sincere desire of being useful, by imparting useful knowledge to others, unmindful of the amount of work it involved (and that even in cases where other *savants* would have kept quiet in order to secure priority), — all this, combined with an almost feminine coyness, hiding his free gifts under his initials, or in private letters. An exquisite specimen of Haliday's courtesy towards his critics is found in his paper, "On the Affinities of the *Aphaniptera*," etc., *Natural History Review*, January, 1856, p. 12. In a footnote Haliday quotes the following passage from Burmeister's "Manual of Entomology," Vol. II, p. 407: "Haliday . . . after the fashion of his compatriots, who seem to consider the multiplication of subdivisions as the highest aim of descriptive zoology, has made of the Linnean genus *Thrips* a new order which he calls *Thysanoptera*," etc. To this Haliday's answer was: "I am now quite disposed to acquiesce in Burmeister's sentence as regards the particular case, and I do not feel called on to protest against the sweeping generality of his obiter dictum."

I shall now attempt to bring out the degree of appreciation accorded by Loew to Haliday's merit, so far as I can gather traces of it in his publications and in his correspondence. In several passages of his works Loew pays Haliday a due tribute of recognition and gratitude. In the Monograph of the European *Ephydridinidae* ("Neue Beiträge," Vol. VII, 1860, p. 2 at bottom), Loew regrets that Stenhammar (1844) had not made use of the valuable work of his predecessor Haliday, "who, for many years, has made a successful study of the British species; . . . the results have been incorporated in Walker's 'British Diptera,' and constitute, in the shape in which they appear in the Appendix to the third volume (p. 343-346), the most finished and perfect work on the systematic arrangement of this family in existence at present." A similar acknowledgment will be found in Loew's paper on *Dolichopodidae* ("Neue Beiträge," Vol. V, 1857, p. 1): "One of the principal publications in the domain of dipterology during the past year is the 'Insecta Britannica. Diptera,' by Walker, now completed with the third volume. An invitation to publish a review of this work (which appeared since in the *Natural History Review*) offered me the occasion to make a thorough study

of this work. I was always particularly attracted by the family of *Dolichopodidae*, which, worked up by Mr. A. H. Haliday in Dublin, is the most finished portion of this publication. It deserved a detailed study, and has furnished me with much information. A very interesting, and for me most instructive, correspondence with Haliday himself, which, besides being an interchange of ideas on many other points concerning classification, was principally devoted to the systematic distribution of the *Dolichopodidae*, convinced me of the necessity of a further development of this distribution, and of a more precise definition of the genera hitherto uncertain and unsteady." This tribute to Haliday did not prevent Loew from exhibiting his own egotism in the "Monograph of the Dolichopodidae" ("Monographs of North American Diptera," Vol. II, 1864). In this volume, intended principally for America, all the genera are treated as if they were his own; the real authors, Meigen, Macquart, Zetterstedt, Stannius, Wahlberg, etc., are not mentioned at all. Haliday's genera *Aphrosylus* (p. 148) and *Campsicnemus* (p. 193) will appear to the reader as if they were Loew's. The occasion when Haliday's name is mentioned (p. 148) is when Loew discards the generic name *Machaerium* Hal. published in 1831 and illustrated by a beautiful figure, in order to replace it by a new name, *Smiliotus* Loew, and that, under the unfair pretext of the former name being preoccupied in botany! In the preface of the same volume (p. iv) Haliday's name is introduced in a purely perfunctory manner. Thus for the mere gratification of his vanity, Loew has deprived his readers of the advantage of finding in his work the history of the different genera, and has unnecessarily exposed them to the risk of a vexatious bibliographical search.

Another compliment was paid by Loew to Haliday in the *Wien. Ent. Monats.*, 1864, p. 124, where he says: "My attention was first called by Haliday himself to the probable identity of his *Tipula dispar* with *Tip. payana* Meig., and after a careful inquiry I must agree with the opinion of this most clear-sighted of entomologists." In this instance I must interfere with my own egotism, and introduce the following explanation: *Tip. payana* Meig. (described originally only in the male sex) is very common in autumn in the

environs of St. Petersburg, and I had no difficulty in ascertaining that a female *Tipula* with abortive wings, which occurred abundantly in the same localities, was its female. At the same time I recognized this female in the *Tipula dispar* Hal. (*Entomological Mag.*, 1832-1838, p. 155), and communicated this identification to Winnertz, who informed Walker of it. As this happened before 1856, Walker published in his "Ins. Brit. Dipt.," Vol. III, p. 342 (1856) at bottom, in the Errata, the following note: "*Pagana* Meig. Syn. *dispar* Hal. — On the authority of Count Osten Saeken, communicated by Winnertz." There is no doubt that Haliday's communication to Loew was based upon this passage.

Considering all the services rendered by Haliday to Loew, it would have been the latter's duty to publish a detailed appreciative obituary notice of Haliday's life and especially of his work, after his death in 1870. Nobody was better fitted for such a task than Loew; nevertheless, he did nothing of the kind.

It was probably at the request of Loew that Haliday in his letters communicated to him a series of remarks on the contents of the first volume of the "Monographs of North American Diptera" (1862). I reproduce here some of these remarks, with the comments necessary for a proper understanding of them.

(1) To "Monographs," I, p. 24, Therenidae. Haliday says: "Why not *Therevidae*? *Thereva*? It is simply a mistake of Agassiz ("Nomenclator") to cite *Thereua* as Latreille's original form; and, if it had been so, analogy would require the correction in *v*" (Haliday).

In my "Catalogue North Amer. Diptera," 1878, p. 96, and also p. 239, Note 168, I have likewise protested against Loew's *Thereua*; nevertheless Loew continued to use it to the very last, without giving any reason for it. I shall insert here, once for all, the result of my investigation of this matter. Loew has *Thereva* in his "Posener Dipteren" (1840). *Thereua* appears for the first time in the *Stett. Ent. Zeit.*, 1844, p. 126, 127 (passim), in an article on the Leptid *Baryphora*. It must have been suggested to Loew by Agassiz's "Nomenclator" in manuscript, of which the part on Diptera had passed through his hands (as is proved by the words on the titlepage: *recognovit Loew*). The printed text cannot have been in Loew's hands in 1844, because it contains references to *Eclimius* Loew, *Haemasson* Loew, both of 1844, *Nygmotodes* Loew, *Posthon* Loew, both of 1845. These references were evidently added by Loew himself in the manuscript. But, after Loew, a philologist overhauled the "Nomenclator," and this gave rise to Loew's complaint in the *Berl. Ent. Zeit.*, 1868, p. 382, and 1876, p. 209, that he had spoiled his *Eclimius*, derived from *ἐκλιμος* (*famished*), and not from *ἐκλειμμός* (*quod restat*). As to *Thereua*, Loew agreed with the philologist, the more so as Bouché, in his "Naturgesch. der Insecten," p. 45,

46 (1834), had written *Thereua*, probably on philological grounds. — At any rate, Haliday was right in restoring Latreille's original spelling.

(2) "Monographs," I, p. 33, line 16 from bottom, Loew says: "There is no possibility, it seems, of discovering any other constant character [than the size of the tegulae] to distinguish Muscaria Calyptratae from Acalyptratae"; "that which appears most serviceable was pointed out to me by Mr. Haliday; it is the transverse suture of the thorax being usually of the same depth on its whole extent in the Calyptratae, whereas in the Acalyptratae it is generally distinct at each side, and imperceptible in the middle." To this Haliday replies: "The character derived from the obliteration of the suture of the mesonotum is already noticed by Meigen. . . . The *Micropezidae*, so far as I know, and some genera which seem to belong to the *Ortalidae*, have it complete. These exceptions, however, do not much affect the value of the character." The only passage in Meigen that Haliday can have had in view is that in "Systematische Beschreibung," Vol. VII, p. 172 (1838), where three divisions of Meigen's family *Muscidae* are adopted. Meigen mentions the "Quernath" [transverse suture] as the character of the first two divisions, *Calyptratae* and *Anthomyzidae*, both of which have been afterwards united under the common name of *Calyptérées* by Macquart. (Compare O. S., *Berl. Ent. Zeit.*, 1896, p. 329.) It is strange that Macquart does not seem to have known this character when he introduced his final subdivision in 1843; he does not make any mention of it, and it seems to have been overlooked ever since, except by Haliday. The term furrow appears to me more appropriate for this structure than suture; there is no real suture about it.

(3) "Monographs," I, p. 42, *Phycodromidae*. Haliday says: "From this family-name I presume you give the preference to *Phycodroma* over *Malacomyia*, although the latter is anterior in date. It is a mistake of Agassiz to cite *Malacomyia* R.-D., which has no existence." — I have taken some trouble to interpret this passage with the help of a copy of Curtis's "Guide" (1837), annotated by Haliday, which he had sent to Loew in 1855, and which is now in my possession. On page 270 at bottom, under *Coelopa sciomyzina* Hal., I find a note in Haliday's handwriting: *fucorum* Zett., with the reference App. 280, which means Appendix to the "Guide," p. 280. In this Appendix we find *Malacomyia* Hal., without any other reference, and this is the first appearance of this generic name in literature. The definition of this subgenus is found in Haliday's article, "New British Insects," in Mr. Curtis's "Guide," which appeared in *Ann. Nat. Hist.*, Vol. II, 1838, p. 186. We find there, under *Coelopa*, the subdivision: "Arista villosa; epistoma acute porrecta: *Malacomyza* [sic! O. S.], c. App. 280." The species, answering to this subgenus is quoted thus: *Coelopa sciomyzina*, *Entomological Magazine*, Vol. I, p. 167 (1832). This reference brings us to Haliday's "Catalogue of Diptera about Holywood," etc., where a detailed description of the species is given. — The next appearance of *Malacomyia sciomyzina* is in Westwood's "Introd.," Vol. II (1840), "Generic Synopsis," etc., p. 144 (the notes to which were provided by Haliday); and again in Walker's "Ins. Brit. Dipt.," Vol. II, p. 157 (1853). Haliday was right in saying that it was a mistake of Agassiz to refer *Malacomyia* to Rob.-Desvoidy (1830), where, on page 651, there is the Tribe *Malacomyidae* but no genus of that name.

The dates of the Swedish authors are: —

Coelopa fucorum Zett., Dipt. Scand., Vol. VI, p. 2474 (1847).

Phycodroma, gen. nov. Stenh., *Handl. K. Vet. Akad.*, Stockholm, 1854, p. 270;

Separatum: 1855, p. 12. Type: *Coelopa fucorum* Zett.

Both references are later than Haliday's, and therefore he was right in vindicating his own priority. I find that Rondani, in his "Sarcophaginae Italicae" (Atti di Milano, 1847, p. 131), although under the influence of Haliday at that time, adopts *Phycodroma* Stenh. (which he spells *Phycodromya*), and places *Malacomyia* Hal. 1838 as a synonym. Nevertheless, on the next page, 132, he has: *P. hydro-myzina* Hal. 1832 (should be *sciomyzina*, comp. on the same page, line 8 from top) with *fucorum* Zett. 1847 as a synonym. Here, therefore, Rondani recognizes the priority of Haliday's publication.

(4) "Monographs," I, p. 129, *Ephydrinidae*. "Analogy would require Ephydridae" (Hal.).

Loew adopted this emendation in his later publications; compare, for instance, "Ueber Dipt. Augsb. Gegend," 1869, p. 47; also in *Berl. Ent. Zeit.*, 1876, p. 76.

(5) "Monographs," I, p. 149, line 16. Haliday corrects: "beneath the eyes" into "beneath the antennae," which is right.

Concerning the venation, Haliday observes about the origin of the auxiliary vein ("Monographs," I, p. xvi, lines 20-22): "Does not the auxiliary vein arise from the second main trunk?" Loew had stated that it issues from the first.

About the English of the "Monographs," Haliday said in the same letter: "It is always intelligible, although not always correct." His corrections are numerous, and display a remarkable grammatical acumen, but it would be superfluous to reproduce them here.

A short account of the relations of Haliday with Rondani will be found in my Chapter XIX.

I regret that I never met Haliday. In 1869 I sent him by post from New York to Lucca, where he resided at that time, a copy of my second "Monograph of the *Tipulidae brevipalpi*" which had just been published. The parcel never reached him, but he received the letter accompanying it, and expressed his regrets at the loss in a letter dated February 15, 1869. He died in July, 1870, and thus I lost the opportunity of meeting him during the winter I spent in Italy two years later.

Besides my gleanings from the letters of Haliday to Loew reproduced in the present chapter, I found in these letters some very interesting remarks and suggestions in regard to classification. I have reserved them for my Chapter XVI: "Characterization of Loew as a Dipterologist."

The list of Haliday's publications, up to 1857, will be found in Hagen's "Bibliotheca Entomologia," Vol. I, p. 334-336. A paper which I possess has been omitted in this list:—

"Recent Works on the Diptera of Northern Europe" (*Natural History Review*, Vol. II, p. 49-61, July, 1855).

Haliday seems to have published very little after 1857. I gather from the "Obituary Notice" (in the *Ent. Monthly Mag.*, London, 1870, p. 91) that Haliday's health compelled him, about 1860, to "seek the more genial climate of Italy," that he settled in Lucca, and that, although he continued to show the liveliest interest in entomology, "his contributions to entomological literature have been few." Of Haliday's publications after 1857, I possess:—

"Relazione sul Baco dell' Oliva. — Estratta dall' Agricoltore, periodico mensile del Comitato Agrario Lucchese." Lucca, 1868. (The subject is, of course, *Dacus oleae*.)

In 1869 Haliday translated into Italian Loew's paper on *Blepharoceridae*, which appeared in the *Bull. Soc. Ent. Ital.*, Vol. I, April, 1869. He added to it a postscript, entitled "Note sulla precedente memoria del Prof. Loew" (p. 99-101).

The portrait of A. H. Haliday is taken from an old photograph, kindly supplied by Prof. E. P. Wright of Dublin University. A block was made and a cliché forwarded to me. For this generosity I feel bound to express to Professor Wright my most heartfelt thanks! Mr. G. H. Carpenter (Science and Art Museum, Kildare Street, Dublin), to whose friendly mediation I owe this privilege, informs me at the same time that a large collection of letters to Haliday from Loew, Rondani, and other entomologists are also in the possession of Professor Wright! This will be good news for future workers on the same subject, but it comes too late for me to profit by it.

Alexander H. Haliday.

IX ON LOEW'S WORK ON AMBER DIPTERA IN GENERAL, AND ON THE CIRCUMSTANCES OF ITS ULTIMATE FAILURE

This chapter is principally based on my personal experience (including my correspondence with Loew), and, I trust, will throw some light upon this otherwise very obscure subject.

In undertaking the work of describing Amber Diptera, Loew was laboring under the strange delusion that fossil forms could be described before the classification of recent ones had reached a sufficient degree of development. Among the Amber Diptera, the *Tipulidae*, owing to the variety of their plastic characters, especially in their venation and in the structure of their antennae, afford the easiest field for study, and for this reason it was to this family that Loew gave the greatest development in his work of 1850 ("Ueber den Bernstein und die Bernstein-Fauna"). He distinguished eighteen genera in Amber, among which ten were new, and the rest were identified with recent forms. Unfortunately for Loew, the classification of the recent *Tipulidae* was, in 1850, in a chaotic condition. Before beginning his work, he should have at least prepared himself for it by a careful study of the contemporary literature on the *recent* forms of *Tipulidae*. His publication shows that, in this respect, he had not done his duty, otherwise he would not have committed the mistake of describing as new the Amber genus *Allarithmia*, but would have recognized in it the recent genus *Eriocera*, described by Macquart twelve years before. Loew had remained in ignorance of Macquart's genus still later, when, in Peters's "Reise in Mozambique," he described a species of the same genus as a *Limnobia*, although he had noticed its anomalous antennae, which should have reminded him of his own fossil *Allarithmia*.¹

¹ In extenuation of this mistake of Loew's it may be alleged that, although Dr. Peters's volume was dated 1862, it had been printed *ten* years before, in 1852 (comp. Hagen, "Bibl. Entom.," Vol. II, p. 386, Addenda, under *Loew*, No. 136). Loew's share in that volume may therefore have been written almost contemporaneously with his paper of 1850. Nevertheless, there is no excuse for his not having been aware of the existence of *Eriocera* Macq. (1838), and for his having said: "The remarkably anomalous structure of the antennae of this species does not permit its being placed in any of the subgenera that have been separated from *Limnobia*." This structure of the antennae is

My special study of *Tipulidae* brought with it the duty of criticising Loew's paper of 1850. In the "Monographs of North American Diptera," Vol. IV, p. 192 and 293 (1869), after having seen the original Amber specimens in Loew's collection, I pointed out that the four species of *Cylindrotoma*, which Loew believed to have recognized in Amber, were *Limnophilae*. This error of his is the more strange as, only a year before 1850 (in the *Stett. Ent. Zeit.*, 1849, p. 341), he had published a correct account of the genus *Cylindrotoma*. In describing *C. nigriventris*, from Siberia, he had stated that *C. distinctissima* should be considered as the type of the genus, and that the other two species introduced into it by Macquart (its founder, 1834) did not belong to it.

In Chapter II of this "Record" I have shown how, several years later, Loew had paid very little attention to my classification of *Tipulidae* (that of 1859), and had, with my work in his hand, mistaken a *Teucholabis* (O. S.) for a *Rhamphidia*. In my second monograph of *Tipulidae* (1869) I had therefore every reason to caution the public against the uncritical acceptance of the four species of *Rhamphidia* which Loew believed to have discovered in Amber, as well as against the other genera in his paper on Amber *Tipulidae* (O. S. in "Monographs," Vol. IV, p. 105, about *Rhamphidia*, and p. 192-193 about the other genera). No wonder that with such an insufficient knowledge of the *Tipulidae* Loew should have mixed up two altogether different genera: his fossil *Toxorrhina* (1850) and his recent *Toxorrhina* (1851). In this case, he had been led astray by the extraordinary long, filiform proboscis, common to both, without noticing that the recent species has a venation unique among *Tipulidae*, while his fossil *Toxorrhina* has a normal venation.

When I questioned Loew concerning the principles he had followed in establishing his genera of fossil *Tipulidae*, he gave me (in a letter of February 5, 1865) a rather long, but very unsatisfactory explanation, which he summed up thus: "I had hoped in this way

somewhat like that of *Anisomera*, but the discal cell is closed, while it is wanting in the latter genus. This species can undoubtedly be considered as the type of a new genus, which it will be time enough to describe when the male is discovered" (Peters's "Reise," etc., p. 2 of the separate on Diptera).

to obtain genera in which even a non-entomologist might easily locate his fossil species, and I also thought that, by this means, I should by and by obtain easily distinguishable genera for living species.”¹

The meaning of the first part of this sentence is plain enough; such genera were intended for *amateur-collectors* of fossil Amber species, and for the purpose of affording them the pleasure of affixing names to them. But the *second* part of the sentence, that Loew hoped in this way to get at the right definitions of genera for living species, is incomprehensible. He felt the weakness of his reasoning and concluded in a jocular tone: “I had no doubt that such genera would be only *provisional*, because, with such a small equipment of characters, one could not expect better genera, but I consoled myself with the reflection that *all* our systematic work is merely provisional.” It would be superfluous to say that this *cœur léger* manner of treating a question is not the way of solving it. But it is evident at the same time that if Loew, before beginning his work on fossil *Tipulidae*, had made a thorough preliminary study of the living forms, he would have discovered, among the mass of fossil specimens at his disposal, a sufficiency of characters for introducing more satisfactory genera. In the meantime Loew, as it seems, was preparing the publication of his detailed work on Amber Diptera, some plates of which he had shown me in 1865. He wrote me (July 24, 1867): “I believe that the first part will be ready for publication (‘druckfertig’) before Michaelmas (September 29, 1867).” This communication, I confess, astonished me, because Loew must have known when I saw him in 1865 that the manuscript he had prepared for the plates (and especially for the *Tipulidae*) was not in a condition fit for publication.

A long interval elapsed after this communication, and, Loew's expectations notwithstanding, the “Amber Diptera” had remained unpublished. Eleven years later Loew, already broken by age and sickness, in a letter from Guben (February, 1878) gave me the following explanation: “I should have been very glad at last to pub-

¹ “Ich hoffte so Gattungen zu erhalten, in welchen auch der Nicht-Entomolog die fossilen Arten leicht unterbringen könnte, und dachte von da aus später einmal zu Gattungen für lebende Arten zu kommen, deren Unterscheidung leicht wäre.”

lish the whole work on Amber Diptera, which cost me so much study and labor; but it seems that I must definitively give it up. Although the Physico-Economic Society of Königsberg had agreed ('hat sich verpflichtet') to publish this work, it has never made any step towards the fulfilment of this promise, so that a large portion of the letter-press and of the copper-plates which were submitted to the Society many years ago are still awaiting publication. I wish you would make up your mind sometime to undertake a revision of the family of Amber *Tipulidae* in its totality; the figures of all the species are drawn, and the necessary data for preparing descriptions are ready ('die nötigen Notizen zu den Beschreibungen gemacht'). To produce a useful and available publication would require the overhauling of all the species with regard to those characters which would qualify them for being located in your systematic arrangement, and to bring the generic names introduced by me into harmony with those that are used now for the recent ones. Such a working up would surely bring new light into the genetic connection of the genera and the distribution of their species." I answered this letter from Florence (April 7, 1878) and said: "I shall reflect upon your proposal to revise the Amber *Tipulidae*. The task would be most interesting for me, because hitherto I have had but a very superficial view of them." Loew's answer to this letter was dated April 21, 1878, a year, day for day, before his death. It gives a striking picture of his state of mind at that time, and an insight into the final collapse of the unfortunate Amber Diptera business. The letter was either dictated, or copied by another hand, because the rather tremulous signature alone was in Loew's handwriting. I translate a portion of it *verbatim*: —

"GUBEN, April 21, 1878.

"Your letter of April 7 reached me a few days ago. I followed your journey with interest, and envied you the enjoyment you were having. The Tertiary Diptera from Aix in Provence have interested me in former times [I had examined some of them in the Museum of Marseille. — O. S.], and have been the subject of many researches. I have had occasion to examine attentively, under the eye of Heer, the species described by him. The remarks which I published about them in the *Zeitschr. gesamt.*

Naturw., 1868, are true to this day.¹ The determinations of Heer reveal a rather clear systematic eye, but they are not based upon any trustworthy plastic characters, so that most of the new genera introduced by him are not tenable, for they coincide with other well-known genera. In general most of the species represented in Aix come very near the Amber Diptera. That, on account of my failing strength, I am compelled to give up entirely my laborious work of many years on the Amber Diptera is to me a subject of great grief. I had hoped that one or the other of my dipterological friends would feel inclined to bring the work to completion in making use of the drawings executed by me with endless care, and of the colossal material of Amber specimens at that time still piled up around these drawings. I can truly say that I have not spared the most urgent entreaties for that purpose, but a willing ear I have nowhere found. To see a painstaking work, the fruit of years of application, treated with utter disregard is most depressing. It drove me long ago to the settled resolve not to go a step farther, but to return to their owners all the specimens confided to me for the sake of description. And even in this undertaking I am sometimes having unpleasant experiences, as many people do not seem to understand the advantage they have in receiving (instead of unclassified, and often unpolished, rough fragments) polished specimens, for the most part accurately named. I shall feel happy when this painful business is finally and completely wound up. If you had consented last year, and at my urgent request, to undertake the working up of the Amber *Tipulidae*, the whole rich material would have been kept together, and I would have placed it all, including the drawings, in your hands. At present, it is unfortunately impossible, and I can dispose of the drawings only, not of the original specimens. Should you think, nevertheless, that you can make use of the drawings alone, we can yet discuss the matter."

In this case Loew's memory failed him completely, because it was not a year, but *only a month* before, that he had made me the offer to work upon the Amber *Tipulidae*; a task which, far from refusing, I had accepted in the very letter referred to above of April 7th, which he was answering on the 21st! Thus an unfortunate concatenation of circumstances has prevented me from making a more detailed study of Amber Diptera. I had visited

¹ This passage refers to the article: "Berichtigung der generischen Bestimmung einiger fossilen Dipteren" (*Zeitschr. gesamt. Naturw.*, 1868). Loew, prompted by the publication of Oswald Heer and L. v. Heyden on fossil *Bibionidae*, in which several useless new genera had been introduced, recommended some common-sense rules to be observed in describing fossil Diptera, rules which he himself had not observed in 1850, and which are still neglected in some recent publications on this subject.

Guben in 1871 (November 3-5) and in 1872 (August 23-25), but I was pressed for time, and two days would not have been sufficient for the work. In 1877, when I spent fifteen days there, I was entirely absorbed by the business of packing up the North American collection. So far as I remember, Loew at that time did not even mention the Amber business to me, and the state of his health made him incapable of unpacking the Amber collection. Of that visit I have given a detailed account to Mr. Alexander Agassiz in a letter which is reproduced below in my Chapter XII, in my account of the transfer of the collection. What became of all the plates and manuscripts prepared by Loew I do not know, and, for the reason stated in Chapter XIV, "I felt that I had neither the right nor the duty to inquire."

X NOTICES ON SOME FACTS CONNECTED WITH THE PUBLICATION OF THE SUCCESSIVE VOLUMES OF THE "MONOGRAPHS OF NORTH AMERICAN DIPTERA"

- 1 *On my share in translating from the German, and in editing, the three volumes of Loew's "Monographs of North American Diptera"*

In my "Introduction" and in Chapter I, "On the Beginnings," etc., I have given a general account of the *modus operandi* of my co-operation with Loew in the publication of his *three* volumes of the "Monographs." I deem it necessary now to give some further explanations, which principally concern my share *in translating* Loew's volumes.

In the Preface to the first volume (p. v) Loew says: "As to the English text, I use, it is true, the assistance and advice of a friend, who is well acquainted with the English language; should, nevertheless, some roughness occur, I beg that it may be attributed to some supplementary alterations of my own, which circumstances prevented me from submitting to him," etc. When the manuscript reached me, I found that the English of it was unfit for publication. I remember, for instance, that "Hinterleib" was translated "hindback" instead of "abdomen." By no means master of the English language myself, I had a great deal of trouble in

correcting it, and the result left much to be desired. In consequence, I wrote to Loew that I should prefer, in future, to have the manuscript in German, and that I would take care to have it translated in America. This was actually done with Vol. II. I entrusted the task to a translator. He did the routine work of translating the descriptions of the species, for which purpose I had drawn up for him a German-English glossary, and I did the rest, that is, translating the preface, the introduction, the descriptions of the genera, etc. But I soon felt that, even with this method, there was too much drudgery for me to undergo in collating and correcting the translation. For this reason, when the different portions of Vol. III were, one after the other, sent to me, I found it easier to translate them myself. The editing and proof-reading of the first two volumes were done by myself; the third volume was edited by my friend Dr. R. A. Witthaus of New York, during my absence in Europe. Before being struck off, the sheets passed, of course, through the hands of the authorities of the Smithsonian Institution. All these details, as well as the dates of publication, are mentioned in the "Advertisements" of Prof. Joseph Henry on the reverse of the titlepage of each volume.

2 *On the long delay attending the publication of the third volume of the "Monographs of North American Diptera"*

The "Advertisement" signed by Prof. Joseph Henry on the verso of the titlepage of Loew's Vol. III (December, 1873) of the "Monographs of North American Diptera" shows that this volume appeared nearly five years after Vol. IV (January, 1869), which contained my "Monograph on *Tipulidae brevipalpi*."

During my visit to Meseritz in 1865, Loew told me that the *Ortalidae* were ready for publication; he repeated this assurance in a letter dated January 31, 1866: "I told you, when I had the pleasure of seeing you, that for the third volume of the 'Monographs' the *Ortalidae* were ready. In the hope of obtaining more material I had delayed, for some time, to make a fair copy of them. This expectation not having been fulfilled, I have long ago begun to write out the manuscript in its final form, and to prepare

a fair copy of it, and at present I am very near the end of this task. The work will nearly fill a volume. The 'Additions' to the *Trypetae*, prepared long ago, might be added to it. This would furnish plenty of matter for Vol. III. But, how about its publication? Do you still feel interested enough in this work to undertake the great burden ('die grosse Last') of translating it? Still less am I sure how I am standing with the Smithsonian Institution." Loew complains about some remissness, on the part of the Institution, in the fulfilment of its engagements towards him, of an apparent want of appreciation of his work, etc., and ends by saying: —

"I want at least to know whether the Institution intends to continue the publication of the 'Monographs,' and whether I should consider myself authorized to order the necessary plates at its expense." Loew had no reason whatever to doubt of my good will, nor of the interest taken in his work by the Smithsonian Institution. I referred his complaints to Professor Baird, who (March 16, 1866) gave me the necessary explanations and assurances, and added: "As to the failure to send him a commendatory letter, remember the immense amount of the work we have to do, and the small force to do it with. We trusted to you as our confidential adviser to arrange all such matters with Loew, and trust you will do so, excusing our delay on account of the war, the fire, the flood, and anything else," etc.

Under the circumstances which I have just related it might have been expected that Vol. III would make its appearance soon. Nevertheless, more than *eight* years elapsed between the summer of 1865, when Loew told me that the manuscript of the *Ortalidae* was ready for publication, and December of 1873, when the third volume made its appearance before the public.

What was the cause of Loew's hesitation to publish a manuscript so far advanced in preparation as he announced it to be? Was he perhaps dissatisfied with its execution, but, at the same time, unwilling to acknowledge it? The fact is, that later he seems to have entirely rewritten it, and this was no doubt due to the mass of new material that I had sent him in the meantime (especially in *Trypetae*). It would be useless and tedious to

reproduce the long story of this delay, and of the excuses and apologies offered by Loew for it. Some of them were well founded: illness of himself and family (1866); the Austro-Prussian war, in which his three sons were engaged; his retirement from his position in Meseritz and his moving to Guben (1868). Nevertheless, the fact remains that Loew during these years did not relax his usual ardor in describing new species of Diptera. The "Centuries of North American Diptera" continued to appear: Century VI (1865), VII (1866), VIII, IX (1869), X (1870). The Prefaces of the new, large work, undertaken by Loew, "*Beschreibungen europäischer Dipteren*," are dated, that of Vol. I, October, 1868; that of Vol. II, December, 1870. Besides these works, the list of Loew's papers contains between 1865 and 1870 about *fifty* numbers, representing publications in the *Berl. Ent. Zeit.*, *Wien. Ent. Monats.*, *Zeitschr. gesamt. Naturw.*, *Zeitschr. Entom. Schles. Ver.*, *Jahrb. Gel. Ges. in Krakau*, *Bull. Soc. Ent. Ital.*, and in several other periodicals. The truth is, that Loew was much averse to monographic work on a larger scale, requiring long preliminary studies and a mental concentration possible only in a state of repose. He much preferred work of a kind that he could take up or put down at any time, I mean the work of producing a large number of short, disconnected articles, such as descriptions of new species, critical summaries of small groups of Diptera, local lists of species, etc. This manner of work may have been easier and more handy for him in his circumstances, and among the multitude of various occupations which his insatiable energy required. But, if such was the case, he should at least have been candid about it, and have openly avowed his preference; and it would have been an easy matter to make arrangements to suit him.

I had written down the above paragraph for some time, when, in looking over Loew's correspondence, I came across the following passage which confirms exactly what I have been saying. Loew's letter is dated February 5, 1865, that is, *before* my visit to him during the summer of the same year: "I believe that we shall, ultimately, agree on this point, that although this kind of publication [the Centuries] is inferior in quality to the publication in 'Monographs,' it offers the advantage of a rapid publication of many new species. And the reason is, that every attempt at determination which proves the species to be new becomes at

once a stimulus for immediately describing it, while the preparation of a monograph requires a more prolonged leisure ('eine zusammenhängendere Musse').” Unfortunately this frank statement came at a time when the third volume of the “Monographs” had been already announced, the plates, sent to Washington, had been printed, and remained for years waiting for the letter-press, delayed by the waywardness of Loew. At the end of that time, as will be seen below, it was too late to think of a new arrangement.

In August, 1871, I again sailed for Europe, and paid a visit to Loew in November of that year. I must have impressed upon him the necessity of doing something for Vol. III, because I find in my letters that I received a first instalment of the manuscript in Baden-Baden in January, 1872, just before returning to New York. Among the letters of Professor Baird to me, I find one complimenting me on my return. I reproduce its beginning, as it contains an allusion to my interview with Loew: “Washington, D.C., February 16, 1872. I was delighted to find by your letter that you had returned to the United States and hope we may keep you here as a permanent investment. We cannot spare men, like yourself, who, besides their own merits as naturalists, do so much to bring together persons of kindred tastes, who otherwise would never meet or come into correspondence. I am glad you interviewed Loew and got the promise of more manuscript. We have had, as you know, a number of plates printed off for years, which we shall be glad to utilize.”

In Baden, upon receiving the manuscript, I immediately began translating it, and continued my work during my journey, and even during my very rough winter passage across the ocean, in the French steamer “Péire,” which reached New York, February 4, 1872.¹ In July of the same year I again returned to Europe, and received from Loew the last part of the manuscript on the *Ortalidae*, the translation of which I at once made and sent to Washington. In the meantime I remained for more than a year in Europe. The translation of Vol. III went to press in February, 1873, and the editing, in my absence, was done by a friend of mine in New York (see above, p. 69). The manuscript of the *Trypetae*, which had to appear

¹ My eyes were so much affected by the air in the saloon of the steamer, closely shut up during the winter passage, that on my arrival in New York I had to consult an oculist.

in the same volume, was still wanting. Constant urging on my part made Loew finish it in great haste, and it reached me in London just before my embarking again for the United States. I translated it as fast as I could, and thus the long-expected volume was published in December, 1873. (Compare the "Advertisement" of Professor Henry on the reverse of the titlepage.) Although numerous corrections were introduced in translating and proof-reading, the haste in which the original manuscript was written may have left some traces in the text of Vol. III.

3 *The Smithsonian conflagration in 1865*

On January 25, 1865, at my breakfast table in my lodgings in New York, I received the first notice of the Smithsonian conflagration when the *New York Herald* was brought with a conspicuous heading: "Fire in the Smithsonian Institution! Baron Osten Sacken's manuscript burned." This was the manuscript of the fourth volume of the "Monographs of North American Diptera," which had cost me a great deal of labor. A few days later I received from Prof. Joseph Henry, who was at the head of the Institution at that time, a letter announcing the loss to me. I reproduce it in extenso as an interesting historical document:—

SMITHSONIAN INSTITUTION, Saturday, Feb. 4, 1865.

MY DEAR SIR, — It is with very great regret that I have to inform you that your Memoir on the *Limnobia* was entirely destroyed by fire, which took place in the Smithsonian building on the afternoon of January 24. It was burned in company with nearly all the official correspondence and records of the establishment. We trust, however, that this disaster will prove but a temporary inconvenience to you, and that you have retained such original drafts of the several parts of the Memoir as will enable you, with the help of a scribe, to reproduce it.

I am pleased to be able to inform you that the conflagration will not materially affect the essential operations of the Institution. The valuable library, the richest in this country in scientific books, the extensive collection of specimens intended for distribution, the Museum of the Exploring Expedition, the stereotype plates of the later publications, are all saved. The loss of the greatest importance is that of the contents of my office, including upwards of thirty thousand pages of copies of letters exclusively of my own composition.

The loss of the building can scarcely be considered a calamity, since it would probably have taken fire at some future time, when the contents of the rooms below would be much more valuable. The building has been from the first a source of much trouble and expense, and should not have been erected. The Smithsonian Institution is a cosmopolitan establishment, and should not be burthened with the support of a local Museum and a large building. Congress will, I think, in due time take the Museum and the building off our hands, and leave the income of the Smithsonian fund to be entirely devoted to the advancement of science. I remain, etc.,

JOSEPH HENRY.

About this loss of mine I published the following statement, which I find in a paper that appeared a few weeks after the conflagration, and contained some fragments of the manuscript that had escaped the fire¹: "Unfortunately, I had retained no copy. Anticipating a voyage to Europe early in the spring, I had hoped to see the manuscript go through the press before that time, and, hurrying to get it ready, did not stop to have it copied. . . . Except the fragments which I publish here, I have absolutely nothing in hand now, and the whole work will have to be done over again," etc.

The loss did not, in the end, turn out to be a loss to science. During a journey to Europe which I undertook soon after it had happened, I gained a great deal of information in the museums I visited, and that proved a benefit to the work when I reproduced it afterwards. The volume appeared in January, 1869, and in the Preface, signed by me in April, 1868, I do not even find any mention of the conflagration.

XI DR. HERMANN AUGUST HAGEN

Hermann August Hagen was born on May 30, 1817, in Königsberg, Eastern Prussia, and died in Cambridge, Mass., November 9, 1893.

I made Dr. Hagen's acquaintance on my way to North America, in the spring of 1856, an acquaintance which soon became a life-long friendship.² I encouraged Hagen to publish a work on the Neuroptera of North America, and, during my residence in the United

¹ Proc. Ent. Soc. Philadelphia, April, 1865, p. 224 (26, 1865).

² Compare my "Introduction," p. 2.

States, I sent him all the material I could get hold of. Besides the Neuroptera of my own collecting, an important contribution was the rich collection of the *Odonata* from Pecos River, Western Texas, made by the Exploring Expedition of Capt. J. Pope, U. S. A., which the Smithsonian Institution upon my suggestion sent to Dr. Hagen. The result of these contributions was his important publication, "Synopsis of the Neuroptera of North America," Washington, Smithsonian Institution, July, 1861. On the reverse of the titlepage the "Advertisement" of Prof. Joseph Henry, Secretary of the Institution, says: "The manuscript of this work was furnished by Dr. Hagen in Latin, and it has been translated into English by Mr. P. R. Uhler of Baltimore. To him and to Baron R. Osten Sacken the Institution is under obligations for the careful examination and correction of the proof sheets." In the Preface Dr. Hagen mentions, among the collections which he received, "the very numerous species collected by Baron Osten Sacken in different parts of the United States, particularly at Washington, at Trenton Falls, at Savannah and Dalton in Georgia, at Berkeley Springs in Virginia, in Florida, in Cuba, on the St. Lawrence River, and at Chicago."

In a letter from Königsberg, dated November 15, 1859, Dr. Hagen acknowledged the arrival of one of my consignments, and added: "It may interest you to have a survey of all your consignments, of which I have kept an accurate account, except the first, the number of specimens of which is given approximatively: —

"First consign. from you about	137 spec.,	from the Smiths. Inst. about	170;	total,	300
"Second, Nov., 1858, "	260 "	" " other persons	431;	"	691
"Third, April, 1859, "	96 "	" " " "	—	"	96
"Fourth, June, 1859, "	38 "	" " " "	—	"	38
"Fifth, Oct., 1859, "	275 "	" " " "	—	"	275
Total,					806
					1400

"The specimens preserved in spirits were not counted, so that the total reaches far beyond 1500 specimens."

Among these materials was the very rare *Panorpid*, *Merope tuber* Newm., which I had had the good fortune to obtain in *both sexes*, the very extraordinary male, with its immense forceps, having been unknown before. Hagen called it¹ "perhaps the most remarkable of all hitherto known Neuroptera."

¹ "Synopsis," p. 249.

I found the female *Merope tuber* in the summer of 1856, one evening, by lamp-light on the wall of the veranda of the hotel in Berkeley Springs, Va. A little later I obtained the *male* (unknown before) in the collection of the Museum of the Academy of Natural Sciences of Philadelphia. Professor Leidy was showing me a box of promiscuous unnamed insects, among which I noticed the specimen. Having called his attention to this remarkable form he authorized me to add it to the collection of Neuroptera which I was preparing to send to Dr. Hagen. (These facts are not mentioned in Hagen's "Synopsis.") *Merope tuber* seems to be a very rare insect. Upon my request, Mr. William Beutenmüller of New York sent me (April, 1901) the following information about its occurrence since my time: "In a paper by James S. Hine, entitled 'A Review of the *Panorpidæ* of America North of Mexico,'¹ *Merope tuber* is recorded from Maine, Pennsylvania, New York, and District of Columbia. He states that he has seen two males and three females."

Such was the result for my friend Hagen of the first three years of my residence in the United States.

In 1867 Prof. Louis Agassiz decided to appoint Dr. Hagen Custodian of the entomological collection which he was forming in the Museum at Cambridge, Mass., a post which Hagen occupied until his death in 1893. As I had had some influence upon Professor Agassiz's choice, he wrote me, while I was in Europe, in May, 1873: "Plus nous avançons et plus je m'estime heureux d'avoir découvert le Dr. Hagen, et d'avoir réussi à l'attacher au Musée. Par ses soins, notre collection entomologique prend tous les jours plus de consistance, à mesure qu'elle s'arrange et se développe," etc.

Hagen, like Loew, was a Hercules of working power, but a Hercules whose only ambition was to be useful, and to fulfil conscientiously what he considered as his duty. I often thought that, in some cases, he carried this sense of self-sacrifice too far, and that he sometimes, like the mythological Hercules, good-naturedly undertook to sweep some entomological Augean stable, which he might have left to be cleansed by some other person whose time was less valuable than his. A monument of his learning, conscientiousness, and marvellous endurance is his "Bibliotheca Entomologica," in two volumes (1862-1863). In a letter which Loew wrote me at that time (June, 1861) I find this characteristic passage: "When I see Hagen trudging along with his fifty pounds of manuscript, from town to town, and from library to library, I think to myself, *this is not the kind of work I would have undertaken.*"

¹ "Univ. Bull. Ohio State University," Series V, No. 7.

A biographical notice of Hagen by Mr. Samuel Henshaw appeared in the *Proceedings of the American Academy of Arts and Sciences*, Vol. XXIX. His domestic life is described in this short sentence: "Of Dr. Hagen's domestic life it is sufficient to record here that in 1851 he married Johanna Maria Elise Gerhards, who survives him." As a friend and sincere admirer of Mrs. Hagen I feel bound to complete this insufficient notice of her. About Dr. Hagen's death Mr. Henshaw's biographical notice says: "Stricken with paralysis in September, 1890, Dr. Hagen lingered for more than three years; his painful sufferings being lightened by all that affectionate and devoted care could do." A letter from a friend of mine, who lives in Cambridge, and who announced to me, at that time, the death of Hagen, contains the following passage: "His wife's devotion, strength, and cheerfulness have been simply magnificent. For more than three years she never once spent six hours, winter or summer, away from the house. She had to lift him, move him in his bed, dress him, and never once flagged or faltered. She seems surprisingly fresh and healthy-minded after all she has gone through, and is certainly an admirable woman."

A short obituary notice was also published in *Psyche*, February, 1894.

After Hagen's death, Mrs. Hagen returned to Königsberg, in Prussia, her husband's residence before his removal to the United States.

XII AN ACCOUNT OF THE CIRCUMSTANCES ATTENDING THE TRANSFER OF THE COLLECTION OF NORTH AMERICAN DIPTERA ACCUMULATED IN THE HANDS OF LOEW FROM GUBEN, HIS RESIDENCE, TO THE MUSEUM IN CAMBRIDGE, MASS.

In the "Introduction" (p. 8) I said that "one of the first duties I had to fulfil, after my return to Europe (in 1877), was to go to Guben, the residence of Loew, and to secure the shipment to the United States of the large collection of North American Diptera which, for the last twenty years, had been accumulating in his hands," and contained the types of his descriptions. I shall now

give a detailed account of the whole transaction. For the facts I rely upon the collection of the letters of Loew to me between 1871 and 1879, and upon my own letters to him during that period, of all of which I possess the originals,¹ and on my letters to the two Agassiz, of which I have retained rough drafts.

These drafts may not be verbatim reproductions of my original letters, which perhaps still exist in the archives of the Museum of Comparative Zoölogy in Cambridge, Mass., for the reason that in making a fair copy of a letter, I often introduce changes which are not always found in the original draft; but, as to facts, the drafts are of course trustworthy.

As I said in the "Introduction" (p. 7), after resigning in 1871 my post of Consul General of Russia in New York, I spent a part of the two following years in Europe. Before leaving the United States, I had, with the permission of Prof. Louis Agassiz, deposited my own collection of North American Diptera in the Museum in Cambridge, Mass. I also had had a conversation with him and his son, Mr. Alexander Agassiz, about my plans concerning the large collection of North American Diptera in the hands of Loew. I reproduce the draft of a letter of mine to Prof. Louis Agassiz, dated from Riva, Lago di Garda, October, 1872:—

Before leaving the United States I took occasion to mention to your son how desirable it would be to secure for the Museum of Comparative Zoölogy the collection of North American Diptera now in Dr. Loew's possession, which contains all the types of his descriptions. Allow me to explain to you the origin and the present condition of this collection. The majority of the specimens and species which it contains were collected by me, and sent to Loew for the purpose of being described. I distinctly reserved my right of ownership in these specimens, as can be proved by the correspondence exchanged between Loew and myself on this subject. My sole intention, however, in thus insisting upon my right of ownership was to secure the eventual return to the United States of these specimens, which in the meantime would have become valuable types of descriptions. And all I am expecting now, in consideration of the cession of my rights, is, that Dr. Loew, my friend, and for many years my collaborator in American dipterology, and to whose writings the collection owes its principal value,

¹ Loew's widow had returned to me all the letters I had written to her husband between 1871 and 1879. Of my letters to Loew between 1856 and 1871, with rare exceptions, I have neither drafts nor originals.

shall be remunerated for the labor he has bestowed upon it.¹ It must be borne in mind at the same time that, besides what he received from me, Loew has considerably added to this collection from other sources (for instance, by purchases from Mr. Belfrage), so that a portion of it is his property. Thus the sum paid to him will also cover the purchase of the specimens of the latter description. The collection contains:—

1. The original types of all the North American species described by Loew (the total number being 1350–1360), say	1300 species.
2. Species described by former authors and satisfactorily identified by Loew, at least	330 species.
3. Undescribed and unidentified species (according to Loew's estimation)	1200–1300 species.
Total	2800–2900 species.

You are aware that the portion of my own dipterological collection which remained in my hands in the United States is deposited now by me (with your leave) in the Cambridge Museum. It is of course smaller than the other as to the number of species. It contains:—

1. Numbered duplicates of a considerable number of species represented in most cases by well-preserved specimens, in addition to the typical ones of the other collection, that may in some cases have become defective in consequence of travel and handling.

2. The types of my descriptions, especially in the family *Tipulidae*.

3. The entire collection of the family *Tabanidae*, which I intend to work up at some future time.

N. B. This was written in 1872. Since then, besides the *Tabanidae*, many other typical specimens of my descriptions have been added to the collection: those of my "Western Diptera," of my paper on *Syrphus* s. str., etc.

4. A large number of undetermined species, not contained in the other collection, especially in the families which have not been worked up (*Muscidae*, *Culicidae*, *Chironomidae*, etc.).

Both collections united must contain about 1800 determined species (about 1450 of which are original types) and certainly beyond 1600 undetermined species; and thus they represent very nearly the actual condition of our knowledge of North American Diptera.

Now, as soon as the former collection becomes the property of the Museum, I will formally renounce in favor of the same Museum my right

¹ Loew had received from the Smithsonian Institution a pecuniary compensation for the manuscripts of the three volumes of the "Monographs." But, of course, this compensation covered *that* part of his work only, and not his other publications ("Centuries," etc.), nor the additions made by himself to the collection.

of property in the latter collection, and thus the Museum, in acquiring one collection, will have secured both.

Before concluding, I will add an observation. Loew continues to work upon the materials in the collection No. 1. He contemplates especially to prepare a critical catalogue of the described Diptera of North America (to supersede my Catalogue of 1858). It would be desirable, therefore, to leave the collection in his hands for a few years longer. Nevertheless, some arrangement should be entered into at once to remove all uncertainty as to the future of the collection, and to secure the ultimate possession of it to the Cambridge Museum. It will remain for you and Dr. Hagen to decide as to the mode of proceeding for attaining this object.

On the same day, in October, 1872, I wrote to Dr. Hagen (at that time Custodian of the Entomological Department of the Museum in Cambridge, Mass.), informing him of some particulars concerning the proposed transaction which had been agreed upon between Loew and myself during my visit to Loew about five weeks before (August 23-25, 1872), but which I had not mentioned in my letter to Professor Agassiz. Loew thought that the sum of from fifteen hundred to sixteen hundred dollars would be a reasonable compensation to him, and that an agreement could be entered into in virtue of which he (Loew) would be bound to deliver the collection within a certain number of years (say ten years).

Professor Agassiz answered my letter rather late, in May 31, 1873, having been prevented by circumstances from doing it earlier, and thus it happened that his letter, directed originally to my bankers in Florence, followed me about Europe from place to place during my travels, and finally came back to Cambridge, Mass., where I found it upon my return to the United States in the autumn of 1873. As the matter was not an urgent one, there was no harm in the delay. I quote the principal passage of Prof. L. Agassiz's letter (translated from the original French) "I am writing to ask you to come to a final agreement for the acquisition of Loew's collection, about which we have already had a conversation, and for which you have been kind enough to offer your good offices and assistance. I shall accept with gratitude whatever you may do in this business, and I write you to-day for the particular purpose of informing you that I hold at your, or Mr. Loew's, disposal the sum agreed upon by you as the price of the collection, which

I shall forward to you by return of mail. I am most desirous of acquiring the collection for us," etc.

In a letter from Cambridge, Mass., November 21, 1873, I informed Loew that Professor Agassiz had agreed to his conditions, and that, upon his (Agassiz's) expressing some anxiety about the identity of the object of the purchase, I had given him the assurance that the collection was kept in a separate case or cupboard ("Schrank"), so that no mistake was possible. I added that upon receiving the price of it, he (Loew) should immediately insure it for the corresponding amount; that, in return for the long delay accorded, he should continue his work on the collection, and also on the promised Catalogue. To this Loew replied (Guben, December 16, 1873): "In the matter of the purchase of the collection, I accept Professor Agassiz's conditions. I shall compensate for the immediate payment of the fifteen hundred dollars by continuing to complete it so far as possible ('nach Möglichkeit zu vervollständigen'), and to use all diligence in the publication of the undescribed species which it contains. You were perfectly right in reassuring Professor Agassiz about the state of the collection, which is kept separate from the others in a case containing fifty glass-covered drawers, easily distinguishable from my other collections. I agree to the assignment of ten years for the final delivery of the collection, although I believe that the growing infirmities of my age will make the interval shorter. All my collections are insured against fire; but I shall make a separate arrangement for the insurance of the North American collection." At the end of the letter Loew thanked me for the sacrifice I had made of my own collection: "The generous way in which you intend to give up your own collection I appreciate with sincere gratitude ('Die generöse Art in welcher Sie sich Ihrer Dipteren entäussern zu wollen erklärt haben, erkenne ich mit aufrichtigem Danke an')." This letter of Loew was dated December 16, 1873; in the meantime (December 14) had occurred the untimely death of Prof. Louis Agassiz. Nevertheless the promised sum for the collection was paid to Loew during the next year, 1874.

Besides my Diptera, I had presented to the Museum of Comparative Zoölogy in Cambridge, Mass., my collection of deforma-

tions of plants, consisting of galls produced by *Cynipidae*, *Cecidomyiæ*, and other insects, and also by *Acaridae*; and of deformations produced by microscopic fungi, and by hypertrophy. I suppose that it is in reference to these collections that Prof. Louis Agassiz wrote me the following letter which I find among my papers; it was written under dictation, during illness, and was dated Cambridge, April 19, 1872, just before my departure for Europe: "Dr. Hagen has just told me what a magnificent present you have made to our Museum. I lament that I am too sick to return my warmest thanks with my own hand, but be assured that I shall ever cherish the remembrance of your liberality towards our Institution. It will always be gratefully recorded with us that the first part of our entomological collection, which has reached a high scientific standard, was that which we owe to you. I had hoped that my convalescence would be short, to enable me to see you before your departure. I can only wish you a pleasant journey, coupled with the request that you may hereafter remember me and continue to me your friendship."

In my simplicity, and in presence of Loew's warm assurances of the interest he took in the collection, I did not realize the gravity of the change which had, in the mean time, taken place in regard to our joint labors. I did not know that at the time when Loew signed the letter accepting the conditions of Agassiz, he had his mandate as a member of the Chamber of Deputies in Berlin in his pocket. In the spring of 1874 I received a letter from him, dated Berlin, May 15, which began in this fashion: "I have just received your letter of April 29, which found me in Berlin, where I am detained as a member of the Chamber of Deputies, and am overwhelmed with work towards the end of the spring session. You may perhaps blame me for having, by the acceptance of this mandate, put an obstacle in the way of my entomological activity. As I had, for this very reason, declined a mandate to the Reichstag that had been offered to me, it was impossible for me to refuse the seat in the Chamber of Deputies without breaking with my political friends and the members of my party. My refusal would have given the seat of the District to the opposite party, and thus I was compelled to accept the burden with resignation. But enough of

this ('Doch genug davon').” And then, for the evident purpose of mitigating the effect of such a beginning, Loew passed without any transition to a rambling discourse, on four closely written pages, about his interest in the collection, his plans for the future new Catalogue, etc. The letter ended thus: “Once more my heartfelt thanks for the great proofs of friendly kindness and faithful friendship by which, I cannot deny it, you have relieved me of a heavy burden of material anxiety in which I have been involved by the circumstances of the times ('durch welche Sie mir in Folge der Zeitverhältnisse, wie ich nicht leugnen kann, eine schwer lastende materielle Sorge abgenommen haben'). Thus I am enabled now to devote more time and strength to the study of the American Diptera, and I promise you to do this as far as the strength of my old age ('alten Kräfte') permits. With the assurances,” etc., etc.

The true meaning of this strange proceeding of Loew's was, that the various sums of money which he had received through my mediation had enabled him to realize his long-cherished plan of re-entering political life. Henceforth his constant pre-occupation consisted in lulling me with his assurances about his interest in our common work, and his preparations for the new Catalogue, while his inner self was occupied in an entirely different sphere.

It was not only for the transaction about the sale of the collection that Loew felt grateful, but also for a payment he had recently received from the Smithsonian Institution for the *third* volume of the “Monographs.” About this remuneration, I had made an arrangement rather advantageous for Loew's finances. I translate a passage of my letter to him, dated May 2, 1872: “I made with Professor Baird the following arrangement, and I hope you will not take it in bad part. As I did not care to make the translation for nothing, I agreed with the Smithsonian Institution that they should consider the *English manuscript* as *your* work, and that they should add the payment for my translation to the remuneration due to you. This will make them understand that I consider the remuneration which you receive as too small, and will add a zest to my labor in translating.”

The consequences of this change in Loew's situation soon became manifest. My claims upon him had much increased since I had settled in Cambridge, Mass. While I lived in Washington and in New York, I had my specialties to occupy me, and my correspondence with Loew had principally our common work for its object. In Cambridge, besides putting in order the Museum's and my own

collections, I had undertaken, independently of Loew, to prepare materials for the new edition of the Catalogue of North American Diptera, and besides this, I often received from various quarters, and more frequently than before, collections of Diptera for determination, which were sometimes of considerable size. Prof. J. H. Comstock, for instance, sent me a very valuable collection for that purpose. I find that between May, 1874, and May, 1875, I wrote fifteen letters to Loew; and I must render him the justice to say that he was very conscientious in answering my numerous questions about details of nomenclature. When he was in Berlin, he had of course to postpone his answers until his return to Guben. During the same interval I received ten letters from him, most of them very long and rambling, and many of them written in the Chamber of Deputies in Berlin.

In a letter of June 19, 1874, wishing to accelerate the working up of some families of Diptera, I suggested to Loew the idea that the North American *Muscidae Calyptratae*, of which there was plenty of material, but which had not been worked up at all, should be entrusted to some German entomologist of the younger generation, for the purpose of being described under the supervision of Loew. I was willing to remunerate that entomologist out of my own pocket, and to undertake the translation of the manuscript into English. I was thinking of *Kowarz*, who would have been perfectly willing to undertake a work which, besides its scientific interest, would have afforded him a welcome addition to his rather meagre income. My proposal was so serious that on the very next day (June 20, 1874) I wrote a second letter to Loew, to explain and develop my proposition. My good will came to nothing, in consequence of Loew's indifference. He received the proposal rather coolly, and said (letter of July 4, 1874) that Schiner alone would have been able to undertake the work. Unfortunately Schiner had died the year before, and the idea of Schiner working under the guidance of his enemy, Loew, was preposterous. A counter-proposition which Loew offered me was just like him. He said that if I was willing to spend money for the description of North American Diptera, it would perhaps be expedient to publish plates, with figures representing generic types, with appended de-

scriptions, *à la Curtis*. This *édition de luxe*, of course, would have been published in Germany, under the superintendence of Dr. Loew, and would have contributed, without much trouble on his part, to his own glory. I paid no attention to this selfish proposition. In a letter (from Halle, October 27, 1874), Loew returned to this subject, and mentioned Kowarz as a fitting person for the work I had proposed; but it was evident that Loew was not serious, and I abandoned the matter. The earlier letter (July 4, 1874) was typical of the rest; six closely written pages, the last of which contained not less than fifty-three lines, a letter written at intervals, in a rambling way, passing from one subject to the other, without treating anything seriously. It was evident that Loew was struggling painfully between two conflicting duties. He now began to think of the possible expediency of sending to America at least portions of the collection (Loew's letters of October 27 and December, 1874). In answer to these proposals, I advised him (January 22, 1875) to write to Mr. Alexander Agassiz (successor of Prof. Louis Agassiz in the management of the Museum), and to inform him that his (Loew's) health prevented him from working any longer upon the collection, and that he would prefer to send it over, either in portions, or the whole at once. I added, for Loew's guidance, that the collection would be kept intact, and not incorporated with the other collection. Whereupon Loew answered (February 15, 1875): "As you inform me that the collection will be kept separately, I have no doubts now how to arrange a gradual transfer of it. I shall begin with those portions which contain most of the types described by me. Whatever I have in hand of unpublished species of these same families, I shall, at the same time, publish." This letter was written from Berlin, and Loew complained that, on account of the length of the session, he would have to remain there till June. The next letter, likewise from Berlin, was written leisurely at the Chamber of Deputies, on March 20, 21, 22, 1875. It was again a long, rambling, nevertheless most friendly letter, containing an account of the state of entomology in Europe in general, reflections on the system of classification to adopt in the future Catalogue, etc. The conclusion was: "I hope to write again soon and more at leisure. *Bis dahin herzlichen Gruss, allezeit aber in hochachtungsvoller*

Freundschaft Ihr herzlich ergebener, H. L." This was the last letter I received from Loew at that time. Several letters of mine remained without answer, and before starting for California I wrote, plaintively, but in vain (November 30, 1875): "Please remember that since March last I have not received a sign of life from you," etc. With this our correspondence ended, and was only renewed when I landed in Europe two years later.

It was evident that Loew had become sick of the insincere game he was playing with me, and had suddenly thrown off the mask, and put an abrupt end to this useless correspondence. In the mean time, I had accomplished my western journey (December, 1875–September, 1876), had written and published my "Western Diptera," and was preparing to leave for Europe. A correspondence took place between Mr. Alexander Agassiz and me about the North American collection in the hands of Loew. I gave Mr. Agassiz (February 25, 1877) a detailed account of the strange position in which Loew had placed me, and of my anxiety about the collection. I asked him to write to Loew and to request him to prepare the collection for shipment next autumn. I remember Hagen chaffing me about it, and warning me that the collection would never reach Cambridge, Mass. Mr. A. Agassiz followed my advice, wrote to Loew, and received an answer which he sent me at Newport, Rhode Island, my residence at that time.

Of this answer of Loew to Mr. Agassiz (dated Guben, April 6, 1877), I shall give here the beginning in abstract, and the rest verbatim. It begins with apologies for dictating the letter, as he had had a stroke of paralysis during the summer of 1876, and had not yet quite recovered from it. The shipment of the collection, for this reason, would not be possible before the autumn of 1877. But for accomplishing this purpose within the specified time, it was absolutely necessary for Osten Sacken to come to Guben, and to discuss different matters on which would depend the usefulness of the collection when it should reach the Museum in Cambridge, Mass. The work on the Catalogue being a part of the contract concluded with Prof. Louis Agassiz in 1873, Loew felt the necessity of explaining his tardiness. For three years he had been occupied in the Chamber of Deputies in Berlin, in the

“Kreistag,” and in the “Kirchenrath,” and these occupations ended with his above-mentioned illness. Under such circumstances, the work prepared for the Catalogue had not yet reached the amplitude that Loew had hoped to give it (“Leider sind die von mir gefertigten Arbeiten nicht ganz zu dem Umfange gelangt, den ich zu erreichen hoffte”).

The letter goes on as follows:—

“This burden of occupation on my side, and the frequent changes of residence of Baron Osten Sacken, were the cause that a great part of the work on North American Diptera prepared by me has not yet come into the hands of Osten Sacken, and has not yet been published. It would be very regrettable if this should not be retrieved. As soon as I am in condition to go to work again, I shall resume my regular correspondence with Osten Sacken, and shall endeavor to continue it steadily, because such is the only way to convey the collection into your hands in the most expedient and satisfactory manner.

“Furthermore, it is only through a conference with Osten Sacken that it will be possible for me to decide which portions of the collection should be sent to Cambridge first, in order to be serviceable to him for the preparation of his intended new edition of the Catalogue of Diptera. In case you should prefer to receive the whole collection sooner, without making use of the preliminary work prepared by me for the compilation of the new Catalogue, it will perhaps be possible for me to accomplish this, within a reasonable time, with the help of an entomological friend. However, the aim in view would be attained in a better and more satisfactory manner with the personal aid of Osten Sacken.”

Whatever the work “prepared for the Catalogue” may have been, I have never seen a particle of it, except a few scraps which will be noticed below, in my “Report” to Mr. Alexander Agassiz. In the mean time, the Catalogue I had prepared independently of Loew was finished in manuscript, and only required collating with the collection in Guben.

The original German of the last paragraph of Loew's letter runs as follows:—

“Durch diese Arbeitslast auf meiner Seite, und durch die häufigen Ortsveränderungen des H. Baron Osten Sacken andrerseits, ist der grösste Theil der von mir in dieser Zeit gefertigten Arbeiten über die N. A. Dipterenfauna noch gar nicht in die Hände von Baron Osten Sacken, und noch viel weniger zur Publication gelangt. Es würde sehr bedauerlich sein, wenn dies nicht noch nachgeholt werden sollte. Sobald ich wieder etwas arbeitsfähiger sein werde, will ich mich von Neuem mit Osten Sacken in feste Verbindung setzen, und dauernd darin zu erhalten suchen, da nur mit Hülfe

einer solchen die Sammlung in erspriesslichster und zweckmässigster Weise in Ihre Hände geleitet werden kann.

“Auch vermag ich nur durch Besprechung mit ihm festzustellen, welche Theile der Sammlung zunächst nach Cambridge gesendet werden müssen, ums einer neuen werthvollen Bearbeitung des Dipteren-Catalogs zu Statten zu kommen. Wenn Sie die ganze Sammlung baldigst haben wollen, ohne dass die von mir gemachten Vorarbeiten für die Zusammenstellung des neuen Catalogs ausgenutzt werden sollen, so werde ich das vielleicht mit Hülfe eines befreundeten Dipterologen in leidlicher Frist zu Stände bringen vermögen. Besser und gründlicher wird allerdings der Zweck durch Osten Sacken's persönliche Beihülfe gefördert werden.”

About these proposals of Loew I reproduce my answer to Mr. Agassiz (Newport, R. I., April 24, 1877), as I find it in the draft which I possess. It gives a curious insight into my state of mind at that time.

“DEAR MR. AGASSIZ: — I return herewith Loew's letter. In my opinion, the safest course to pursue, would be to take advantage of the passage on the third page ('In case you should prefer to receive the whole collection,' etc.) and to answer that the Museum wishes to come into possession of the collection as soon as possible, and without depending on Osten Sacken's arrival, or co-operation. It would be useful to remind Loew to return at the same time those specimens which I foolishly sent him, while at work in the Museum. I doubt very much of the utility of further publications on his part in the present state of things (assuming that such publications were possible and seriously contemplated). They would necessarily appear in Germany, and in the German language, and thus throw an obstacle in the way of the American student instead of helping him. Loew cannot count upon me for translating and editing them, as I have done for the three volumes of the 'Monographs of North American Diptera'; I have had enough of that kind of labor. Loew's letter does not afford any clue towards explaining his behavior towards me. He seems to forget that his silence began more than a year before his illness. And if he thinks that, after such a behavior, I will continue to play second fiddle to him, he takes altogether too favorable a view of my good-nature. His want of candor appears especially in the passage, 'the frequent changes of residence of Osten Sacken,' etc., as if my movements had anything to do with his writings, and as if he had not written his last letter to me more than *eight months* before I left for California!

“I have done with my winter's work (Western Diptera) and intend to take a short holiday in New York. I do not think I shall sail for Europe before June.”

My decision to have the collection shipped without any further delay proved most fortunate, because the decay of Loew's faculties

progressed very rapidly during the following summer (1877), and he was hardly able to transact any serious matter with me when I visited him in autumn.

I sailed for Europe in June, 1877, and spent a fortnight in Guben in September. And now comes the strangest incident of this whole transaction. Among Loew's letters which I have before me there is one, upon which an annotation in my own handwriting explains that this letter had been written by Loew in Berlin in March and April, 1875, but, instead of being sent to me at that time, was kept in Guben, *until it was handed to me by Loew himself two years and a half later*, in September, 1877, when I paid him my last visit!

This letter had been begun a few days after Loew's last letter to me, of March 20-22, 1875, which I have characterized above as *long and rambling*. A fortnight elapsed before Loew added a paragraph to this new letter (on April 14), and then it remained in his hands, unfinished. What explanation Loew gave me on that occasion, I do not remember; at any rate an explanation for me was unnecessary, because, as I said above (p. 86), "it was evident that Loew had become sick of the insincere game he was playing with me."

The old letter, handed to me in such a strange manner, begins thus (*translation*):—

"MY DEAR SIR AND FRIEND:—Your letter of March 5 [1875] arrived two days after the posting of my last altogether meaningless letter. I admire your vigorous entomological activity, and envy you the leisure you are enjoying, and of which I am entirely deprived; you may imagine how anxious I am to be at home. That you have succeeded in unravelling, I may say *off-hand* ('so beian'), the genus *Syrphus* s. str., I admire!¹ I stuck in a jungle of uncertainty in attempting to determine the species of that group. It would be splendid, if you succeeded likewise with *Sphaerophoria*, the most difficult among the genera of *Syrphidae*," etc.

I reproduce the German text of this part of the letter: "Mein hoch geehrtester Herr und Freund, Ihr Brief vom 5. März [1875] traf zwei Tage nach Abgang meines vorigen, *recht inhaltslosen* Briefes hier an. Ich bewundere Ihr rüstiges entomologisches Schaffen und beneide Sie darum, da ich hier gar keine Musse zu gewinnen

¹ This is an allusion to my paper: "On the North American species of the genus *Syrphus* (in the narrowest sense)"; *Proc. Boston Society Natural History* 1875 my No. 47, 1875).

vermag ; Sie können denken, dass ich mich herzlich nach Hause sehne. Dass Sie die Gattung *Syrphus* s. str. so beian haben abmachen können, bewundere ich ; ich bin bei dem Versuch die Arten derselben zu bestimmen gar sehr im Gestrüpp der Zweifel stecken geblieben. Prächtig wäre es, wenn es Ihnen noch gelänge in der schwierigsten aller Syrphidengattungen, nämlich in der Gatt. *Sphaerophoria*, einiges Licht zu verschaffen ; ich vermag bis jetzt die festen Grenzen zwischen den americanischen Arten ebenso wenig zu finden, wie zwischen unseren europäischen, und jene von diesen zum guten Theil nicht zu unterscheiden," etc.

The rest of the letter treats of entomological matters. The collector Jacob Boll had been in Berlin recently with a splendid collection of Diptera from Texas. Loew complained of Dr. Gerstaecker, the Custodian of the entomological collection of the Berlin Museum, who had played false with him in purchasing the best part of Boll's specimens at a very cheap rate, and let him depart without letting Loew know anything about it. At this point the letter comes to an abrupt termination.

Jacob Boll, born in Switzerland in 1828, died in Texas in September, 1880. The lepidopterist H. Frey of Zürich said of him that "he was born a collector, with a wonderfully quick eye." H. K. Morrison, in the United States, was another instance of a marvellous talent in the same profession.

About the result of my visit at Guben, and the receiving and packing up of the collection of North American Diptera, I drew up a report to Mr. Alexander Agassiz, dated from Berlin, September, 1877. I reproduce the rough draft of this report, as I find it among my papers : —

"I have just returned from Guben, where I have received and packed the collection of North American Diptera. According to your instructions, the three large cases containing it have been sent to the office of Richard and Boas in Hamburg. . . . I fondly hope that the collection may arrive safely, although I am not without anxiety about it. The drawers which contain it are bad for transportation, heavy, and with glass covers. Strips of paper have been pasted, crosswise, on these covers, so as to support the glass in case of breakage. This was done by order of Loew before I came. Some textile fabric would have done better I think. The drawers (42 in all) are packed in very strong wooden cases with mattresses inside. The packers, who seem to be experts, assure me that all is safe. Still I shall feel very anxious until I hear of the safe arrival of this precious collection. If I had other drawers at my disposal (or Deyrolle's 'cartons' as the best) I would not have grudged the trouble of transferring the whole collection.

In the meantime I have done what I could to secure its safety. Before packing, I collated the whole collection with my manuscript Catalogue, and put in order those portions which were in confusion. Furthermore I tested the strength of the insertion of nearly every pin, and secured those which seemed loose. I write out separately a detailed technical report of what I have done, and of the condition in which I found the collection." [This Report is contained in my Chapter XIII.]

"Loew seems to be well enough, he can walk and talk and give useful information on whatever he is asked. At the same time, *c'est un homme fini*. He cannot stand the slightest intellectual effort, any concentration of mind, or of memory. Writing the simplest letter is a great effort for him. His physician told me that his case was an ossification of the blood-vessels of the brain, which was incurable. But in ordinary intercourse one could stay some time with him without noticing anything. — I spent not less than *fifteen*, not very pleasant days of hard work in Guben, which of course will also benefit my Catalogue of Diptera. I obtained many data for it from the collection, and from some manuscript notes of Loew, but I had to spare Loew himself as much as possible. Next winter I intend to spend in Heidelberg."

"P. S. The height of the drawers will allow them to be placed inside of the Museum-drawers; so that Hagen will have no trouble about it."

This last detail is important, because after the safe arrival of the collection in Cambridge, Mass., the drawers containing it were preserved in the Museum in the same state in which they had left Loew's house. The expenses incurred by me for the packing of the collection, the freights, etc., were immediately refunded to me by Mr. A. Agassiz.

In presence of the tragedy of Loew's decline and death (an account of which will be found in Chapter XV, "Obituary Notice"), we must not consider the detail, but the total result of his contribution to American dipterology, which consists of a very large amount of descriptive work, always thoroughly conscientious, and of the realization of the long-cherished ideal of a typical collection, which was delivered by Loew in a perfect condition. This was the same Loew who, according to his biographer, had made in his youth (and kept) the vow not to partake of any warm food, until he had paid off the debts contracted during the penurious years of his University studies! At the same time I must confess that if Loew had met my good will with more straightforwardness and

candor, if he had been, in this respect, more like Dr. Hagen, we should have reached the same result, or perhaps more, but, at least for me, with less friction.

My principal motive for drawing up this long account of my relations with Loew, and of my scheme to form, for future use, a collection of the original types of North American Diptera, was the intrinsic interest which attaches to the matter. I confidently hope that, besides the biographical and psychological features connected with my story, the details it contains will be of some use to those who may have occasion to consult the collection in future. The desire to do justice to myself, legitimate as it is, would not have alone prompted me to undertake this somewhat tedious task.

XIII SPECIAL REPORT ON THE COLLECTION OF NORTH AMERICAN DIPTERA, ACCUMULATED IN GUBEN, AND TRANSFERRED IN 1877 TO THE MUSEUM IN CAMBRIDGE, MASS.

1 *How I received the Collection*¹

Before having the collection removed from Loew's dwelling and boxed up, I collated all species contained in it with my manuscript Catalogue, which I had brought with me. The specimens which had been temporarily removed to other boxes and drawers were put back where they belonged. I found that, on the whole, the collection was remarkably complete and in good order. Some few of Loew's descriptions had been made from specimens borrowed from other collections, and those, of course, were not present. Very few types were wanting for other reasons; for instance, from the accidental loss of a label, or on account of the destruction of the original specimen. The *type-specimens* were provided with labels in Loew's own handwriting fastened on the pin; the others were placed alongside of them, or behind them, in a second row. Specific names alone are marked on the labels; the generic names are

¹ This is the separate technical Report about the collection mentioned in my letter to Mr. Alexander Agassiz in Chapter XII. I have modified it in some parts, especially at the end, to suit present circumstance and to make it more intelligible. If I publish all the *minutiae* about labels, localities, original discoverers of the species, etc., it is because I am convinced that they are not useless.

almost entirely omitted in the collection. This will offer no inconvenience whatever, considering that only advanced students, and not beginners, will be admitted to the use of the collection. In some few cases the types bore on the label a name different from that which had been used in the description; the real name is, however, always easy to trace. In some instances of this kind, but not in all, I added a label with the true name, without removing the label in the handwriting of Loew. (All labels of my own which I introduced in the collection are signed with my name.) In cases when the locality named in the description did not appear among the specimens on hand, I had reason to suppose that the description had been drawn from specimens sent in my earlier consignments, and that they had been replaced later by better preserved specimens from other localities.

With very few exceptions, everything was left as it was found. At the request of Loew, I put in better order the *Dasygogonina* and the genus *Tipula*, which, in consequence of the numerous additions made, were somewhat in confusion and too crowded. I put some order in the genus *Syrphus*, and also transferred the contents of one of the drawers, which I did not think safe, into another, taking care to preserve as much as possible the arrangement which I found. Loew told me that the *Empidæ* had been put in order, at his request, by Mr. Kowarz. Wishing to reduce as much as possible the number of drawers to be boxed up, I left behind the *Tabanidæ*, as the collection of them contained no types and was insignificant in comparison with my typical collection, presented by me to the Museum in Cambridge, Mass. For the same reason, I did not take the *Limnobia*, as all the types will be found in my collection in the Museum. But in both cases I singled out a few species which I thought worth preserving. Besides the portion of the collection which had been worked up by Loew, there are whole families which are still in a crude condition. Nevertheless they show a beginning of an arrangement by Loew, and contain many labels and notices in his handwriting which may be useful. I should recommend to leave such portions of the collection untouched, as, in that state, they may be serviceable to those who will in future work up any of such families.

2 *Miscellaneous Notes on the Localities of the Diptera in Loew's and my Collections (now both in the Museums in Cambridge, Mass.) and on the Collectors who, besides myself, have contributed to them*

Many specimens, in both collections, will be found without any indication of locality. Most of them are specimens of my own early collecting, principally about Washington, D. C. In sending them to Loew I always informed him of the localities, which for this reason ought to be found recorded in his descriptions.

Diptera coming from various sources bear different little labels, some of which I shall attempt to explain.

1. Labels with numbers, corresponding to the same numbers in the Osten Sacken Collection (in the same Museum), represent specimens which I sent to Loew, while keeping duplicate specimens in my own collection. (Compare my *Reports to the Trustees of the Museum* 44, 1875, p. 15.)

2. Round, blue labels mean specimens from Mr. Calverly (at that time in Brooklyn, N. Y.), collected in *New York State*.

3. Silver squares mean *Cuba* (Loew's Coll.).

4. Ochre — yellow squares — *Texas* (id.).

5. Green squares with *R* upon them mean, "*Red River of the North*" (collected by Mr. Robert Kennicott); similar green squares with *Fa* upon them, indicate my collecting in *Florida*; and those with *Cu*, my collecting in *Cuba*.

6. Small pink squares mean *Sitka* (Loew's Coll.); somewhat similar, but larger squares of a reddish color I have for some time used for *District of Columbia*, but they do not occur often.

7. Some paper squares with dates will be easily recognized as indicating flies from *Pecos River*, collected by United States officers under the command of Captain (later General) Pope. In Loew's "*Centuries*" such specimens are marked: *New Mexico* (Captain Pope).

8. White labels, with numbers, mean: collected by Mr. S. H. Scudder in *Massachusetts*. Nearly similar labels indicate insects received from *Connecticut* (E. Norton).

9. Small yellow round labels mean *Florida*, collected by Mr. E. Norton.

10. Small triangular labels, yellow and green, mark a considerable number of specimens from *Western New York*, which I received for determination from Prof. J. H. Comstock with the permission to retain duplicates.

Some further notices may be useful:—

Diptera from *Nebraska*, some of which are described in the "Centuries" (for instance, I, 37, 57, 79, 80; II, 9, 15; IV, 52, 73, 91; V, 42; VI, 15, 18; VII, 27, 47, 48; VIII, 43, 94; IX, 21), were collected by Dr. F. V. Hayden, during the earlier period of his career. I mention this, because the name of the collector has in most cases been omitted by Loew.

Diptera marked N. II. (New Hampshire) on white, square, printed labels, were all collected by me in the White Mountains, New Hampshire, whether my name is mentioned or not (this concerns especially Century IX).

Between the years of 1858 and 1861, I received several lots of Diptera, collected by Mr. W. H. Edwards (of Coalburgh, Virginia) principally in the State of New York; but I do not remember whether they were separately labeled or not. Edwards's name often occurs in the "Centuries," especially in the first.

Several specimens from *California* I received from Mr. Alexander Agassiz. They are described in the "Centuries," as received from him, but whether they bear particular marks in the Collection, I do not remember.

Many new species of Diptera contributed by Dr. William Le Baron, in Geneva, Ill., will be found described, principally in Loew's "Centuries" (*c. g.* Century II, VI, etc.).

Diptera from *Oregon* (Centuries VI, 69) and *Matamoras* (Centuries VI, 65) are wrongly attributed to me in the "Centuries." I have never visited these regions.

Before the arrival, in 1877, of the collection from Guben, I had made a present of my own collection of North American Diptera to the Museum of Comparative Zoölogy in Cambridge, Mass. (compare Chapter XII). With this collection I amalgamated the materials in Diptera which had been accumulated in the Museum before my arrival, principally by its Custodian, Mr. Philip R. Uhler, predecessor of Dr. Hagen. Of the result of this operation

I presented a detailed Report to the Trustees of the Museum, which is incorporated (p. 14-17) in their Annual Report of 1874 to the State Legislature. This Report of mine (44, 1875) contains abundant notices on the localities, as well as on the captors and contributors of Diptera to the Museum before 1873-1875, and is a useful contribution to the history of this collection before its amalgamation with the Loew and Osten Sacken collections.

XIV MY RELATIONS WITH LOEW SINCE MY RETURN TO EUROPE IN 1877, UNTIL HIS DEATH IN 1879

My Chapter XII contains a detailed account of the transfer of the collection of North American Diptera from Guben to Cambridge, Mass., and it remains for me now to conclude the story of my relations with Loew up to his death.

Loew's very singular conduct towards me between 1875 and 1877, and the total cessation of our correspondence, did not prevent him from hailing my return to Europe with delight. The first letter I received from him in Europe (dated August 10-14, 1877) began with the words: "My most heart-felt *Salve* upon your arrival on German soil, which I connect with the hope of having a long interview with you, and of the revival of our relations." The tone of his letters had now a touching ring of sincerity, quite different from what I had been accustomed to before. But they were painful to read on account of the evident decline of his faculties. They were written with visible effort, and with frequent interruptions. In the above-quoted letter of August, 1877, he showed a great anxiety about the fate of his own European collection and of his numerous manuscripts, and asked me to help him in the matter. To purchase the collection myself was out of the question; I could not assume the responsibilities which the possession of such a collection would have involved. Fortunately, the question received the best possible solution in the purchase of it by the Prussian Government for the Berlin Museum.¹ About his manuscripts

¹ Compare Chapter XVI, at the end, where details about the fate of Loew's European collection will be found.

Loew wrote: "I have *Collectanea* and several unfinished works, a part of them nearly ready, but which my present condition prevents me from finishing. For instance, my work on the Amber Diptera" (about this compare my Chapter IX); "a work on *Insects noxious to Agriculture*, prepared (as a prize-essay) in collaboration with Schaum; the beginning of a work on Fedtchenko's collection of Diptera from Turkestan; preparations for a reform ('Neubearbeitung') of the System of Diptera for the intended new edition of your Catalogue of North American Diptera" (about this, compare my Chapter XII); "a fourth volume of the *Supplements* to Meigen, which lies before me nearly completed ('welcher fast vollendet daliegt'), and many other papers ('und vieles Aehnliches')." ¹

Alexis Fedtchenko (1844-1873). I cannot let pass the name of this admirable man and naturalist, who honored me with his friendship, without paying him a tribute of respect and affection. His biography, with a portrait, will be found in the Collection of Biographies of Russian Naturalists, published (in Russian) by the *Imperial Moscow Society of Natural Sciences*, Vol. I (1888). A sympathetic notice on his untimely and lamentable death on the "Col du Géant" (near Mont Blanc) on September 15, 1873, was published by G. Lohde, of Leipzig, in the *Berl. Ent. Zeit.*, 1873, p. 236. An important addition to it will be found in the same periodical, 1874, p. 150. Besides Fedtchenko's many-sided work in all branches of natural science during his explorations of Central Asia, he had a decided taste for dipterology. His "Catalogue of the Diptera of the Environs of Moscow" (in 4to, 1868) is a model of a work of that kind. The Diptera collected by him in Central Asia were published by Loew principally in his volumes of "Beschreibungen europäischer Dipteren" (1871-1873). Loew expresses his thanks to Fedtchenko in the Preface (p. vi) of the *third* volume.

During my visit to Guben in September, 1877 (of which I have given an account in my Chapter XII), I saw very little of Loew. His nervous irritation was such that soon after my arrival his physician ordered him to be completely isolated, and I accomplished my object of packing up and sending off the collection without being allowed to see him again. On November 17 I was able to inform Loew, by letter, of the safe arrival of the collection in the United States. He expressed his great satisfaction in a letter begun in December, 1877, continued on January 5, 1878, and

¹ That this fourth volume remained unpublished, was due to the fact that no bookseller could be found to undertake such an unremunerative publication! (Compare the statement of Dr. J. Schabl (of Warsaw) in the *Wiener Ent. Zeit.*, 1902, p. 71.)

finished on January 11. Since September, he had spent some time in the country, and felt strengthened. I reproduce (in translation) the end of the letter:—

“To what I have said, I shall add nothing, for fear of making it still more confused. That I am going down-hill rapidly, you will easily perceive. The sad consequence of it is, that all my scientific correspondence begins to lag, and that all communion with friends becomes more rare; in a word, a solitude, intellectual and emotional, is forming around me; I experience a painful longing for a change, and, if possible, for an improvement. It is in the midst of such a state of depression that I beg you soon to cheer me with an answer, and with friendly advice about the disposal of my collection. In old and sincere devotion, ever yours,
H. LOEW.”

Two or three more letters were exchanged between me (from Heidelberg) and Loew (from Guben). He discussed some entomological matters, especially in a letter of his, dated February 12, 1878, which is rather long and nevertheless quite coherent. Loew complained of the chaotic state of contemporaneous dipterology, and attributed it to two causes: (1) The confusion of the System, and (2) the isolation of dipterologists, who have gradually formed “abgeschlossene Landmannschaften” (isolated corporations), each one following a separate nomenclature, etc.; a “Babel-like confusion of languages is the rule, and the collapse of the great tower of Babel seems to be impending.” Loew throws out a hint I should undertake the part of a mediator. The next and last letter shows a lamentable deterioration. It is dated April 21, 1878, a year, day for day, before Loew’s end. It is written under dictation, and signed by the tremulous hand of Loew. The portion of it which concerns the *Amber Diptera* work, I have reproduced in my Chapter IX; the rest of the letter treats principally of the chances of selling his European collection. Of his literary remains of which he had made mention in his letter of August 10–14, 1877 (compare above, p. 97), he never spoke to me again, either during our last interview in Guben (September, 1877), or in any of his subsequent letters. If he had entertained for a moment the idea of instituting me his literary executor, he must have abandoned it afterwards. For this reason, after his demise, I felt that I had neither the right nor the duty to inquire about his manuscripts.

XV PROF. FR. HERMANN LOEW

AN OBITUARY NOTICE BY C. ST. [CARUS STERNE]¹

A brief notice among the parliamentary news of the "Vossische Zeitung" announces the placid death, after a long illness, of the distinguished pedagogue, naturalist, and pioneer of German unity, *Hermann Loew*, in Halle on the Saale, on April 21 [1879]. A short notice about the life and life-work of the deceased will not be unwelcome to his numerous pupils, political partisans, and friends. He belonged to a family which, for centuries past, had followed the career of civil functionaries or of divines. He was born on the 19th of July, 1807, in Weissenfels [a short distance south of Halle. OSTEN SACKEN], as eldest son of a functionary in the Department of Justice in the Duchy of Saxony, who afterwards became "Geheimer Regierungsrath" in Prussia. From the conventual school of Rossleben he passed to the University of Halle, where he devoted himself assiduously to various studies in mathematics, philology, and natural history, and thus laid the foundation for his many-sided capacity for the profession of teaching. In the beginning of the third decade of this century, he went to Berlin, and gave lessons in different schools of higher grade, for some time also in the military school (Kadetten-Schule). Occasionally, he held the position of a private tutor of young men, for instance, of Prince Biron, of the celebrated oculist Graefe, etc. It characterizes his strength of will that, at the beginning of his career, he vowed never to touch a warm morsel until he had paid off the debts contracted during the penurious years of his studies, and that he actually carried out this vow.

He was soon afterwards appointed superior teacher of the Frederick William Gymnasium in Posen, and married there (1834) the daughter of "Ober-Prediger" Ehrlich, a favorite preacher at that time. He had made her acquaintance as a student, and for forty-five years she was for him a faithful wife and nurse. He had in

¹ Translated from the *Deutsche Entomologische Zeitschrift*, 1879, p. 419-421. *Carus Sterne* is the pseudonym and anagram of the well-known German author, Dr. *Ernst Krause*.

Posen a large field of activity as a teacher of mathematics and natural history¹; at the same time he directed his attention to the study of entomology, and especially to the, at that time, much neglected order of two-winged insects. To this specialty he remained true during his life-time, and became later on one of the first authorities on the subject. He first attracted attention by his "*Horae anatonicae*," a work of the most minute research in the anatomy of insects, and especially of their genital organs. His next publications appeared in the so-called "Programmes" of the Friedrich Wilhelm Gymnasium.

His long-cherished wish to make a journey of exploration to the East was finally fulfilled. He undertook it (1841-1842) in company with the celebrated geographer, Kiepert, and the philologist and geographer, August Schoenborn, who afterwards became his brother-in-law, but at that time was Professor in the same Institution, and known as author of several Latin school-books. Loew was unfortunately not in a position to publish the results of his journey independently. He communicated a portion of them to the entomologist Burmeister, and also to Alex. v. Humboldt. The greatest part he made use of in his later publications. All his life he longed for travel in distant countries, but in this respect circumstances did not favor him.

In the midst of his labors the closing years of the first half of the century brought on a period of political troubles, and found him, in the advanced post which he occupied,² an energetic champion of German nationality. German to the core, he boldly withstood the separatist longings of the Poles. The confidence of his fellow-citizens in him was shown by his election to the first German Parliament in Frankfort a. M. in 1848, when he was only forty years old. Always abstaining, according to his habit, from noisy manifestations, but all the more zealously using a latent influence, he devoted himself here untiringly to the support of Gager's Imperial Party. The appalling news of a case of cholera

¹ Several of his scholars became in due time scientific celebrities; for instance: Prof. *Kuno Fischer*, and the mathematician Professor *Konigsberger*, both of whom I have known in Heidelberg. — OSTEN SACKEN.

² Posen, near the frontier of Poland.

in his family called him home (in the summer of 1849) from the midst of his political activity. On the very day of his forty-third anniversary, this plague carried off his eldest daughter, who had just come of age, — a grief which he never entirely got over. He soon became aware that the dream of his youth, the unity of Germany, was not to be realized at that time, and returned in resignation to his pedagogic career, and to the study of natural history.

In the meantime his extraordinary abilities had been appreciated, and in October, 1850, he was appointed Director of the Royal "Realschule" in Meseritz, which afterwards became a Gymnasium. He found this Institution in an inchoate state of development, and soon raised it, in spite of the small means at his disposal, to the rank of one of the best schools of that time. The teaching of his favorite subjects, especially mathematics and natural history, was raised to a high degree of perfection. In his method of teaching he showed real genius; far from following the usual routine, he was always suggestive and inspiring, instilling into the dullest heads a glimpse of the superiority of their master. By such means he acquired in a rare degree the affection of his scholars.

Unfortunately, the reactionary tendencies which prevailed at that time prevented his merit from being appreciated as much as it deserved. And yet, absorbed by the duties of his official position and by his scientific studies, he carefully abstained from politics. In order to avoid all occasions of conflicts with his superiors in the Educational Department, he resisted repeatedly offers of a seat in the Prussian Landtag for the district of Meseritz-Bomst. In addition to the difficulties of his political position, Loew had for several years to endure severe bodily sufferings. When, after one of the most painful periods of his life, — I mean in the years 1851–1854,¹ in which he suffered the torments of stone in the bladder, — he returned to his duties with an expression of deep suffering on his face, but at the same time with the old energy and tenacity, hardly one of us dared to hope that he would still be able to devote twenty-five years of his life to the service of science and of his country. Although he continued to follow his calling with all the fibres of

¹ It will be noticed, in Hagen's "Bibliotheca," Vol. I, p. 491, that, during the years, 1852–1854 (Nos. 68, 69) Loew published hardly anything. — OSTEN SACKEN.

his heart, he finally felt that he could no longer endure endless annoyances, and he sent in his resignation in 1868.

In the meantime his scientific work was going on incessantly, and had earned him a universal reputation. His "Dipterologische Beiträge," that appeared as a separate publication, and numerous papers scattered over different periodicals, are considered as fundamental. He published separate works on North American Diptera, and their classification and description is, for the most part, his work. By the introduction of the venation¹ as a systematic character, very little valued before him, he founded a new branch of this study. Many new species were discovered or described by him for the first time. A still greater popularity was obtained by his works on noxious insects, especially *Cecidomyiæ*. He also paid attention to fossil insects, and particularly to those enclosed in Amber and preserved in wonderful perfection. Through his intercourse with dealers in Amber he had formed a large collection of such Amber mummies; and I can remember him now, sitting at his working-table, which was laden like a jeweller's counter with these brilliant objects, and examining them with his magnifying-glass. These works on the Diptera of the Tertiary period, and especially on the Amber Diptera, still await publication.²

In 1868, free from all official cares, he moved from Meseritz to Guben. Here he had plenty of time to give himself up to his favorite studies, working incessantly, and only now and then interrupting his work by scientific excursions in Germany and abroad. His appearances at the yearly meeting of German naturalists are remembered by his friends and colleagues. Of friends he had a great many in all parts of the world, and his correspondents could be counted by the hundred.

The claims of public life that could not spare his many-sided knowledge and experience gradually again took possession of him. Solicited by different associations, elected a City-Councillor and Vice-President of the Council, he could not escape other engagements, and, after a long interval, he accepted a seat in the Legis-

¹ About Loew's treatment of the *venation*, I shall have something to say in my "Characterization" of him as a dipterologist. — OSTEN SACKEN.

² Compare my Chapter IX on Loew's work on Amber Diptera, etc. — OSTEN SACKEN.

lature (in Berlin) for the district Sorau-Guben, and for the term of 1873-1876. The dream of his youth was now fulfilled, and he could, with a lighter heart than before, devote his labors to the development of liberal institutions. He joined the National-Liberal party.

During the summer of 1876, while he was staying at Blankenburg (Thüringen) on a holiday trip, he had a first paralytic stroke. He recovered from this, but after that time these attacks on his iron constitution recurred several times, until at last his once so bright spirit yielded to the inexorable decrees of Fate. For this gifted man, after years of toil, a brighter life's evening was not to be. His grave disorder overpowered him, and he sought succor and rest in the "Diaconissinnen-Haus" in Halle on the Saale, hoping thus to escape the mental derangement which he felt like a dark shadow over him. From a fresh paralytic stroke he was not to recover. Since the beginning of March, 1879, he remained confined to his bed; an exceedingly painful nephritic ailment aggravated his condition, and, in spite of the tenderest care, he expired on the 21st of April, after several days of unconsciousness. He was buried on the 24th in Halle, the place where, half a century before, in the fulness of youthful strength and spirits, he had laid the foundation of his vast scientific knowledge. Bowed down by grief, his widow and the three sons, survivors of seven children, stood around his coffin; they must have found consolation in the thought that thousands of friends, of political partisans, of naturalists, and of pupils of all classes shared their sorrow and honored his memory.

XVI CHARACTERIZATION OF LOEW AS A DIPTEROLOGIST¹

In view of my purpose of giving an account of Loew's life and work, I was fortunate in finding his biography already written by Dr. E. Krause, who knew him well and could appreciate him as a man and as a citizen. During the *twenty-nine* years (1850-1879)

¹ The numbers appended to Loew's publications in this "Characterization" are those of Hagen's "Bibliotheca," as far as No. 124; beyond that, the Nos. 125-222 are those of my "Verzeichniss der entomologischen Schriften von H. Loew," etc., in the *Verh. zool.-bot. Gesellsch.*, Wien, 1884, p. 455-464 (104, 1884).

that I had relations with Loew, I met him only five times (1859, 1865, 1871, 1872, and 1877), always at his place of residence (at Meseritz, and afterwards at Guben), and each time only for a few days. The impression he made upon me during these short interviews is in perfect accordance with the pleasant portrait of him drawn by Dr. Krause. It is to me a source of great regret that the time of Loew's last illness and death almost coincided with my permanent establishment in Europe. A few years of close communion with him would have brought us nearer to each other in mutual appreciation, perhaps for the benefit of science, but most surely it would have been for my own benefit to have held intimate intercourse with a very superior man. My high esteem of Loew I have shown in devoting *twenty-one* of the best years of my life (1856-1877) to his service.

Before attempting to render an account of Loew's career as a dipterologist, I feel bound to define my qualification, as well as my limitation, for such a task. My qualification consists in the twenty-one years of collaboration with him in the work on North American Diptera; my limitation, in not having made a particular study of European Diptera, so that I am unable to form, as a specialist, an adequate opinion of the principal portion of his life's work. This limitation does not extend, however, to my criticisms of his views on *classification*, as these are applicable to his work upon the faunae of both continents.

What I intend to do is to show in what kind of work Loew's great merit consists, and to point out, at the same time, in what qualities he was deficient. I will show that his failings are principally perceptible at the beginning of his career, while he was feeling his way, and, in his thirst for work, trying for it in different directions, until he recognized his true vocation, *descriptive dipterology*, and concentrated upon it his whole power, with marvellous ardor and endurance. And I shall sum up by proving that his colossal descriptive work will be the permanent foundation of his fame.

At a time when, after Meigen's retirement, European dipterology threatened to remain at a standstill, and exotic Diptera were left to the tender mercies of the respectable but very insufficient Macquart, and to the mischievous activity of Walker, Loew ap-

peared and, in the interval between 1840 and 1878, produced nearly 6000 octavo and 1200 quarto pages of principally descriptive work, comprising more than *four thousand* new European and exotic species (fossil Diptera not included). Considering that this work, with, I may say, one single exception (his publications on fossil Diptera), was thoroughly conscientious, and showed throughout a remarkable homogeneity in the finish of its execution, we cannot refrain from acknowledging the extent of the progress achieved, within the interval of less than forty years, by the Herculean effort of one single man. What would have been the state of dipterology now, if Loew had not appeared, and progress had depended on the efforts of nationally isolated entomologists, men sometimes of undoubted talent, like *Robineau-Desvoidy*, *Say*, *Curtis*, *Haliday*, and *Rondani*, but each one working in his own way, having his own system, and his own particular terminology? I have quoted in my Chapter XIV a passage from one of Loew's letters to me, in which, at the close of his own career (in 1878), he complained of the isolation of dipterologists, who had formed, as he said, national *corporations* ("abgeschlossene Landmannschaften"), each one following a separate system and nomenclature, "a Babel-like confusion of languages is the rule, and the collapse of the great tower of Babel seems to be impending!" There is no doubt that, without the intervention of Loew after the retreat of Meigen, this state of things would have been much worse.

After this general statement, I shall proceed with a survey of Loew's entomological publications.

As a naturalist, Loew had received an excellent training in the University of Halle, where he had studied mathematics and natural history. He could dissect, and was a skilful draughtsman. In looking over the list of his earlier publications in Hagen's "*Bibliotheca*," we find a number of purely anatomical papers among them (I shall give their titles in English): No. 5. On the genital organs of female Diptera (1841); No. 7. Anatomy of the coleopteron *Buprestis mariana* (1841); No. 8. On peculiar nerve-structures in the genital organs of the females of some insects (1841); No. 10. *Horae anatomicae*, a separate work of 126 pages with plates, on the anatomy of insects (1841); No. 15. On the so-called "*Saug-*

magen" (jabot, crop) of Diptera (1843); No. 16. Some anatomical remarks about Neuroptera (1843); No. 54. Same subject (1848); No. 107. On the halteres of Diptera (1858), a paper with the mere negative result of refuting the view of Braxton Hicks, that they were organs of smell.

These publications prove that, between 1841 and 1843, Loew took a very lively interest in insect anatomy. The more strange it is that this interest seems to have disappeared later, and that Loew's anatomical knowledge did not leave any apparent trace in his later descriptive work, or in his views on classification. In Loew's Preface to the second edition of his "Posener Dipteren" (1840), he intimates that, "as a preliminary result of a considerable number of anatomical researches," an arrangement in many respects more natural than Meigen's might be brought about (compare the whole passage, reproduced by me in Chapter VII). Loew never kept this promise.

Loew did not publish much on the *biology of insects*. Of this class of papers we find, in Hagen's "Bibliotheca":—

No. 12. On the habit of *Gelechia lappella* Linn. (Microlepidoptera). (*Stett. Ent. Zeit.*, 1842.)

No. 13. Remarks on some larvae of *Ceratopogon* and *Sciara* (*ibid.*, 1843), a paper about which I shall have something to say below.

No. 14. On the caprification of figs. (*Ibid.*, 1843.)

Finally, under No. 48 (1847) we find a paper which, as we shall presently see, was in its kind *unique* among Loew's publications. As it has been published in a rather inaccessible periodical, and has for this reason remained almost unknown, I shall add a detailed abstract of it here.

Mittheilungen über die Verwandlungsgeschichte einiger Insecten und über ihren Haushalt auf Pflanzen

(*Allgem. Deutsche Naturhist. Zeitung*; Dresden u. Leipzig, 2. Jahrg., 1847, p. 287-301, with a plate.)

Loew begins with praising the growing taste for entomology in Germany, which he attributes to the recent foundation of the Entomological Society in Stettin, and the facilities it offers to students in promoting correspondence and exchange, procuring books, etc. He continues: "The starting of two periodicals, the *Stettiner*

Zeitung and the *Linnaea Entomologica*, contributed to the increase of entomological literature.¹ The descriptive and systematic departments of entomology have thus gained much, and it is to be regretted that the anatomical and biological departments have not been equally favored. Little has been done in these departments since the wonderful works of Réaumur and De Geer were published. Unfortunately, these works were written before the development of the entomological system, and for this reason the species described in them are not named. Recently, naming has become easy, and biological observations should be encouraged. One may be a good observer without possessing a vast entomological knowledge. A good eye and an attentive watching of life in nature are sufficient. Look sharply, describe accurately and immediately, draw what can be drawn, and inquire from others without stint, whenever inquiry is necessary. . . . Things to observe are so many, observers so scarce, that there is no need to fear repeating old observations." Loew recommends especially the study of the agency of insects upon plants in the shape of deformations, galls, etc., and mildly reproaches botanists with their preference for dried specimens. He offers several examples of observations made by him during his rambles in the country, and adds that, if they are well received, it would afford him pleasure to continue such publications. This promise, as many other good intentions of Loew, was not fulfilled. The wise advice he gives would be in keeping with the ways of Zeller, but not with those of Loew, who, as we shall see in the sequel, cared very little for living Diptera, and wanted them pinned as soon as possible. Observations of life require a repose not consistent with Loew's ever-restless disposition. And it is for this very reason that I have called Loew's No. 48 *unique* among his publications. It had no successor.

Three paragraphs follow this Introduction of Loew's No. 48 (1847) :—

1. Observations on *Veronica anagallis*. Description of two insects bred from this plant: 1. *Gymnetron villosulus* (Curculionidae), the larva and pupa of which are figured; 2. A larva of *Cecidomyia*, which has not been bred.

2. Observations on *Sonchus arvensis*. Two *Trypetæ* (*dilacerata*, sp. n., and *sonchi*) and the fly *Gitona distigma* Meig. were obtained from swollen flower-heads; details are figured.

3. Observations on *Inula britannica*. From galls upon this plant Loew had bred *Cecidomyia inulae*, sp. n., and a minute hymenopterous parasite. Details are figured.

The paper already quoted above: "Remarks on some larvae of *Ceratopogon* and *Sciara*" (*Stett. Ent. Zeit.*, 1843, p. 27–31), deserves especial notice, because it is the first in which Loew expresses himself on some knotty points of classification. About *Ceratopogon* this paper brings nothing new. It quotes Guérin, as describer of the early stages of the genus, and omits to mention the earlier

¹ This was written in 1847, that is, three years before the rupture of all relations between Loew and C. A. Dohrn, the President of the Entomological Society in Stettin. — OSTEN SACKEN.

observations of Bouché (1837). In regard to the systematic position of *Sciara*, Loew, during his whole life-time, remained in an incomprehensible perplexity, and made, as I shall show further on, repeated and undeserved reproaches on this subject to Meigen and Macquart. It is worthy of notice that in the same volume of the *Stettiner Zeitung* (1843) in which the very unsatisfactory paper on *Ceratopogon* and *Sciara* appeared, there are two other papers by Loew which foreshadow his best manner, descriptive and critical essays on *Chrysogaster* and *Milichia*.

To Loew's work on the biology of Diptera must be referred the following papers on the paedogenesis of *Cecidomyiidae*:—

No. 151 bis.¹ Bericht über die lebendig gebährenden Dipteren-Larven, etc., *Berl. Ent. Zeit.*, 1864, under the heading Neuere Litteratur, p. v-x.

No. 166. Notiz über eine neuere, die lebendiggebärenden Dipteren-Larven betreffende Publication (*Berl. Ent. Zeit.*, 1865, p. 270).

Loew's versatility appeared to advantage in several publications on economic entomology:—

No. 97. Ueber die den Roggen, etc., verwüstende Fliegenmade. *Schles. Zeitschr. Ent.*, 1858. *Cecidomyia secalina*, sp. n.

No. 111. Die neue Kornmade, etc., 1859 (*Cecidomyia secalina* Loew). This paper was published in pamphlet form and, as Loew informed me (March 18, 1859), proved a success; 800 copies were sold in less than four weeks.

No. 196. Thierische Parasiten (Coleoptera, Lepidoptera, Diptera, etc.) — *Jahrb. d. Landwirthschaft*, Vol. III, 1870, p. 522-539. The title is misleading, as the contents refer almost exclusively to insects injurious to agriculture, and are principally extracts from other publications. The Diptera are four *Cecidomyiæ*, several *Chlorops*, and *Oscinis frit*.

A work on "Insects noxious to Agriculture," prepared as a prize-essay in collaboration with Schaum (already mentioned by me in Chapter XIV) remained unpublished.

I shall endeavor now to give a survey of Loew's work upon the

¹ This paper was inadvertently omitted in my "Verzeichniss," etc., in the *Verh. zool.-bot. Gesellsch.*, Wien, 1884. This omission will be noticed under 104, 1884, of my "List."

classification of Diptera, which, I regret to state in advance, was not the department in which he was particularly successful.

In judging of Loew's attempts to classify the *Nemocera*, we must distinguish the years before the beginning of his correspondence with Haliday (in 1847) from those after this date. Before it, Loew was under the influence of Westwood's "Introduction," etc. (1840), and especially of the concluding part of it, the "Synopsis" (at the end of the second volume), which he erroneously attributed to Haliday (comp. "Dipterologische Beiträge," Vol. I, p. 10 at top, 1845). In the very first letter of Haliday to Loew (September, 1847), there is a passage which proves that Haliday did not hold himself responsible, for instance, for the connection of the *Psychodidae* with the *Cecidomyiidae* adopted by Westwood. *Cecidomyia*, *Psychoda*, *Sciara* (*Molobrus* Latr.), and *Anarete* Halid. had been united by Westwood in his subfamily *Cecidomyiinae* ("Synopsis," p. 126, in Westwood's Vol. II).

In his early papers, Loew betrays a remarkable want of method in assuming relationships upon very vague indications, and attributing too great an importance to superficial resemblances in the venation. In his "Beschreibung einiger neuen Gattungen der europäischen Dipterenfauna" (*Stett. Ent. Zeit.*, 1844, p. 117), he says: "I believe that this group, *Psychodidae*, can very well be united with the *Cecidomyiidae*, as Westwood has done it; the resemblance of the early stages may even justify the connection of *Sciara* with them." On the next page (p. 118) he produces a tabular arrangement, very like that which appeared next year in his "Dipterologische Beiträge," Vol. I, p. 10 (1845).

In the same article of 1844, Loew introduces the new genus *Liponeura* (Blepharoceridae) and brings it (p. 121-122) in connection with *Macropoeza* (a relative of *Ceratopogon*) and with *Diamesa* (a relative of *Chironomus*), a juxtaposition based merely upon the venation. Loew says: "These three genera form a peculiar little group, and the question is, whether still other genera do not belong to it." *Chenesia* Macq. (which afterwards proved to be the same as *Orphnephila* Hal.) is introduced into this same pretended group, with the remark: "I have no doubt that *Simulium* has a certain relationship with the same genus"; upon which follows a discus-

sion about *Simulium*, containing a series of untenable propositions, and among them that, on account of their three Afterklauen, they should be placed among the *Tip. florales* (*Bibionidae*, etc.). The strange expression "Afterklauen," in this case, of course, must mean pulvilli, and yet *Simulium* has no pulvilli, and only a rudimentary empodium. It is singular that Loew, as late as 1858, continued to entertain the same erroneous notion about the pulvilli of *Simulium*! (See *Berl. Ent. Zeit.*, 1858, p. 102, line 13.)

As I have said above, Loew all his life remained uncertain about the true position of *Sciara*. In the already quoted paper in the *Stett. Ent. Zeit.*, 1843, p. 29, line 8, Loew reproaches Meigen with having overlooked the close relationship between *Sciara* and *Lasioptera* (!), and with having placed it near *Mycetophila* (in which Meigen was right). He makes a very superficial comparison between the pupae of *Lasioptera* and those of *Sciara*, and infers from their supposed resemblance that there must be a relationship between the imagoes! When he objects to Zetterstedt having called *Sciara* a fungicole, he is right in so far only as its larva generally feeds on decaying matter (including dry cow-dung); but it also occurs in mushrooms (*teste* Dufour)¹; and at any rate Loew is in error when on that account he refuses to *Sciara* a place among the *Mycetophilidae*. He quotes Macquart ("Histoire Naturelle des Insectes Diptères," Vol. I, p. 126, 1834), who says that "*Sciarae*, in their organization, differ in some respects from the *Mycetophilidae*" ("n'appartiennent pas entièrement aux *Tip. fungicoles*"), which, however, did not prevent Macquart from leaving them in that family, to which they actually belong. In the "Dipterologische Beiträge," Vol. I, p. 10 (1845), Loew still entertains the opinion that *Sciara* must be placed at the end of the *Cecidomyiidae* and "that the *Psychodidae* might very well ('ganz füglich') be united with them in the same family, the principal character of which would consist in the absence of a discal cell." An arrangement corresponding to this assumption (and which I have already

¹ Dufour says (*Ann. Sci. Nat.*, 1839, p. 31, at top): "If, as Mr. Macquart maintains, the larvae of *Sciara* occur in turf ('terreau'), this fact must not be generalized, for the larvae of *Sciara ingenua* Duf. live in different kinds of mushrooms." [I have also bred *Sciarae* from mushrooms. — OSTEN SACKEN.]

mentioned on a preceding page) was given on page 10 of the "Dipterologische Beiträge," Vol. I (1845), where the whole group *Tipulae gallicolae* is divided into Section I, *Polyneura*, containing the *Psychodidae*, and Section II, *Oligoneura*, containing the *Cecidomyiidae*, and including also *Zygoneura* Meig. and *Anarete* Halid. (*Sciara* is omitted in this table, perhaps from inadvertence). In all these attempts an undue prominence is given to the venation.

The discussion in the "Dipterologische Beiträge," Vol. IV (*Cecidomyiidae*, 1850, p. 17), contains the same assertions on the close relationship between *Psychodidae* and *Cecidomyiidae*, except that here Loew allows himself to go a step farther, and to introduce his section *Tip. gallic. polyneura* (*Psychodidae*) as a separate family: *Tip. noctuaeformia*. He says: "The complicated structure of the mouth-parts which enables some species to become blood-suckers, and the observation that the larvae of some of them (compare Schrank, 'Beiträge,' p. 1) are aquatic and breathe with branchiae, are decisive."

The persistency of Loew in uniting the *Psychodidae* with the *Cecidomyiidae*, as late as 1850, proves that he had not paid attention to a passage in the first letter which Haliday had written to him in September, 1847, and which I discovered among the manuscript annexes to that letter (compare my Chapter VIII, Haliday and Loew). This passage, written in Latin, refers to *Psychoda* in Curtis's "British Entomology, Diptera," plate 745 (1839), and to its letter-press, in which Haliday had introduced a subdivision of this genus, adopting the new genera *Saccopteryx*, *Trichomyia*, and *Sycorax*. Haliday informs Loew in this passage that *Psychoda*, in the narrower sense, as defined in Curtis, requires a further subdivision into two genera: (1) *Psychoda* proper, and (2) *Pericoma*; he adds a short definition. This explains why, many years later, in Walker's "Insecta Britannica. Diptera," Vol. III, p. 256, where *Pericoma* appears for the first time in print, it is referred to as "Haliday MSS." In the same passage of his letter Haliday says about the relationship of the *Psychodidae*: "Nexum hujus familiae cum Gallicolis probare non queo, et si nomina clarissima Loewius, Westwoodius in illam partem abierint." He conjectures that these authors had been misled by the error of Bouché ("errore gravissimo permoti"), who

figured the larva of *Psychoda* as peripneustic. As these larvae are amphipneustic, they are, according to Haliday, more allied to those of the *Tipulidae*. And indeed, in Walker's third volume (1856), the *Psychodidae*, which Haliday now calls *Phlebotomidae* (after Rondani), are placed next to the *Tipulidae*. But in the same manuscript passage Haliday ventures a still bolder assertion ("si fas mihi audacter opinari"), that *Psychodidae* should be placed somewhere near the *Culicidae*, as the latter have ("Dufour docente") five Malpighian vessels, which agrees with what he had found himself in dissecting *Pericoma* and *Psychoda* ("quod tam in *Pericoma* and *Psychoda* valere comperi").¹ The discovery of this passage in Haliday's letter of September, 1847, gave me great pleasure, because I had, independently of him, although many years later, expressed the same opinion about the relationship of *Psychoda* with the *Culicidae* (in the *Berl. Ent. Zeit.*, 1892, p. 433, footnote; 130, 1892).

Considering his solution as regards the connection between *Psychodidae* and *Cecidomyiidae* as final, Loew ("Dipterologische Beiträge," Vol. IV, p. 17, 1850) passes to what he considers the more difficult question about the location of *Sciara* and *Zygoneura* (*loc. cit.*, p. 17, at bottom): "The *Sciarae*, especially the more delicate ones, closely resemble in their whole structure the gall-flies; their venation shows hardly any difference from that of gall-flies with five-veined wings; the apparently different structure of their filiform, never verticillate antennae, loses much of its importance when we consider that some gall-flies (*C. sarothamni*, *echii*, *ribesii* Meig.) show a similar structure; the shape and mode of life of the larvae of *Sciara* are not very different from those of gall-flies ('nicht gar sehr verschieden'); the spiracles in both are placed on the sides of the single segments, and some of them, like the majority of *Cecidomyiae* (e. g. *Sc. pruinosa* Bouché), are able to spin," etc. After this long exposé of the analogies, as if in justification of his earlier opinion, Loew proceeds to explain the differential characters of *Sciara*, as they had since become plain to him (the elongate

¹ This statement about the Malpighian vessels Haliday reproduces in his admirable paper, "On the affinities of the *Aphaniptera* among insects" (*Nat. Hist. Review*, January, 1856, p. 17, footnote).

coxae, the long spurs on the hind tibiae, the difference in the nature of the hairiness of the wings, the peculiar structure of the larvae, especially of their head, the absence, in the pupa of *Sciara*, of tubular, horn-like prothoracic spiracles).¹ He admits that, under such circumstances, the partisans of the opinion that *Sciara* is a Mycetophilid would be right, if the genus *Zygoneura* had not been discovered in the meantime. It holds, according to him, an intermediate position. "If *Sciara* is placed among the *Mycetophilidae*, *Zygoneura* would more naturally have to remain among the *Cecidomyiidae*. For a final decision, we must wait for the discovery of the early stages of *Zygoneura*." — Now I had, in 1862, described the early stages of this genus (in my above-quoted paper on the larvae of *Mycetophilidae*), and had shown that the larva of *Zygoneura* is quite like that of a *Sciara*, and the pupa shows the horn-like thoracic spiracles which occur in some species of *Sciara* (comp. figure 22 at the bottom of my plate). And many years after me, Beling (*Wiener Ent. Zeit.*, 1885, p. 308) likewise described the larva of *Zygoneura* as being exactly like that of *Sciara* ("der Larva einer *Sciara* vollkommen gleich gestaltet").

In 1862 ("Monographs North American Diptera," Vol. I), Loew finally placed *Sciara* among the *Mycetophilidae*, but, nevertheless, again with the remark (p. 13), "that it shows some affinity with the *Cecidomyiidae*." Whether this change of opinion had any connection with my publication on the larva of *Zygoneura* in the same year, I am unable to ascertain. And yet notwithstanding this affinity between *Mycetophilidae* and *Cecidomyiidae* thus correctly indicated, Loew placed the two families very far from each other, the *Blepharoceridae*, *Psychodidae*, and *Tipulidae* intervening between them! Thus the same Loew who had reproached Meigen with overlooking a pretended close relationship between *Lasioptera* and *Sciara*, and taunted Zetterstedt for considering *Sciara* as a fungicole, Loew, twenty years later, lost in his perplexities, had not reached any definite conclusion about *Sciara*! Haliday in

¹ In this Loew was mistaken; the pupae of some *Sciara* have such tubular spiracles, as Loew himself had stated in the *Stett. Ent. Zeit.*, 1843, p. 30. Compare also my paper: "On the larvae of *Mycetophilidae*" (15, 1862), or its reprint (113, 1886). The references will be found on p. 158 and 165 of my original edition, and on p. 11 and 17 of the reprint.

the meantime (in Walker's "Insecta Britannica. Diptera," Vol. III, p. 72, 1856) had correctly placed the *Cecidomyiidae* alongside of the *Mycetophilidae*, and referred *Sciara* and *Zygoneura* to the latter family.

Compared with other genera of *Mycetophilidae*, the position of which in this family has never been doubtful, *Sciara* is but very little aberrant. Its larva is much more like that of a true *Mycetophila*, than the larvae of some other, undoubted *Mycetophilidae*, like *Sciophila*, *Bolitophila*, *Ceroplatus*, without mentioning *Mycetobia*. Meigen had established a separate family, "Trauermücken," (*Lugubri*) for *Sciara* ("Systematische Beschreibung," Vol. I, p. xxxvi, 1818), and had observed in a footnote: "Latreille wrongly unites this genus (*Molobrus* Latr. = *Sciara*), and also *Rhyphus*, with the *Mycetophilidae*." Winnertz and Rübsaamen (*Berl. Ent. Zeit.*, 1894, p. 19), with regard to *Sciara*, have followed Meigen. But Haliday, Macquart, and Schiner were right in following Latreille.

If I have insisted upon Loew's earlier utterances concerning questions of classification, it is because they contain much that foreshadows his later career in this specialty: his want of method, his inability to lay hold of the leading characters, and, as a consequence, his irresolution, combined at the same time with an habitual acerbity of his often unjust criticisms of other authors.

Many other instances may be adduced to prove that Loew was less gifted with a spontaneous insight into the natural affinities of Diptera than his contemporaries, Robineau-Desvoidy and Rondani.

In my chapter on Rondani (Chapter XIX) I shall give a detailed account of the unjust and arbitrary proceedings of Loew towards him in regard to the subdivision of the *Cecidomyiæ*. The sections of *Cecidomyiæ* and *Lestremiæ*, correctly defined by Rondani as early as 1840, Loew at first called a useless splitting ("Zersplitterung," *Stett. Ent. Zeit.*, 1847, p. 146, line 6). His next step (1850), when better informed, was to recognize that Rondani was right, but that the name *Lestremiæ* should be changed into *Anaretina*; finally in the "Monographs," Vol. I, p. 7 (1862), he adopted Rondani's subdivision and its definition, without the slightest acknowl-

edgment to him, maintaining at the same time the change of name. This change was unfortunate, because Loew's pretended *Anarete* proved, much later, not to be the *Anarete* of Haliday at all! Mr. J. J. Kieffer, in 1898, discovered a Cecidomyid in which he recognized *Anarete* Loew, but did not succeed in identifying it with *Anarete* Halid.; he applied to me (January 24, 1898) for an explanation, and I referred him to my statement in the *Berl. Ent. Zeit.*, 1892, p. 451 (130, 1892), where I had proved that Schiner was right in referring *Anarete* Halid. to the *Scatopsina*. Whereupon Kieffer proposed the name *Pseudanarete* (*in litteris*) for the genus of Loew. I am not aware that he has published it since.

In consequence of Loew's erroneous determination of Haliday's *Anarete*, the following passage in "Dipterologische Beiträge," Vol. IV, p. 19, 1850, requires a correction: "Attention should be called to the hitherto entirely overlooked relationship of the *Cecidomyiæ* with the genus *Scatopse*; the venation of *Lestremia* and *Anarete* is like that of *Scatopse*, etc." This *Anarete* Loew is the Cecidomyid *Pseudanarete* Kieffer (*in litteris*). If Loew had had the true *Anarete* Halid., he would have been right in connecting it with *Scatopse*. As it is, the comparison which he draws between *Cecidomyia* and *Scatopse* has no foundation. *Scatopse* being a close relative of *Bibio*, as Loew himself had recognized in his paper in the *Linnaea*, Vol. I, p. 324 (1846), it would follow from his conclusion that *Bibio* is a relative of *Cecidomyia*, which is a *reductio ad absurdum*.

Still later, in 1858, Loew suggested other unexpected and fanciful ideas about the relationship of the *Bibionidæ*. It was his fixed but erroneous notion that the structure of the antennæ constitutes the only difference between the *Nemocera* and *Brachycera*, and hence, as late as 1858, we find in his paper: "Ueber einige neue Fliegengattungen" (*Berl. Ent. Zeit.*, 1858), on p. 102, at the bottom, the following incongruous passage about the genus *Rhachicerus* Halid., and the Amber genera *Chrysothemis* and *Electra* Loew.

"These three genera have their natural position among the *Xylophagidæ*, which is indicated by the structure of their mouth, as far as it is known. But the circumstance that this arrangement tends somewhat to obliterate the line of separation between the two principal divisions of Diptera, was the only reason which induced me, in my paper on Amber, to admit *Chryso-*

themis and *Electra* among the *Nemocera*, and to place them, notwithstanding their complicated venation, at the end of the family *Bibionidae*. In that paper I thought it convenient to adopt this arrangement for the benefit of those who had not made the Diptera their special study, the more as the transition from the *Nemocera* to the *Brachycera* is effected by the *Bibionidae* on the one side, and the *Xylophagidae* on the other, and because, in that paper, I had placed the *Bibionidae* at the end of the one division, and the *Xylophagidae* at the beginning of the other. That the three genera, *Rhachicerus*, *Chrysothemis*, and *Electra*, find their natural position among the *Xylophagidae*, I have never called in doubt."

Such were the confused notions of Loew on the true characters of the two fundamental divisions of Diptera, after eighteen years of study of this Order! He persisted in these same notions in his lecture on Amber Diptera at the meeting of the German Association of Naturalists in Königsberg (1860), and in the Preface of the "Monographs North American Diptera," Vol. I, p. 3-4, 1862. In this last work he says: "It is a fact that some discoveries made in modern times have obliterated to a certain degree the sharpness of the limit which was considered to exist between the *Nemocera* and *Brachycera*." And farther on: "All these facts, however, are not sufficient to compel us at present to give up the separation of the *Nemocera* and *Brachycera*." In my Introduction to this "Record," p. 11, I have referred to a passage in Loew's letter to me, dated July 25, 1860, in which he formally expresses the opinion that "the coalescence of *Nemocera* and *Brachycera* is an accomplished fact," and that he would have introduced it into the new Catalogue of North American Diptera, if he had been the author of it. In one of the last letters I received from Loew, in August, 1877 (already quoted by me on p. 97), he mentions some of his nearly finished, or unfinished papers, and among them of "preparations for a reform ('Neubearbeitung') of the System of Diptera, for the intended new edition of your Catalogue of North American Diptera." After all the utterances of Loew quoted by me above, there can be no doubt that the intended reform would have consisted in the coalescence of the two principal divisions of Diptera!

Loew would not have fallen into this capital error if he had consulted Latreille's "Genera," etc. (1809), p. 238, where the concept of the *Nemocera* Latreille (which at that time were called *Tipulaires*)

was based on the number of joints, not of the antennae only *but also of the palpi*. The name *Nemocera* was introduced by Latreille for the same concept eight years later, in 1817. (For details, compare my paper: "On the characters," etc., in *Berl. Ent. Zeit.*, 1892, p. 413-422; 130, 1892.)

As in my translation of the above quoted passage I have rendered the sense of Loew as clearly as possible without attempting to be literal, I reproduce here the German original: "Alle drei Gattungen haben ihre natürliche Stellung bei den Xylophagiden, auf welche auch der Bau der Mundtheile, soweit er bekannt ist, hinweist. Nur der Umstand, dass dadurch die Gränzlinie zwischen den beiden Hauptabtheilungen der Diptern etwas verwischt wird, hat mich bestimmt in meiner Schrift über den Bernstein die Gattungen *Chrysothemis* und *Electra* den Nemoceren beizurechnen, und sie trotz dem zusammengesetzten Flügelgeäder als letzte Gattungen zu den Bibioniden zu bringen; ich glaubte dort mir dies denen zu Gunsten erlauben zu können, welche sich nicht speciell mit dem Studium der Diptern beschäftigen, da in der That der Uebergang von den Nemoceren zu den Brachyceren durch die Bibioniden einerseits und durch die Xylophagiden andererseits vermittelt wird, und da ich in jener Schrift erstere mit den Bibioniden schloss und letztere mit den Xylophagiden begann. Dass die drei Gattungen *Ruchicerus*, *Chrysothemis* und *Electra* ihren natürlichen Platz aber durchaus nur bei den Xylophagiden haben, ist mir nie zweifelhaft gewesen."

I have mentioned (p. 109) Loew's attempt in 1844 to connect the newly discovered genus *Liponeura* with *Macropeza*, *Diamesa*, and other genera, which have nothing to do with it. Several years later Loew became more intimately acquainted with the *Blepharoceridae*, and published two successive monographs of them (1869 and 1877). In these papers we find him hesitating in the choice of a character for a primary subdivision. In the first paper, published in Haliday's Italian translation of it, he adopted as such the length of the proboscis; in the second, he made use for the same purpose of the spurs at the end of the hind tibiae. In my successive papers (60, 1878; 124, 1891; 144, 1895) on the same subject, beginning with the first, which appeared a year after Loew's last paper, I have shown that, for a primary divisional character of the family, the presence or absence of the incomplete vein near the posterior margin, as far as our present knowledge goes, is more satisfactory than the characters proposed by Loew. A singular proof of Loew's too great reliance on the venation, as an index of relationship, I find in a letter to me (October 1, 1858), written

just after he had received the remarkable Blepharocerid *Hammathorrhina* from Ceylon. He discovered in this insect a relationship with *Cecidomyia* ("ein Fingerzeig nach *Cecidomyia*"), a surmise which, of course, had no other foundation than a superficial resemblance of the incomplete venation of this genus with that of some *Cecidomyiæ*.

In my paper, "Contribution to the Study of *Liponeuridae*" (*Berl. Ent. Zeit.*, 1895; 144, 1895), I published (p. 163-165) a criticism of Loew's erroneous synonymy of *Blepharocera fasciata* and of *Liponeura cinerascens*. This very intricate case occurred during Loew's latest period, when he was already disabled by illness, and is therefore easily excusable.

Concerning Loew's constant and total neglect of the *Tipulidae*, I shall only recapitulate what I have said before in Chapter II. Loew's first attempt at a classification of this family appeared in his paper: "Ueber den Bernstein und die Bernsteinfauna" (1850). Of the utterly unscientific method used by him in introducing this classification I have given an account, based upon his own confession (compare Chapter IX). Since that first attempt, Loew seems to have conceived a singular aversion for any study, or improvement, of the classification of the *Tipulidae*, as if he felt equally reluctant to give up his own work, or to have it demolished by others. I cannot otherwise explain the supercilious treatment he gave to my classification of this family. I have shown elsewhere (p. 63) that in Dr. Peters's "Reise in Mozambique" (1862) Loew had published, under the generic name of *Limnobia* in the widest sense of Meigen, a Tipulid which belonged to the section *Anisomerina*. I have also shown (p. 64) that, during more than fifteen years, Loew had taken for congeneric the Amber Tipulid which he called *Toxorrhina*, and a recent Tipulid from the West Indies, which he had described and figured under the same name, although the former has a normal, and the latter a totally different and very anomalous venation. It was towards the end of his career only that Loew showed me some justice, and urged me to overhaul his Amber Tipulidae and to apply to them my systematic arrangement (see p. 66, above).

About the work of Loew on the classification of the *Orthor-*

rhapta Brachycera, I shall be less circumstantial, as my acquaintance with this suborder is comparatively limited.

It could not have been expected that in 1860-1862 (in the "Dipteren-Fauna Südafrika's" and in "Monographs," Vol. I, p. 15-18) Loew should have had a clear insight into the grouping of the *Beridina*, *Xylophagidae*, and *Chiromyzidae*, for it is not quite cleared up to this day; and yet he might have avoided the mistakes of placing *Chiromyza* among the *Beridina* ("Dipteren-Fauna Südafrika's," p. 1, 1860); of confounding (*ibidem*) *Metoponia* Macq. (an Australian Chiromyzid) with the North American Berid, *Beris fuscitarsis* Say (for which I later proposed the generic name *Allognosta*, *Berl. Ent. Zeit.*, 1883, p. 297)¹; and of not noticing that Latreille's *Xylophagus* is our *Subula*, and that his *Pachystomus* is nothing but our *Xylophagus* described from a specimen with mutilated antennae (compare Osten Sacken, *Berl. Ent. Zeit.*, 1882, p. 379). The fact that the family *Xylophagidae* had no consistency whatever, being based upon an erroneous juxtaposition of two very different forms, *Xylophagus* and *Subula*, was at that time perhaps not obvious enough for Loew to perceive. About the Sections *Sargina*, *Odontomyina*, and *Pachygastrina*, Loew has produced excellent descriptive work.

In preparing his monograph of the European *Tabanidae* (1858), Loew did not notice that the development of the ocellar tubercle, combined with other characters, offers a very good basis for the establishment of a separate genus. This oversight led him to reject the genus *Therioplectes* proposed by Zeller sixteen years before, without perceiving that, as I have later shown, a slight modification of the definition makes this genus answer its purpose very well. Compare my Prodrömus of the *Tabanidae* (1875), p. 425 (43, 1875).

¹ In the "Dipteren-Fauna Südafrika's" (1860), p. 1 at bottom, Loew says that *Metoponia* Macq. is much more related to *Beris*, than to *Chiromyza*, a statement which is erroneous, because based upon the misinterpretation of the genus *Metoponia*. What Loew further says in the same place, "that *Inopus* Walk. (Ins. Saund.) is the same as *Metoponia* Macq.," is incomprehensible, because *Metoponia* Loew (nec Macq.) is *Allognosta* O. S., and the latter genus has nothing to do with *Inopus* Walk. As to the true Australian *Metoponia* Macq., "Diptera Exotica," Suppl., Vol. II, p. 28, Tab. I, f. 4, it is quite different from *Inopus*, has a different venation, and belongs to the *Chiromyzidae*.

In 1872, Loew described a North American *Chrysops* (*C. gigantulus*). In working up my Californian collection of Diptera, I described the same species as *Silvius trifolium*, sp. n., because it never occurred to me to look for it among the described species of *Chrysops*. During our interview in Guben (in 1877), I asked Loew for the reason of this strange location by him of *gigantulus*. He merely answered, "It does not look like a *Silvius* at all." But it looks still less like a *Chrysops*!

In his "Dipteren-Fauna Südafrika's" (1860), Loew introduced two sections of the *Cyrtidae*: *Oncodina* and *Cyrtina*. This subdivision is reproduced in "Monographs," I, p. 21 (1862). As it was based merely upon the venation, which is very unsteady in this family, the subdivision soon proved a failure. According to this grouping, the closely allied genera *Philopota* W. and *Helle* O. S. would have to be placed in two different sections; Schiner's subdivision ("Diptera Novara," p. 140-145, 1858) seems to be more satisfactory, at least in the adoption of the natural section *Philopotina*. It is remarkable that in all his papers on *Cyrtidae*, in which Loew had described sixteen species, he had never noticed that the first posterior cell, in all the known species of that family, is bisected by a cross-vein, which is, so far as known, a character peculiar to the family. When I first noticed this character in preparing my (as yet unpublished) paper on *Cyrtidae*, I was much puzzled by Loew's description and figure of *Pithogaster* (*Opsebius* Costa) *annulatus* Loew (*Wien. Ent. Monats.*, 1857, p. 33, Tab. I, f. 1) in which that supplementary cross-vein is neither figured, nor mentioned in the letter-press, so that at first I took this case for an exception to the rule. In examining since Loew's type-specimens in the Berlin Museum, I ascertained that the cross-vein *is present*, and, therefore, that Loew had not noticed it!

It would almost seem as if Loew's exuberant working-power made him impatient of any delay, and drove him to begin with describing before having mastered his subject by preparatory study. This haste is most conspicuous in one of his principal works, I may say his life-work, the Monograph of the European *Asilidae* ("Ueber die europäischen Raubfliegen," in the *Linnaea Entomologica*, Vols. II, III, IV, V, 1847-1851). Loew, who

was always recommending the use of what he called plastic descriptive characters, did not perceive in this case the difference between macrochaetae and ordinary bristles, and thus missed one of the most useful descriptive characters in this family. The short prefatory notice (of only eight pages) preceding the descriptive portion of his work on *Asilidae* (*Linnaea*, Vol. II, p. 384–392, 1847) is nothing but a rather superficial survey of previous publications. It also contains a paragraph on “Nomenclature,” establishing rules which Loew himself, in later years, has not observed. A few years before, in 1840, in the Preface of his second edition of the “Posener Dipteren,” Loew had likewise proposed a very inexpedient rule of nomenclature, which he never followed afterwards. (Compare my paper in the *Berl. Ent. Zeit.*, 1896, p. 284, in the footnote ; 153, 1896.)

It was in that portion of his Monograph only in which Loew reached the difficult Section of *Asilina*, that he found it necessary to explain himself more distinctly on the use of the terms referring to bristles, hairs, etc., which play so prominent a part in the descriptive paragraphs of that section. This explanation (*Linnaea*, Vol. III, p. 419–421), for the reason already stated, the inattention to macrochaetae, must be considered as a failure. Three years before this publication of Loew, Rondani (*Nuov. Ann. Sci. Nat.*, Bologna, 1845), in a paper on *Tachinidae*, had distinguished macrochaetae from ordinary hairs and bristles, and introduced this term for them, and it is to be regretted that, after this good beginning, he has not developed the advantages of this innovation, and has not made use of it in his later publications. (A detailed notice of this paper of Rondani in 1845 will be found in my Chapter XIX.) Loew was in possession of Rondani's paper, but, instead of paying attention to this important progress, he contented himself with eleven lines of supercilious and flippant criticism of it in his general article upon Rondani's publications in the *Stett. Ent. Zeit.*, 1847, p. 155.

In my “Essay of comparative Chaetotaxy,” etc. (102, 1884), I have taken notice of Loew's neglect of macrochaetae in the following terms : —

"There is a mine of systematic characters yet to be explored in the study of the Chaetotaxy of the *Asilidae*. As an instance I will cite *Dasyopogon diadema* and *D. teutonus*, placed by Loew in the same ultimate subdivision of *Dasyopogon* (*sensu stricto*); a glance at the Chaetotaxy of these species shows that *D. teutonus* has no bristles on the scutellum, while in *D. diadema* the scutellum is beset with 4-6 strong macrochaetae. A further examination reveals other abundant differences in the arrangement of the bristles on the thorax and on the legs; it discloses at the same time other characters peculiar to each of the species, in the shape of the abdomen, in the structure of the forceps of the male, etc., — all of which tend to prove that these species belong to two very distinct genera. And yet, when we read the descriptions (not excepting even those of Loew), we find almost nothing but colors mentioned."

There is no doubt that Loew's work on European *Asilidae*, in bringing together a mass of new material satisfactorily (for the time being) grouped and described, represented, when it was published, a considerable progress, and rendered a real service to European dipterology. But it must be acknowledged at the same time that, considering the large amount of material Loew had at his disposal, he might have done more for the classification of the family in general, and thus paved the way for dipterologists of other parts of the world besides Europe.

The above paragraph was already written when I received the April number of the *Wiener Ent. Zeit.*, 1899. It contains an article by Mr. F. Hendel of Vienna: "Ein verschollener *Asilus* Central-Europa's." The author shows that the concepts of several of the genera, carved out by Loew from the old genus *Asilus*, are very indefinite and unsatisfactory, and that Loew himself, when he obtained new species, was doubtful in which of these genera to place them.

In attempting to subdivide the *Bombyliidae*, Loew met with many perplexities. In the *Linnaea*, Vol. I (1846), p. 371, he said: "The separation of the genus *Lomatia* from *Anthrax* will be very difficult to maintain, as both, although sufficiently distinguished in their European representatives, seem to coalesce so much in their exotic species that the limit between them becomes very indefinite." Loew was not more advanced fourteen years later in the "Dipteren-Fauna Südafrika's," 1860, p. 176, at top. Here he closes a very long discussion with the humble confession that "finally nothing remains to be done but to be governed by a certain instinct in distinguishing the general appearance ('Gesamthabitus'), like that of *Bombylius*, from that of an *Anthrax*. . . .

That such a subdivision would be quite arbitrary, and without any scientific value whatever, is self-evident." My own little experience of the *Bombyliidae* has taught me that a subdivision of them, not into two, but into several groups is quite feasible. Schiner had paved the way towards such a subdivision in his "Novara Diptera," p. 116 (1868), and, eighteen years later, I have added my mite of suggestions in the chapter on *Bombyliidae* in the *Biol. Centr. Amer. Diptera* (1886), p. 75-162.

One of Loew's favorite subjects was the family *Dolichopodidae*. As I have not made a thorough and independent study of this family, I shall confine myself here to bringing together a few literary data which may be useful in future.

Loew's last and principal work on this family is contained in the second volume of the "Monographs of North American Diptera," Smithsonian Institution, Washington, January, 1864. The volume had already gone through the press when a *Supplement* came in from Loew, containing descriptions of some new species sent to him by me in the meantime, and also a chapter of "General remarks on the generic characters." This *Supplement* will be found at the end of the volume, accompanied by a footnote signed by me (p. 321).

This publication offers the most complete and matured exposition of Loew's views on the classification of the *Dolichopodidae*.

The family (*loc. cit.*, p. 14) is divided into two large divisions, distinguished from each other by a single character: the presence, or absence, of short hairs on the upper side of the first joint of the antennae. The consequence of this subdivision is that the genus *Argyra* Maeq. is cut in two: the larger part of it appears (*loc. cit.*) in the *first* division, as Gen. XIV, *Argyra*, and the rest in the *second* division, as Gen. XXVI, *Leucostola*. On p. 151 of the same volume, under the heading of *Leucostola*, we find this statement: "The close relationship of the genus *Leucostola* to that of *Argyra* can be easily perceived by a comparison of their characters. There is scarcely any difference between them, but that the first joint of the antennae of *Leucostola* is entirely without any hair, while in *Argyra* it is distinctly covered with hair." Such a statement contains in itself a condemnation of the subdivision proposed by Loew for the family! What process of reasoning may have induced Loew to

adopt it? In the paper: "Die Familie der Dolichopodiden" ("Neue Beiträge," Vol. V, 1857), the earliest monographic publication of Loew on this family, I find a passage about this same character which, nevertheless, does not afford the desired explanation; on p. 39, line 12, in discussing *Argyra*, Loew says: "Among the common species, *Argyra vestita* forms an exception in the bareness of the upper side of the first joint of the antennae. In all other characters . . . it agrees so well with the typical species of *Argyra*, that I would have willingly ('gerne') left it in the same genus. The bareness or hairiness of the first joint of the antennae is such an important character for the often difficult determination of female specimens, that species which do not agree in this character cannot possibly be generically united. It becomes therefore necessary to form a separate genus for this species, for which I accept the very characteristic name *Leucostola* proposed by Mr. Haliday."

The weakness of this reasoning is evident. Should it even be proved that the character in question is available for the genus *Argyra*, it does not follow that it can, without any further discussion, be applied to the whole family of *Dolichopodidae*. Nevertheless, four years later, we find this character adopted in the monograph of the North American *Dolichopodidae*, published in German ("Neue Beiträge," Vol. VIII, 1861, p. 1), and afterwards in the Synoptic Table of the English edition of the same monograph ("Monographs of North American Diptera," Vol. II, 1864), as I have shown above. Schiner ("Fauna Austriaca," Vol. I, p. 188) was therefore quite right in *not* adopting the genus *Leucostola*.

In the collection of letters addressed by Haliday to Loew on dipterological subjects (and now in possession of Mr. Verrall, see above, p. 51), the discussions about *Dolichopodidae* occupy a considerable space. In a letter dated from Dublin, Nov. 22, 1856, I found the following passage, which I reproduce exactly as it stands, without quite understanding its meaning (the italics are Haliday's): "I scarcely expect that the character of the *naked* or *pubescent* 1st joint of the antennae will seem to you of the weight I have ventured to give it, but will ask of you to examine *the result*, as to the group it defines, and tell me if it does not *indicate* pretty truly a series of affinities which may be probably better *defined* by some *other* character. I admit, one or two instances, already known to me, seem to militate against it, *e. gr.* *Argyra vestita*, separated in a different tribe from the rest of the genus as received, etc."

As this letter of Haliday, dated November 22, 1856, was closely followed by Loew's monographic publication on European *Dolichopodidae* in the "Neue Beiträge," Vol. V, 1857, it would appear that Loew in this case was under the influence of Haliday. However, the same letter of Haliday contains a scheme of a distribution of *Dolichopodidae* into sections, in the synoptic table of which the pubescence or bareness of the first joint of the antennae plays an important part, but is not the *basis* for a primary subdivision, as it is in Loew's table. ·

This collection of letters will probably afford an abundant material to the future student for the investigation of the influence upon each other of the two eminent dipterologists in the classification of the *Dolichopodidae*, but such an investigation is entirely beyond my competence.

In the *Empidæ* (in the broadest sense), Loew, so far as I can judge, has done excellent descriptive work, both for Europe and for North America. But, in the "Monographs of North American Diptera," Vol. I, 1862, he committed a mistake in not following Haliday ("British Entomology. Diptera," Vol. I, 1851), and in not uniting into a single family the *Hybotidae*, *Tachydromidae*, and *Empidæ*. I have already alluded to this subject, above, on p. 49, in the Chapter VII ("Historical and critical remarks," etc.) in which I have introduced some criticisms of the generally unsatisfactory classification of the Diptera *Orthorrhapha* in that volume of the "Monographs."

In working up the North American *Syrphidae*, Loew, to use his own expression (*in litteris*) "stuck in a jungle of uncertainty" in attempting to disentangle the existing descriptions of the North American species of *Syrphus* (in the narrowest sense). With more material at my disposal, I succeeded better in this task, and thus excited Loew's astonishment to such a degree that it may have been the cause of the unexpected rupture of his correspondence with me, which was resumed only two years later. (The story of this curious incident has been related by me in detail in Chapter XII.)

Loew, as he once admitted to me in conversation, had made but very little study of the *Calyptrata* (in Rob.-Desvoidy's sense) and he therefore did not publish much about them. His papers on the genera *Homalomyia* and *Azelia* are praised by a specialist, Mr. P. Stein. This author, in his paper on "North American *Anthomyiidae*" (*Berl. Ent. Zeit.*, 1898), complains of Mr. F. Walker's numerous descriptions and adds: "How different, in comparison, are

the descriptions of Loew, in which it is almost impossible to mistake the species. And it is for this reason very much to be regretted that this veteran in dipterology had no taste for *Anthomyiidae*, as appears from a very drastic utterance which he made to my respected friend von Röder in Hoym, when the name of the genus was mentioned: 'Preserve me from that vermin' ('Gehen Sie mir mit dem Geschmeiss')! It is indeed remarkable that Loew persistently resisted the attraction which the unsightly but interesting *Anthomyiidae* afford, and, with the exception of a few genera (*Homalomyia*, *Azelia*, *Lispe*), would never have anything to do with them. The few descriptions of North American *Anthomyiidae* which he gave in the 'Centuriae' leave nothing to desire ('sind mustergültig')."

Loew's new genus *Blaesoxipha* (*Sarcophagidae*, 1861) provoked an apparently well-merited criticism of Schiner (*Verh. zool.-bot. Gesellsch.*, Wien, 1863, p. 1037).

Loew's field of predilection (and the one in which he excelled) was *minutiae*, as he himself says in his letter to me of November 10, 1862 (already quoted on p. 50): "*Minutiae* are my true specialty, particularly when they fall within the domain of the *Acalyptrata*!" This preference may be explained by the fact that *Acalyptrata*, in general, offer characters more definite and easier to get hold of than some families with species of larger dimensions. Genera and species of *Tabanidae* and *Asilidae*, for instance, are more difficult to define (so as to be recognizable in a description) than most of the *Acalyptrata* (*Ortalidae*, *Trypetidae*, *Helomyzidae*, *Ephydriidae*, etc.). For Loew's keen eye, size made no difference; but owing to his very moderate ability to discern leading characters, he was led instinctively to recognize that his work on *Acalyptrata* was more satisfactory to himself than perhaps any other work, and hence in the latter part of his career he became especially devoted to it. In the *Acalyptrata*, macrochaetae are less mixed up with other hairs, and, I may say, obtrude themselves more easily upon the attention. This explains why Loew began to make a regular use of macrochaetae in his descriptions of *Acalyptrata*, and especially in his paper "On the European *Helomyzidae*" (*Schles. Zeitschr. Ent.*, 1859). But he did not, by far, exhaust this new

resource, even in this paper. If he had reduced Chaetotaxy to a system, he would have found it most serviceable in his later works. The thoracic bristles, for instance, were too much neglected by him in his publications on *Ortalidae*.

Of Loew's greatest delusion, the supposed coalescence of the two larger divisions adopted by Latreille, *Nemocera* and *non-Nemocera* (*Brachyera* Macq.), I have already spoken above, on p. 48, as well as in my "Introduction," p. 11. Macquart showed a clearer insight into that matter when he said (in his "Insectes Diptères du Nord de la France," 1823) "that the *Nemocera* constituted almost a different order, and that the difference between them and the other Diptera might almost be compared to that between the Hymenoptera, for instance, and the Neuroptera." (This passage has been quoted by me in the *Berl. Ent. Zeit.*, 1892, p. 419; **130, 1892**).

If Loew (as I said on p. 106) did not publish much on the biology of Diptera, it was because he seems, in general, to have taken but little interest in *living* Diptera, and in this he stood in striking contrast to Zeller. Loew's principal preoccupation when he was in the field, hunting flies, seems to have been *to bag* as many as possible. A characteristic paper in this respect is his "Eine dipterologische Razzia auf dem Gebiete des naturw. Vereins für Sachsen und Thüringen" (*Zeitschr. gesamt. Naturw.*, August, 1857). Loew reproaches the local "Natural History Society of Saxony and Thüringen" with their sluggishness in collecting Diptera, and shows them what can be done in their district. He spent a week in July in collecting near Wernigerode, in Prussian Saxony. The season notwithstanding, which during that year had everywhere been unusually unfavorable for insects ("ein besonderes insectenarmes Jahr"), and in spite of the unpropitious weather, which prevailed during the week of his collecting, Loew caught two hundred and forty-seven species of *Brachyera* alone, of which he published an annotated list. He did not count the Diptera which he had merely seen, but not caught ("Die Aufzählung von mir blos *gesehener* Dipteren scheint mir bedenklich," etc.). Among the captured Diptera *seven* species were described as new.

Another exploit of the same kind is related in the article, "Ueber die, etc., auf der Ziegelwiese bei Halle beobachteten Dipteren" (in the same *Zeitschr. gesamm. Naturw.*, 1864, p. 377-396). Like the preceding article, this was written for the edification and encouragement of the same Saxo-Thüringian Natural History Society. Loew takes care to explain at great length the most unfavorable conditions under which, in July, 1864, he made four forenoon excursions in a limited locality ("Ziegelwiese") near Halle; two of these excursions lasted about three hours, the two others only one hour; in all, he spent eight hours upon them. During this comparatively short time he captured one hundred and eighty-five species, which make an average of twenty-two species an hour. But, as it appears from Loew's account that of many species he took more than one specimen (of *Tetanocera halensis*, sp. n., he took *ten*), the whole booty represents an almost incredible amount of work! And this work was performed by a man convalescent from a severe illness ("eben überstandene schwere Krankheit"), and debilitated by daily salt baths ("der angegriffene Zustand, in welchen mich täglich gebrauchte Soolbäder versetzten").

In a letter to me (23d April, 1879), Zeller invited me to join him in an excursion to the "paradise of flies," as he called it, Bergün in Switzerland, and added the following good-natured allusion to Loew's passion for *bagging* as many flies as possible: "With this invitation I do not connect any *arrière pensée*, as Loew used to do when he happened to invite me to accompany him on an entomological excursion. I will not use you in a lepidopterological capacity for the purpose of helping me to reach the number of *one thousand*, which I expect to be the total number of the Lepidoptera occurring in that locality."

Concerning Loew's accuracy in recognizing species described by other authors, — in other words, his capacity for *determining* Diptera, — I am prevented from having any opinion (as I have expressed it above, p. 104) in consequence of my want of a special knowledge of the European fauna. Schiner, in this respect, has more than once published remarks that deserve attention in regard to the arbitrariness and untrustworthiness of some of Loew's changes in the adopted nomenclature, and especially on his reviving very doubtful specific names introduced by older authors. One of such articles of Schiner will be found in the *Verh. zool.-bot. Gesellsch.*, Wien, 1872, p. 63 sqq., on the synonymies of some *Empidæ* and

Chloropidae. Whether Schiner is always right I am incompetent to decide, but what induced me to refer to the question of recognizing descriptions is the experience I have had of Loew's method in that matter.

For the purpose of recognizing an insect in one or more descriptions, two different methods may be used, one of which may be called the *method of comparison*, and the other the *method of visualization*.

The *method of comparison* is the ordinary one, when the specimen to be determined is held up and compared in every detail with the description. In using this method, one is often bewildered by discrepancies, especially when the description is long.

The *method of visualization* consists in examining the specimen first, and impressing its principal features upon one's memory, so as to be capable of *visualizing* it in its absence. The next step is to read the description (or descriptions, when there are several), and, while doing it, to build up the described insect in one's imagination. Thus a mental image is produced in which the visualized species to be determined can easily be recognized, even in the absence of the specimen. After having thus selected a description apparently answering the specimen to be determined, the description is read for a second time with the specimen in hand; and this second reading enables one to decide whether the discrepancies are important or not, and, in the latter case, to accept the identification.

The method of *visualization* is quicker and surer than the other, and, with it, I have often succeeded in deciphering Walker's sometimes long but unmeaning descriptions. In the following instance the advantage of the method of visualization was unmistakably proved. Loew had prepared a preliminary list of American *Dasypogonina*, in which the species known to him were distributed among his new genera. Some of Walker's and other unrecognizable specific descriptions were enumerated under the heading *Dasypogon* in the widest sense. Loew challenged what he called my perspicacity ("Ihren Scharfsinn") in unravelling some of Walker's species of the latter group. I find my identifications (which Loew accepted) enumerated in a letter of mine of October 9, 1874, of which I re-

produce the corresponding passage: "The species of *Dasygogon* which you left to my perspicacity to interpret I have subjected to a critical scrutiny. *Dasygogon rufescens* Macq. is very probably *Diognites discolor* Loew; Macquart has overlooked the spurs on the front tibiae. *Dasygogon fallo* Walk. is *Cyrtopogon chrysopogon* Loew; in Walker's description you must read *face* instead of *front*. *Dasygogon macerinus* Walk., wretchedly described, seems to be your *Anisopogon gibbus*. Finally, *Dasygogon lutatius* Walk. is the species which, in my last invoice, I sent you under No. 458," etc. This last species is introduced into my Catalogue of 1878, p. 69, as *Cyrtopogon lutatius* Walk.; the other species will be found in their places as probable synonyms of Loew's corresponding species.

To the same wrong method of determining Diptera may be attributed Loew's mistakes in the interpretation of generic descriptions. The genus *Metoponia* Macq., an Australian Chironomid, he identified with a North American Berid (compare above, p. 119); the North American Tipulid *Cladura* O. S. he wrongly recognized in a small European Limnophilid (compare my "Studies on Tipulidae," Vol. II, p. 206 at top); *Anarete* Hal. he mistook for a Cecidomyid (compare above, p. 115); his *Chrysops gigantulus* was in reality a *Silvius* (compare p. 120), etc.

For a responsible entomologist it is quite indispensable to possess this faculty of visualization, and it would be quite expedient, I think, to examine aspirants for appointments in museums in regard to it just as candidates for railway service are examined for color-vision.¹

In my Chapter I, "On the Beginnings of my Relations with Loew," etc., I have complained of my inability to come to an understanding with him concerning the terminology of Diptera. Whenever I inquired about his views on the terminology of the venation, or of the sclerites of the thorax, I found him unprepared and hesitating, but profuse of promises. So, after much correspondence about venation, he wrote me (March 4, 1868): "My work on venation you shall receive immediately, but I must first make a fair

¹ This account about "the two methods of determining Diptera" I have published in the *Ent. Monthly Mag.*, London, December, 1901.

copy of it." I never received anything of the kind, and in a letter of December 22 of the same year there is a long, confusing paragraph about *marginal* and *submarginal* cells, without any mention of the promised paper. Loew's terminology in "Monographs," Vol. I, p. xxiv (1862), with the *intercalary veins*, etc., did not suit me at all, although he considered it as the final exposition of his views on the subject, and, as late as 1868 (in his "Beschreibungen europäischer Dipteren," Preface, p. xii), recommended it to his readers for fuller information. In my *second* monograph on the *Limnobia* (1869), I finally concluded to adopt Schiner's view of the *fourth vein*, which did good service as a working hypothesis.

In my paper (*Berl. Ent. Zeit.*, 1896, p. 287; 154, 1896), "Notice on the Terms *Tegula*, *Antitegula*," etc., I have shown that Loew had misused the term *tegula*. In this paper I said that in editing the first volume of the "Monographs," etc., I had of course no right to change Loew's terminology, but that I had taken the liberty, in the footnote of page xiv, to remark: "Some authors call them squamae. — O. S." I have also had a discussion with Loew about the term *arista*. Loew had always used the term "Fühlerborste" (in Latin *seta antennarum*), which, in Vol. I of the "Monographs," translated under his supervision, was rendered by the term *antennal bristle* (*loc. cit.*, p. 34, 35, and *passim*). This term is misleading, because it suggests the idea that this organ has something in common with a real *bristle*, whereas it must be considered *as a part of the antenna*. In the following volumes of the "Monographs," translated under my supervision, I adopted therefore the term *arista*, that had been used in England. Loew objected to it: "Arista has much against it" ("Arista hat manches gegen sich," letter of April 14, 1863); nevertheless, this time I persisted. This unpreparedness of Loew in matters of terminology became still more apparent when the Smithsonian Institution asked him to draw up a General Introduction to Diptera, to be placed at the head of the first volume of the "Monographs." This very natural request caused Loew the greatest perplexity; he accomplished the task, but with "great effort," and confessed at the same time that he had "learned much during this work." (Compare my account in Chapter VII, p. 47.)

Of the excessive and sometimes confusing prolixity of some of Loew's descriptions of species, and of his style of writing in general, I have spoken before, in Chapter I, p. 32.

About Loew's unpardonable and studied disregard of his contemporaries in dipterology, a disregard which proved very detrimental to science, I have introduced detailed accounts in the present "Record." A list of references to such accounts will be found in my "Introduction" (p. 19). At the same time I have observed that "the only secret motive of this idiosyncrasy that I can surmise is the consciousness of Loew that Nature had formed him for something better than describing Diptera." And indeed it often occurred to me to apply to Loew the well-known exclamation of Dr. Johnson to Hannah More: "Milton, Madam, was a genius that would cut a Colossus from a rock, but could not carve beads out of cherry-stones." A satisfactory explanation, psychological, or perhaps pathological, of this idiosyncrasy of Loew I do not pretend to attempt, but I shall always maintain ("Introduction," p. 21) "that, while condemning his injustice in the strongest terms, I do not mean to impugn his personal character as a man of truth and honor." There have been other great men who have shown similar anomalies of character. Galileo, one of the master minds in science, was jealous of some of his contemporaries, especially of the great Kepler, whose discoveries and attempts at a correspondence he treated with studied neglect (compare the biography of Galileo, in Arago, *Oeuvres*, Vol. III, p. 261-262; Paris and Leipzig, 1859).

In connection with the marked disregard of Loew towards his dipterological colleagues he was reproached, and, I think, with good reason, with not having been "an inspiring teacher," and not having had followers, like Schiner. His supercilious and repellent manner, which I experienced myself at the outset (compare above, p. 29), may have disheartened the zeal of many novices, and even discouraged prominent talents. Who knows but that Zeller may have been influenced by Loew when, after a brilliant and promising *début*, he suddenly abandoned dipterology? It belonged to a man like Zetterstedt, strong in his conviction of doing useful work, to continue it in his own way for fifteen years with commendable composure, in spite of Loew's persistent disdain (compare my Chap-

ter XVIII on Zetterstedt). Such behavior on the part of Loew is the more strange as it stands in complete contrast to his conduct as a school teacher, always "suggestive and inspiring . . . he acquired in a rare degree the affection of his scholars"; such are the words of his biographer, Dr. Krause, who was one of them! (Compare above, Chapter XV, p. 101.)

The peculiar circumstances connected with the publication of Loew's new series of descriptive work, entitled "Beschreibungen europäischer Dipteren," deserve a separate notice. Three volumes of this series have been published (in 1869, 1871, and 1873). In 1877, Loew wrote me that, among his unpublished manuscripts, there was a *fourth* volume almost finished ("welcher fast vollendet daliegt," see above, p. 97). Like the rest of these manuscripts, this volume has not been printed.

The first of the volumes of the "Beschreibungen" has *two* titlepages. On the first we read: "Systematische Beschreibung der bekannten europäischen zweiflügeligen Insecten, von Johann Wilhelm Meigen. — *Achter* Theil, oder *zweiter* Supplementband. Bearbeitet von Hermann Loew. — Halle, 1869." The *second* titlepage bears the inscription: "Beschreibungen europäischer Dipteren, von H. Loew. *Erster* Band. Halle, 1869."

The same arrangement is found, *mutatis mutandis*, in the *second* and in the *third* volumes: the *second* has, on the *first* titlepage: "*Neunter* Theil, oder *dritter* Supplementband"; and on the *second* titlepage: "*Zweiter* Band." The *third* volume has, on the *first* titlepage: "*Zehnter* Theil, oder *vierter* Supplementband"; and on the *second* titlepage: "*Dritter* Band."

As the *first* titlepage of the *first* volume bears the inscription "*Zweiter* Supplementband," and the *second* titlepage, which has for title "Beschreibungen europäischer Dipteren," is marked "*Erster* Band," it is evident that an intercalary "*Erster* Supplementband" was planned, *but not published*. About this intended intercalary work, Loew's Preface of Vol. I of the "Beschreibungen europäischer Dipteren" (1869) contains an explanation. After relating at great length how, since Meigen, the systematic distribution of the Diptera has undergone many changes, and how a large number of new genera have been described, Loew goes on: "This consideration seems to make it advisable, while undertaking a new working up of the European Diptera, to separate the systematic department (which is liable to become soon antiquated) from the descriptions of new species, which remain permanently indispensable. I intend therefore to publish on the System of European Diptera a separate work, illustrated by numerous figures, and to collect the descriptions of the species in a second work, of which the present publication is the first volume.

"The *first* work, for which rather voluminous preparatory manuscripts are ready ('zu welchem ziemlich umfangreiche Vorarbeiten fertig sind'), will contain in its Introduction the necessary information about the terminology, and the distribution of Diptera into families, and will also provide, in separate fascicles for each family, the subdivision of the families into genera. I shall not bind myself to any definite succession of the families, but shall publish those first the

systematic distribution of which is the clearest. I can hardly hope to complete the working up of all the families; but, should I be compelled to desist, it will be easy for my successor to continue the work, and to bring it to a conclusion. I shall be very glad to entrust single families, for the purpose of working them up, to dipterologists who may have chosen them for a specialty. And it also will give me great pleasure if some of the tried senior workers of the present generation, or others, of the junior class, of whom there is no lack at present, would consent to take a share in a work, the successful consummation of which transcends the power of one single man," etc.

In this pompous and diffuse style the Preface goes on for *fourteen pages*. It would be useless to conjecture whether Loew was sincere in anticipating the realization of this ideal programme for future dipterology, of which he would have been the centre. The hard fact remains that, within the next five years he published (as I have shown above) *three volumes of purely descriptive work*, and had prepared materials for a fourth volume. He would have gone on with the publication of these volumes if, in 1873 (the date of the last volume), he had not suddenly gone into politics, and had not since then, until his death in 1879, relegated dipterology into the background. That other work, promised with such insistence in the high-sounding Preface, never appeared, and an explanation of this delay was never given, nor even alluded to in the Prefaces of the second and third volumes. The double title of the first volume was nevertheless reproduced in the second and in the third, as the only reminder of the given promise!

I have introduced this digression as indispensable to my "Characterization of Loew," etc. Without it, many a dipterologist would perhaps have made use of the "Beschreibungen" before having read its Preface, and therefore without understanding the origin and the meaning of the double title of this publication.

Loew possessed a superabundance of working power and of tenacity at work. It is for the purpose of exemplifying these qualities that I have reproduced in *facsimile* his *tour de force* in calligraphy (in Chapter I), which represents, I may say, an almost superhuman effort. In dipterology, after trying different directions for his studies, he settled upon the *descriptive* department as the most suitable for his insatiable craving for work. If he had lived ten years longer, he would probably have continued publishing "Centuries" and volumes of "Beschreibungen europäischer Dipteren" as the most expedient form of publication for his talent. It is strange, nevertheless, that he never came openly to acknowledge this vocation, but always attempted to explain it away by apologies and equivocations of every kind.

Heretofore I have examined Loew's work objectively, from the purely critical point of view, and have shown in what his true merit consists. But I feel bound, before concluding my "Charac-

terization," also to place on record his moral merit by pointing out the very unfavorable and often most discouraging conditions under which his work was carried on. In the Introduction to my "Verzeichniss der entomologischen Schriften von Hermann Löw" (104, 1884) I have enumerated some of these obstacles: his life in small towns, far from museums and libraries; the difficulty in finding publishers for his writings; the illiberality of museums in not lending him materials for work, etc. Dr. Krause's "Obituary" of Loew (the translation of which is reproduced in my Chapter XV) mentions still other difficulties: Loew's varied and sometimes burdensome professional duties, his very limited financial resources, and, in many cases, the state of his health. In my Chapters XIV and XV are described the tragic circumstances of the decline of Loew's faculties, of his last illness, and of his death, all of which entitle him to a place, not only among the heroes, but also among the martyrs of science!

Not long before Loew's death, his collection of Diptera was purchased by the Royal Zoölogical Museum in Berlin for six thousand marks (about fifteen hundred dollars). Dr. Stein, one of the assistants of the Museum, was entrusted with the care of the collection. According to his estimate it contained about seventy-five hundred species, represented by sixty thousand specimens. A discussion arose whether to keep the collection apart, at least for an indefinite number of years, or to have it incorporated immediately within the general dipterological collection of the Museum. The best entomological authorities were of the opinion to keep the collection apart. Consulted by Dr. Stein, I gave my opinion in that same sense, in a letter containing an elaborate exposition of my reasons. The Director of the Museum at that time, Dr. Peters, was of a contrary opinion, which provoked a protest from many entomologists in Germany. An allusion to this episode in the *Deutsche Entomologische Zeitschrift* (1879, p. 419, footnote) was followed by the resignation of Dr. Peters as a member of the Society of which the "Zeitschrift" was the organ. The story of this "storm in a teacup" will be found in several articles of the *Deutsche Entomologische Zeitschrift* (for instance, 1879, p. 23: "Loew's Fliegensammlung"; 1880, in the Proceedings, p. 1-4).

APPENDIX TO CHAPTER XVI

Notice of the Dipterologist, Ferdinand Kowarz

Ferdinand Kowarz, as a dipterologist, has not received, it seems to me, the appreciation due to him. His publications are much less voluminous than those of Loew, and his rôle, during his

lifetime, was less conspicuous. Nevertheless, his merit is very great and his name should be inseparable from that of Loew. For this reason I place this unpretending notice about him next to my "Characterization of Loew as a Dipterologist."

Ferdinand Kowarz was born in Plan (Bohemia), February, 1838, and followed the career of an *employé* in the Imperial Austrian Telegraph Department. My high appreciation of his publications induced me to pay him a visit in May, 1878, at Asch, a little town in the northwest corner of Bohemia, where he was "Kaiserlich-Königlicher Telegraphen-Beamte" (Telegraph Official). I noticed the admirable order which prevailed in his collection of European Diptera, in which *all the families* were worked up with the greatest care. When visiting Vienna soon afterwards, I took occasion to call the attention of Kowarz's chief, Brunner von Wattenwyl, to this remarkable subordinate of his, and to his merit as a dipterologist. Brunner, himself a distinguished entomologist, and well known for his publications on *Orthoptera*, thanked me for this communication, and soon afterwards promoted Kowarz to a much more important post, as head of the Post and Telegraph Office ("K. K. Postverwalter") in the well-known watering-place Franzensbad in Bohemia, which position Kowarz occupied for more than twenty years, until his retirement with a pension. This promotion was an advantage for the material welfare of Kowarz (who was married and had children), although the duties connected with his new post left him less time for his entomological occupations.

Loew was in constant correspondence with Kowarz, and even had paid him a visit at Asch, but did not seem to appreciate him as he deserved, and spoke of him in a patronizing tone of superiority. Kowarz's first publication (*Verh. zool.-bot. Gesellsch.*, Wien, 1867), "Beschreibung sechs neuer Dipteren-Arten" (six pages with woodcuts), is that of a trained dipterologist. His successive papers on Loew's favorite family *Dolichopodidae* ("Dipterologische Notizen," 1868; *Chrysotus*, 1874; *Medeterus*, 1877; *Argyra* und *Leucostola*, 1878; the *Dolichopodidae* in his "Beiträge zu einem Verzeichnisse der Dipteren Böhmens," 1884; *Sympycnus*, 1889) show a treatment more careful than that of Loew, and afford an

Dr. Hermann Loew.

abundance of illustrations on plates, or in single figures, which are wanting in most of Loew's papers on the same subject. As a specimen, I would recommend for study the article on the difficult genus *Chrysotus* (*Verh. zool.-bot. Gesellsch.*, Wien, 1874) with its admirable plate of details.

A due tribute to Kowarz was rendered to him recently by Dr. Joh. Schnabl, in Warsaw (*apropos of Anthomyiae*), in the words: "Der ausgezeichnete Forscher in der Dipterologie, F. Kowarz, gehört zu den ersten, welche hier den Weg öffneten" (*Wiener Ent. Zeit.*, 1902, p. 135).

I have not the slightest doubt that future workers will more and more appreciate Kowarz, will admire the independence of his researches (under his difficult and straitened surroundings), and will consider him among the foremost dipterologists of the latter half of the nineteenth century.

Unfortunately, Kowarz, some time before his retirement, was compelled by his circumstances to part with his library and with his collection (the latter was purchased by Mr. G. H. Verrall, of Newmarket, England). Under these conditions it will be still more difficult for him to continue his work on Diptera.

XVII PHILIPP C. ZELLER AS A DIPTEROLOGIST

Philipp Christoph Zeller was born on April 9, 1808, in Steinheim, on the Murr (Württemberg), and died on March 27, 1883, in Grünhof, near Stettin. A short obituary notice with a portrait will be found in the *Stett. Ent. Zeit.*, 1883, p. 406; a longer, very sympathetic account of his life and distinguished services, as one of the greatest of lepidopterists, was published in the same periodical by the competent hand of the lepidopterist Frey in Zürich (*Stett. Ent. Zeit.*, 1883, p. 413-416). A very characteristic postscript is added to it by Dr. C. A. Dohrn, President of the Entomological Society in Stettin.

For a long time I had been meditating a paper on "Zeller as a Dipterologist," and now I avail myself of the opportunity for inserting a notice about him here.

In Zeller, the lepidopterist, the ideal of a perfect dipterologist was lost! He was, after Rob.-Desvoidy, one of the few who put in practice the notion that dipterology is the science, not of *pinned* Diptera only, but also of Diptera in life. His observations of the *motions* of Diptera during different functions of their life are, in many cases, more complete than those of any other dipterologist, and deserve more study and imitation than they have received.

The principal work of Zeller on Diptera is contained in two successive papers published in Oken's *Isis* of 1840 and 1842. The first paper is entitled, "Beitrag zur Kenntniss der Dipteren aus den Familien der Bombylier, Anthracier, u. Asiliden (*Isis*, 1840, p. 10-77, with a plate containing seventy-two figures). The second paper, "Dipterologische Beiträge" (*Isis*, 1842, p. 807-847), contains twenty separate articles on different dipterological subjects, among them an elaborate monographical essay on European *Tabanidae* (p. 812-825). In the short introductory paragraph of this second article Zeller promises a third "Beitrag," which, however, never appeared. The explanation of the plate, which refers to both papers, is placed at the end of the second.¹

The very remarkable publications of Zeller on Diptera in the years 1840 and 1842, buried as they are in the ponderous volumes of Oken's *Isis* of sixty years ago, have been very little consulted and appreciated by dipterologists. And yet any one who will take the trouble to study these pages will acknowledge that they contain perhaps the most delightful reading on Diptera in existence. The scope was a small one, the local fauna of a portion of Silesia, but Zeller has understood how to introduce into his work a real wealth of valuable observations of Diptera in

¹ Articles of minor importance on Diptera by Zeller will be found quoted in Hagen's "Bibliotheca," under Zeller, Nos. 8, 10, 16, 17, 36; they all appeared in the *Stett. Ent. Zeit.* In the same periodical, in 1872, there is a "List of Diptera from Bergün in Switzerland," by Zeller. Zeller told me that of his *first* paper in *Isis*, 1840, there were no separate copies in existence. He had none himself when I saw him, because he had made a present of his own copy to Zetterstedt. I had the good fortune to discover another separate copy which Oken, the editor of *Isis*, had sent to Rondani. I bought it at Hoepli's in Milan, together with some other books from Rondani's library. It bears the inscription (in pencil): "Al Ill. Camillo Rondani a Parma per l'Editore Prof. Oken." Of the *second* of Zeller's papers in *Isis* there are some rare separate copies scattered in libraries.

life, and of facts about geographical distribution derived from a large personal experience in different parts of Europe. The descriptive part of the work is treated with rare detail and accuracy; notices on the varieties of the species are sometimes almost too redundant (for instance, in the *Tabanidae*). Among the comparatively small number of genera and species that are treated of, many new characters and subdivisions are introduced, often with singular perspicacity. But the most attractive and novel department of the work is biological, consisting of accounts of the habits and of the demeanor of flies. To illustrate Zeller's manner, I shall reproduce a few instances. About *Bombylius* he says, p. 14:—

“The mode of flight of the true *Bombyliidae* is not unlike that of *Syrphus*. They hover for some time in the same place, jerk off suddenly, and then gradually sink, and thus approach the flowers they intend to suck. In the meantime they hold their hind legs horizontally, diverging; the front and middle pair almost hanging, and kept close together; the latter are often rubbed against each other, just as house-flies rub their front legs when cleaning themselves with them. The proboscis is horizontally stretched out. When near a flower, they generally touch it with the tips of their front legs, without, however, resting upon it; the wings vibrate without interruption, and support the body evidently more than the legs. The proboscis is inserted in the flower, whenever it is possible, vertically. For the night's rest they alight upon blades of grass or on flowers. When an enemy is approaching, they vibrate their wings, without being able to fly. Later in the evening, when the air is cooler, they become incapable even of this sign of life. The sound emitted by them during flight is, in some species, very loud, and resembles the piping of *Culex*, except that it is louder. The sound of *Bombylius minimus* is very low, and yet audible, when the ear is brought near it.”

Not content with such generalities, Zeller notices the peculiarities of behavior of almost every species: *Bombylius minimus*: “Flies near Berlin, Frankfort (on the Oder), and Glogau in pine forests, about open, sandy places in July. His favorite flower is *Thymus serpyllum*. He often alights on the sand. I have already spoken of his low piping tune.” Similar details about *Bombylius minor*, *posticus*, *concolor*, etc., eight species in all.

Concerning *Anthrax*, he says (p. 25):—

"These flies are very sober and but little subject to thirst, for they hardly ever visit flowers, even on hot days. It is during the present season that I have, for the first time, observed them to do so. Formerly, I had seen them sometimes alighting on flowers; but they always flew off without sucking. The flowers of *Sedum acre* (stone-crop) have a great attraction for them; round a spot covered with *Sedum* in blossom, on July 6, 1839, I have seen at the same time *Anthrax morio*, *afra*, *maura*, *varia*, and *mucida*, each represented by several specimens, and sucking the nectar, and this sight at once put an end to the doubts I had entertained about Meigen's assertion.¹ Still later (July 12) I found *A. cingulata* on the flowers of *Athamanta oreoselinum*. Before that, I had but once observed an *A. morio* sucking the dew accumulated in the hollow of a leaf. . . . Notwithstanding the large wings and their strong vibration, I have never heard them hum. A large *A. cingulata* I believe to have heard producing a slight sound."

In regard to the *Asilidae*, he says (p. 34): —

"So far as I know, nobody has as yet observed an analogy between them and the *Libellulæ*, especially *Libellula* (*Agrion*) *pulla*. This analogy, nevertheless, exists, and was first noticed by me in a species in which it is almost reduced to a minimum, *Asilus punctipennis*. It is much more apparent in the slender species of *Dioctria* and *Leptogaster*. The opportunities for comparing them are frequent, because they very often occur together in the same localities. As resting-places they select protruding ends of branches, sticks of wood, or blades of grass, from the end of which they have a good survey, and can easily start for flight. Between bushes both make the same adroit evolutions, and keep the legs ready for grasping. The legs are provided with spines of different size which enable them to have a good hold of their prey. The hind femora are longer than the front and middle ones. The prey being secured, the robber occupies the nearest resting-place and devours or sucks his booty with so much greed that, forgetful of his usual caution, he is easily caught."

It would be only justice to Zeller to have his hundred pages reprinted and made more accessible to the general public. Zeller had an excellent influence upon Loew, with whom he spent eight years as Professor in the Gymnasium in Meseritz (1860–1868), and to whom he generously communicated the admirably preserved collections of Diptera which he used to bring home from his walks

¹ Meigen, III, p. 142, says: "Sie fliegen aus bei heissem Sonnenscheine und besaugen die Blumen."

and journeys (for instance, in 1843, from Sicily). Loew often referred to him in his letters to me. In one of them (December 16, 1860) he says: "Zeller's companionship last autumn has renewed my interest in practical entomology; I brought home all kinds of stuff, which has at once produced some remarkable specimens. Nothing is more remunerative than to heap up in a cold room decayed wood, dry stems of plants, heads of Composites. Towards the spring, without any further trouble, the walls and windows of the room are covered with a multitude of flies, some of which are hardly ever found in the open air." In another letter (May 25, 1861) Loew says: "A great comfort for me in the bad weather and other miseries we are enduring at present is the intercourse with Zeller, who is far superior to me in entomological knowledge and from whom I learn a great deal. He is an able and sympathetic being," etc. Whether Loew was always just towards Zeller is another question. Zeller (1840) had been the predecessor of Loew in monographing the German *Tabanidae*, and his work, although limited to Silesia as to area, is excellent. Loew in his publication on the same subject (in 1858) treats Zeller's paper with his usual superciliousness; he adopts two of his new species of *Tabanus*, but takes no other notice of his work, except in the following off-hand passage: "Zeller, in Oken's *Isis*, 1842, has proposed the generic separation of the species of *Tabanus* with strongly pubescent eyes, under the name of *Therioplectes*; the acceptance of this proposition would indeed render the determination of the species easier, if the eyes of the glabrous-eyed species are absolutely glabrous, and not merely provided with a more or less sparse pubescence." Now I have shown (in my "Prodromus of the Tabanidae of the United States," Part I, 1875, p. 425; 43, 1875) that, with a slight modification of the definition of *Therioplectes*, it becomes an excellent generic concept, and especially acceptable in such a large genus as *Tabanus*.

In my paper, "Bibliographical and, in part, psychological Inquiry on the two Editions of the earliest Publication of H. Loew" (*Berl. Ent. Zeit.*, 1896, p. 279-284; 153, 1896), I have shown that the publication of the *second* edition (which appeared in *Isis*, 1840) was evidently prompted by no other motive than the emula-

tion of Loew, stimulated by the appearance of Zeller's excellent paper on Diptera in an earlier number of the same volume of *Isis*.

XVIII NOTICE OF JOHANN WILHELM ZETTERSTEDT (1785-1874)

Among the services rendered by Zeller to science, I must mention his relations with Zetterstedt. He made himself very useful to him by his communications, and Zetterstedt showed himself very grateful. The first volume (of the fourteen of the "Diptera Scandinaviae") opens with the following dedication:—

*Zellero, inter Lepidopterologos et Dipterologos
Europae magni, spectati et cari nominis viro.*

These words are printed in the middle of the page, in large type; and at the very bottom of the page, in small type, is added: *Operis auctor.*

In the Preface, p. xi, there is the following passage: "Insecta numerosa, quae Dr. Zeller, acutissimus et egregius Silesiae Lepidopterologus pariter ac Dipterologus liberalitate admiranda quotannis e Glogavia mihi largitus est, eo majoris pretii et acceptiora habeantur, quo non solum typica sunt descriptionis clar. Auctoris, verum etiam in individuis optime conservatis data et observationibus plenis ac confidentibus allegata fuerunt."

Observe the words *in individuis optime conservatis*. The habit of handling Microlepidoptera had accustomed Zeller to the most careful treatment of specimens. The "setting up" of his Diptera is perfect. At the same time he was a wonderfully active collector and used to bring home numberless specimens. In this respect he may be compared to the Swiss Jacob Boll and the American Morrison; but these were professional collectors.

Loew seems to have had no relations whatever with Zetterstedt. In his Vol. XII (1885), Zetterstedt gives a list of thirty-eight entomologists who had corresponded and exchanged specimens with him. Loew is not among them. Volume after volume of "Diptera Scandinaviae" were dedicated to entomological celebrities: Wahlberg, Stäger, Boheman, Dahlbom, Stenhammar, Bonsdorff, Macquart (1852), and it is only in the last volume (Vol.

XIV, 1860) that Zetterstedt, at the age of seventy-five, as if in forgiveness of the past, placed the dedication: "Viro amplissimo, Doctori H. Loew, Professori et Directori in Meseritz, Posnaniae, Plur. Societ. Litterar. Membro, Dipterologo nostri aevi celebrissimo, hunc sui operis tomum dicavit." In 1868, on the occasion of receiving Schiner's "Novara" work from the author, Zetterstedt sent him his photograph with the inscription: "J. W. Zetterstedt, natus Maji 1785 Lund, — die 25 Maji 1868 grata et amica mente salutatur celeberrimum et nostri aevi Dipterologum facile principem Dom. Doctorem J. Rud. Schiner." Schiner was delighted with this compliment as testified by him in his letter to me July 30, 1868.

An excellent article containing an impartial appreciation, and at the same time a criticism of Zetterstedt's "Diptera Scandinaviae" was published by Haliday (*Natural History Review*, July, 1855, p. 43-61). He says (p. 53): "The system of the 'Diptera Scandinaviae' is avowedly an artificial one; but viewed simply as such, it does not seem to fulfil its end well enough to compensate for the disregard of natural affinities. Zetterstedt, in grateful deference to the authority of his illustrious master in entomology [Fallén], retained in a great measure the arrangement and the nomenclature of Fallén's older System, although his unbiased judgment might have been better satisfied with the more recent Systems of Meigen or Macquart."

Zetterstedt, in his Preface (Vol. I, p. viii), has distinctly explained that he only aimed at an artificial arrangement, a fact which has been often overlooked by those who have made use of his work. I have especially insisted on this point in an article in the *Berl. Ent. Zeit.*, 1897, (p. 151, at bottom). Zetterstedt's defects notwithstanding, his life-work is a great one; as Haliday says (*loc. cit.*, p. 52): "His great work, a monument of untiring industry, erudition, and acute discrimination, is comprised in eleven 8vo volumes [this was written in 1855; the number increased to fourteen in 1860. — OSTEN SACKEN] which average above 400 pages each; and the publication, commenced at the author's own expense, and afterwards worthily sustained by the public purse, has extended over a period of ten years." [In

all, from Vol. I, 1842, to Vol. XIV, 1860, *eighteen* years had elapsed. The fourteen volumes contain 6609 pages, which gives an average of 472 pages per volume. — OSTEN SACKEN.]

XIX CAMILLO RONDANI AND HIS RELATIONS WITH LOEW

Camillo Rondani¹ was a descendant of a very old, noble family of Parma, which could boast of having produced men of distinction as early as the twelfth century. Among his ancestors was a painter of some renown, Francesco Maria Rondani (1490–1548). (“La noblesse est une dignité due à la présomption que nous ferons bien, parceque nos pères ont bien fait.” Joubert, *Pensées*, XVI, 54.)

I paid a visit to Rondani in Parma in the middle of May, 1873. He made a very favorable impression upon me, and struck me at once as being a perfect gentleman, and an experienced entomologist. Rondani was a contemporary of Loew in so far as he was born in 1808, Loew in 1807; both died in the same year, 1879. An active intercourse between them by letters, or by exchange of specimens (like that between Zeller and Zetterstedt), would have been equally useful to science in general, and to the studies of both in particular. The beginning of such an intercourse did indeed take place, but in the most unsatisfactory manner, and I am going to show that the fault was not on the side of Rondani.

Rondani's scientific work was not confined to systematic and descriptive entomology alone; he took a great interest in *economic entomology*, and acquired a measure of popularity by his numerous publications on noxious insects, and by his constant warfare against ignorance and superstition. (“His publications in this department are not only distinguished by the merit of learning, but also by their practical utility, and by the courage he showed in sustaining his own opinion against that which was current.” — M. Lessona, “Camillo Rondani.”)

The general esteem in which Rondani was held in Italy was mani-

¹ I am spelling here *Rondani* with an accent on the first syllable, because he complained to me that many people pronounced his name wrongly, *Rondani*.

fested after his death (September, 1879). A subscription was raised for the purpose of erecting a memorial in the shape of a marble bust, which was placed within the building of the University, and inaugurated in May, 1881, in presence of the Civic Authorities, of the Professors of the principal educational institutions in Parma, and of a large concourse of the public, including ladies ("la severa cerimonia era allietata dalla presenza di molte belle ed eleganti signore," said a local paper). A pamphlet was published: Camillo Rondani, Commemorazione, Parma, 1881; forty pages 8vo, with a plate, representing the bust. Two other biographical notices appeared in the same year, 1881, by Professor Michele Lessona, in Turin, and by Dr. A. del Prato in Parma. I reproduce these details, because it is not often that a dipterologist has been thus honored. It offers a striking contrast to the apparent indifference with which the news of Loew's, Schiner's, and other dipterologists' deaths have been received by the general public.

My acquaintance with Rondani's works is not very thorough, because I have never been engaged in the special study of European Diptera, and it would have been unjust to judge Rondani merely by his work on non-European faunas. But, from a general survey of his works, I obtained the impression that he had an excellent eye for affinities, as well as for the discovery of leading characters, and that, in this respect, his natural ability was decidedly superior to that of Loew.

In preparing my papers on "Chaetotaxy" (1881 and 1884) I had used the term *macrochaetae* without inquiring about its origin and its first appearance in literature. It was only several years later that I happened to look into one of Rondani's earlier publications (in the *Nuov. Ann. Sci. Nat.*, Bologna, 1855); "Descrizioni di due generi nuovi di insetti Ditteri." It treats of *Tachinidae*, and shows that Rondani had immediately become aware of the importance of their characteristic bristles, as affording distinctive characters which had not been used before ("caratteri distintivi i quali non erano prima considerati"). At the same time, he attempted to introduce a more precise nomenclature of them ("ho creduto necessario di modificare alcune espressioni poco precise che si veggono usate dai ditterologia"). He begins by pointing out the

importance of some large, characteristic bristles, which he calls *macrochaetae*, and adds: "A word which may be translated by the other word *crassisetæ*, and thus the ordinary word *setæ* will remain for bristles which are distinctly smaller ('piu esili'), and *pili* for those that are the most delicate ('che sono sottilissime')."'

Rondani then proceeds as follows:—

"Although the *macrochaetae* and other bristles, inserted on the head of different families of Diptera, have been used by dipterological writers as distinctive characters, it was done in a rather confused and not uniform manner, so that it was impossible to understand with certainty, either the exact place of their insertion, or their relative position, or their number or other peculiarities, and hence, in consequence of that very uncertainty, these characters have had but little value. The face of certain species was called *ciligera*, or *setis marginata*, or *vibrissata*; but these expressions did not indicate whether the face was provided with bristles both on the cheeks and on the edge of the facial groove, or either on the one or the other; nor did these expressions convey the notion whether the whole cheek or the facial groove was furnished with bristles, whether the latter were numerous or only few, whether they were independent of the frontal bristles or a mere continuation of them. Considering these uncertainties, it seemed to me important to put an end to them in future, and to correct this inconvenience by distinguishing the *macrochaetae* of the head according to the place of their insertion, after which it would be much easier to describe their other peculiarities."

Rondani then proceeds to establish the terminology of the *macrochaetae* of the head, and distinguishes them as *frontales*, *foveales*, *faciales*, and *orales*. In defining these terms, he further develops this terminology, and distinguishes among the *frontales* the *verticales*, *oculares*, *ocellares*, and *occipitales* (and so on, for the rest). Then he passes to the abdomen and distinguishes the *macrochaetae* as *superae* and *laterales*, *marginales*, and *intermediae*. He adds some remarks concerning a certain caution necessary in the making use of these characters.

It must be borne in mind that the paper of Rondani containing these suggestions had been published in 1845, and that it was two years later, in 1847, that Loew began the publication of his life-work as one may say, on *Asilidae*, in which he did not make any use of chaetotactic characters, as he should have done, and thus

left "a mine of characters" untouched (as I have expressed in my paper on "Chaetotaxy," in the *Trans. Ent. Soc.*, London, 1884, p. 515; **102, 1884**).

Rob.-Desvoidy had carefully defined and named the parts of the head, but his terminology of the bristles was vague (*cilia frontalia* for the bristles of the upper part of the head; *cilia apicalia* for the bristles of the abdomen). He used these terms in his last work (1863); in the "Myodaires" (1830) the description of the bristles is still more uncertain. Compare for instance the description of the genus *Athrycia* in the "Myodaires" (p. 111) with that of the same genus in the "Hist. Nat.," etc., Vol. I, p. 829 (1863).

I do not find that the term macrochaetae has been used by Macquart, Loew, Haliday, or Walker, and, so far as I can find out, it was Schiner, in his "Fauna," who popularized it for the first time. But it is nevertheless strange that, although Rondani made the first step towards a systematic Chaetotaxy, he did not persist in this undertaking, which would have been of much use to him in his later publications.

Apart from Chaetotaxy, Rob.-Desvoidy and Rondani had, in general, given more attention to the preliminary work indispensable for the proper classification and description of Diptera than Loew. It is unnecessary for me to insist upon this again, after the account I have given, in my "Chapters" concerning Loew, of the striking contrast between the magniloquent Preface of Loew's second edition of his "Posener Dipteren" (1840) and his complete failure in realizing the expectations raised by it; or of Loew's terror, when he was requested by the Smithsonian Institution to prepare a *General Introduction* on Diptera, to precede the intended series of "Monographs" (compare above in my Chapter VII: Remarks . . . concerning Loew's first volume of the Monographs of North American Diptera).

Among a lot of pamphlets (derived from Loew's library) which I purchased at a bookseller's in Leipzig, I found three *manuscript letters* of Rondani to Loew. The earliest of them is dated February 15, 1846, and begins with the words: "Literas tuas carissimas die 25 Januarii 1845 inscriptas, initio tantum mensis Februarii

1846 accepi," etc.¹ Rondani thanks for the communication of one of Loew's papers (it was "Dipterologische Beiträge," Vol. I, 1845, as results from the context), and complains of the difficulty he has in understanding his letter written in German: "Utinam tamen observationes tuas quae magni pretii sunt mihi, sermone pro me scriberes faciliore, nam germanicum mihi sat est peregrinum." Rondani, having been originally educated for the clerical profession, could write Latin fluently, and in that Loew probably was not able to follow him. Nevertheless I think that, with more good will on his part, this obstacle could have been overcome. In this first letter, Rondani answers Loew's observations, accepts some of them, and rejects others; he complains especially of Loew having been led into error by the short notice in *Isis* about his genera of *Cecidomyiae*; "Pauca in 'Iside' relata in opinionem erroneam te duxerunt," etc. In his second letter (March 15, 1846) Rondani informs Loew that he sends him his publications, excepting the "Memoriae" 1, 2, 3, and 5, of which he had no separate copies left (compare my Catalogue of Rondani's works); he replaces them by manuscript extracts enclosed in the same letter, with tracings of the figures.

In response to Rondani's communication of his Essays, Loew published a review of them, marked No. I, in the *Stett. Ent. Zeit.*, 1847, p. 146-147. The magisterial and patronizing style of this review was, in my opinion, somewhat unbecoming towards a contemporary as to age, and even, I may say, an equal as to merit, because the Loew of 1847 was by no means the same as the Loew of a later period. In this review, Loew says (p. 146): "The second paper (1840) contains a systematic subdivision of the *Tipularia gallicola* which the author splits ('spaltet') into *Lestremiinae* and *Cecidomyiinae*, a splitting (Loew on purpose uses this contemptuous expression 'Zersplitterung') in which the author will

¹ The cause of this delay of more than a year is not explained, but may have been due to the primitive conditions in which Rondani lived in Parma at that time. An instance of these conditions is the address of his letters to Loew: *Palatinat de Posnanie, dans la Grande Pologne, Posen*. Rondani must have used a map of the eighteenth century, because Posen was annexed to Prussia in 1793, and before that time that part of the country was called "La Grande Pologne" (Grosspolen), while the southwestern part of Poland, with Cracow, was called "La Petite Pologne" (Klein-Polen).

hardly find any followers." And nevertheless, the same Loew, better informed three years later (probably by Winnertz, with whom he corresponded at that time), but without any reference to his previous statement, said ("Dipterologische Beiträge," Vol. IV, p. 12, 1850): "He (Rondani) divides the Gallicolae into two families, *Lestremiinae* and *Cecidomyiinae*; although the formation of the names cannot be justified" [why?], "it must be acknowledged that he has correctly recognized the two principal groups of the family." More than that, in the "Monographs of North American Diptera," Vol. I, p. 7 (1862), Loew adopted, *without the slightest acknowledgment to Rondani*, exactly the same subdivision, based upon the very characters which Rondani had discovered more than twenty years before, and which, at that time, Loew had rejected! The same characters were reproduced by Winnertz, in his paper on *Lestremiinae* (1869).

The arbitrary substitution by Loew of the name *Anaretiinae* (for *Lestremiinae* Rond.) was unfortunate. In the *Stett. Ent. Zeit.*, 1845, p. 395-398, Loew had described *Anarete albipennis*, sp. n. (Germany), and decided, after a long and verbose discussion, that, on account of its venation, it should be considered as related to the *Lestremiinae*, etc. In this Loew was right, because *his Anarete* (as was proved afterwards) was not the *Anarete* of Haliday at all, but a *Cecidomyia*. Haliday's *Anarete* is a *Scatopse*, as Curtis had recognized it long ago ("Guide to an arrangement of British Insects," 2d ed., 1837); Winnertz and Schiner, perhaps independently of Curtis, put it in the same position, where it undoubtedly belongs (as I have shown in *Berl. Ent. Zeit.*, 1892, p. 451).

The concluding passage of Loew's review (1847) is evidently meant for an encouragement for Rondani. Loew promises him soon to publish a critical review of several of *his own* (Loew's) early publications, and adds: "It is only just to apply the same rule to one's self as to others; truth must prevail above everything, and all disguise or palliation of error should be avoided." This was certainly an excellent declaration of principle; unfortunately the promised article No. II never appeared, although, in the previous publications of Loew, on the very subjects in which he had exercised his criticism against Rondani (the relationships of *Ceci-*

domyia, *Psychoda*, *Phlebotomus*, *Anarete*, etc.), there are passages which greatly need emendation, and in which Rondani's critic betrays a very superficial knowledge of the subjects in question, and adopts very hasty conclusions (compare, for instance, the passages in the *Stett. Ent. Zeit.*, 1844, p. 118, and p. 121-122; also in the "Dipterologische Beiträge," Vol. I, p. 10-11, 1845). These passages, the publication of which Loew must have afterwards regretted, made him, at a later period of his career, very prudent in the proposal of innovations in the system (compare the passage in "Die Dipteren-Fauna Südafrika's," Preface, p. v, 1860). Concerning the behavior of Loew in regard to other portions of Rondani's work on *Cecidomyiæ*, I prefer not to say anything, as there are other dipterologists, more competent than I, to pass judgment upon them.

In 1858, Loew published a severe, but this time better justified, critique of Rondani's "Dipterologiae Italicae prodromus," Vol. I (1856), in the *Berl. Ent. Zeit.*, 1858, p. 338-340.

The first volume of the "Prodromus" contains dichotomic tables of the families and genera of Italian Diptera, published in advance of the intended work upon them. The number of genera (according to Loew's statement, *loc. cit.*, p. 338) is 587; among them a considerable number are new ones, the names of which are introduced without any other description but that contained in the data of the dichotomic tables, connected with the name of the typical, often as yet undescribed, species. Loew was right in calling this premature publication a failure,¹ calculated to impede, rather than to advance, the future progress of dipterology. Loew enumerates a large number of errors, misspelling of names and misprints. In my "Studies on Tipulidae," Part. II, p. 230, I gave a critical review of Rondani's treatment of this family in the "Prodromus."

To judge of the contents of the other volumes of the "Prodromus" is a task beyond my power and foreign to the subject of the present chapter, to which I now return.

¹ In making this reproach to Rondani in 1858, Loew seems to forget that, only a few years before, in 1850, he had published a similar premature dichotomic table of the new genera of Amber Diptera, without sufficient definitions. In my Chapter IX I have reproduced the very weak apologies he offered for that table.

From a letter to me (Guben, March 5, 1869), I translate the following passage, which proves that Loew's correspondence with Parna, at that time, was not very active: "Guben, March 5, 1869. Rondani, with whom I exchange *separata* from time to time, continues his work busily in his well-known manner. Lately, he launched his 'Review of Italian *Ortalidae*,' and is now occupied with the *Trypetidae*, for which, according to Haliday's communication, he seems to have rich material. *Polyergus* Haliday delights me from time to time by his detailed and often instructive letters," etc.

When, in 1873, Loew's monographic paper on *Ortalidae* appeared, ("Monographs," etc., Vol. III), I ventured to ask him why, after reviewing in this volume the work of all the earlier authors, even Rob.-Desvoidy's, concerning this family, he had not included Rondani among them. Rondani's papers on *Ortalidae* had appeared in the *Bull. Soc. Ent. Ital.* two or three years earlier, between 1869 and 1871. Loew's answer to me is contained in the following passage, which I do not quite understand, and, therefore, do not attempt to translate:—

"Dass Rondani's Arbeit über die *Ortalidae* in meiner Arbeit nicht berücksichtigt ist, hat seinen Grund lediglich darin, dass meine Arbeit lange vor deren Erscheinen vollendet war, und auch bei der letzten Redaction derselben sich diese noch nicht in meinem Besitze befand. Ich habe mich bereits in der 1868 erschienenen Revision der europäischen *Ortalidae* auf meine jetzt erst im Druck erschienene Arbeit und deren systematische Resultate, die da zum Theil mitgeteilt sind, bezogen, und Rondani bezieht sich in seiner Arbeit wieder auf die Mitteilungen. So ist es wohl am besten, meine Arbeit behält den Charakter ihres früheren Abschlusses bei, wie ich ihr denselben gelassen habe."

Loew knew that the publication of the first part of Rondani's *Ortalidae* had taken place in March, 1869. The last part of it appeared in 1871. Loew might have easily obtained this last part if he had been seriously interested in its contents. Why Loew deliberately ignored Rondani's work on *Ortalidae*, I leave for future specialists of this family to unravel.

In a letter of November 17 (finished November 22), 1872, addressed to me while I was in Rome, Loew wrote: "I envy you your residence in Italy. If it were possible for me to have about two months of freedom from work, I would have used them for an

excursion to Italy, in order to anticipate the advent of spring. As there is not the slightest prospect of it, I intend, if feasible, to send Kowarz on a collecting tour to Italy, as the more intimate knowledge of the Italian fauna of Diptera is indispensable to me. It may be expedient to arrange it so that this excursion should include a visit to Parma, and procure me, in that way, as much light as possible on Rondani's nomenclature of the species. Could you give me some advice about the best time for undertaking such a journey, and some indications concerning the best localities," etc. This great desire for more information upon the Italian fauna stands in strange contrast with the marked repugnance that Loew always showed against any correspondence with Rondani. Loew wrote me this while he was keeping me for years in suspense about the manuscript of the third volume of the "Monographs." I must have felt rather tired of such proceedings, because I do not find in my correspondence that I made any reply to this Italian proposal.

During the spring of 1873, Loew asked me to procure him in Italy the volumes of Rondani's "Prodromus," as, up to that time, he had used a borrowed copy. I sent him the work as a present, and received his thanks (May 9, 1873). I take note of the following passage: "This work, with all its imperfections, is not without some positive merit ('ist nicht ohne bestimmte Vorzüge'), which makes it indispensable, so that I am glad to have it." This rather late acquisition must have offered Loew an opportunity for a more serious study of the "Prodromus." I find that the very next year, in his paper on *Azelia* (1874, p. 9), he pays a compliment to Rondani on his progress between Vol. I and Vol. VI of the "Prodromus." In Vol. I Rondani had united *Azelia* and *Homalomyia* under the name of *Myantha*, gen. n. In Vol. VI he gave up *Myantha*, and restored the other two genera. Loew says: "He has characterized them by plastic characters that are better chosen than those previously used by authors in defining the genera of *Anthomyiidae*, so that his work on this family, in this instance and in others, constitutes a true progress in its classification. The characterization of *Azelia* by Rondani is, on the whole, sufficient, although not quite applicable to female specimens. The characters used in the definition of the five species enumerated by him are useful ('brauchbar'),

but scanty in number, so that some of the species cannot be interpreted with absolute certainty, but only with great probability ('so dass einige der Arten nicht mit absoluter Gewissheit, sondern nur mit grosser Wahrscheinlichkeit gedeutet werden können'). It would be difficult to bestow praise more sparingly, and with more caution. And yet, one feels like exclaiming: "better late than never." Both Loew and Rondani died five years later, in 1879.

Rondani for a time had been in correspondence with Macquart. Among the books from his library which I purchased from the bookseller Hoepli, in Milan, there was a manuscript of sixteen quarto pages in Macquart's handwriting. It is entitled: "Suite de l'examen des Tachinaires envoyés par Mr. Rondani à Mr. Macquart en 1844." It begins with No. 21 and ends with No. 90, each number representing a species about the name of which Macquart gives his opinion.

Haliday, during his residence in Lucca (Italy), must have been very useful to Rondani. As an instance of his solicitude, I will mention that he provided the latter with a set of his own (Haliday's) publications, with manuscript notes and corrections. Of several papers of which Haliday had no separate copies to give away, he supplied copies in his own handwriting. The thirty-three pages of the article in the *Entomological Magazine*, 1832-1833, p. 147-180, "Catalogue of Diptera occurring about Holywood, in Devonshire," Haliday translated for Rondani into Latin. I found this manuscript among my purchase from the bookseller Hoepli, in Milan. It is a model of calligraphy.

Opinions and criticisms about Rondani's work on *Acalyptрата* will be found in Dr. Th. Becker's publications in the *Berl. Ent. Zeit.*, 1894, p. 81 (*Scatomyzidae*), 1895, p. 174 (*Sapromyzidae*), *ibid.*, p. 322 (*Lonchaea*).

XX NOTES ON CHAETOTAXY, COMPLEMENTARY TO MY PREVIOUS PUBLICATIONS ON THE SAME SUBJECT

The study of Diptera, previous to the introduction of a regular system of Chaetotaxy, may serve as a remarkable instance of the *routine* (irrational old habit) which, with rare exceptions, prevails in all human affairs.

At present, in questions of classification, we use chaetotactic characters as sure, indispensable, and easily applicable indexes of affinities. But, before 1880, the want of appreciation of these characters brought about hypothetic relationships established merely upon superficial resemblances, or upon characters of but secondary importance. The result was that groupings and arrangements were adopted which now appear to us simply preposterous.

Beginning with myself, I must acknowledge that for more than twenty-five years, I had been studying Diptera without feeling any necessity for an improvement in the method of describing their bristles and hairs. As late as 1877 I described *Rhaphiomydas* as a Mydaid, a notion which, three years later, appeared to me absurd.

The explanation of this apparent obtuseness on my part, however, is rather easy. During my residence in the United States, although I was collecting all Diptera, I studied and worked up monographically the *Tipulidae*, *Cecidomyiidae*, and *Tabanidae*, families which are not provided with macrochaetae. It was only after my return to Europe that I undertook the study of Asiatic, African, and Australian Diptera, and thus came into contact with forms of all families. In 1880, while at work on my "Enumeration of the Diptera of the Malay Archipelago," which appeared in February, 1881, although I had for the first time to describe a considerable number of *Diptera Cyclorrhapha* abounding in macrochaetae, I still followed the old routine. Thus, in describing the Orthalid genus *Anguitula* (*loc. cit.*, p. 482, line 6 from bottom) I said: "No bristles at all on the head; the usual erect bristles on the vertex are replaced by a few hairs, visible only under a strong lens," etc. But when, later, I was preparing my "Supplement of the Enumeration," and had to describe a very bristly fly, a Dexid (*Urodexia*, gen. n.), it occurred to me that the distribution of such conspicuous bristles must be subject to some rule. I took my time, worked out my "System of Chaetotaxy," and published my first paper upon it in 1881; my "Supplement of the Enumeration" appeared in February, 1882. The thoracic bristles of *Urodexia* are described in it chaetotactically as follows: "Four dorso-central rows; the outer ones of five bristles each; two intrahumeral bris-

gles, one humeral; about five mesopleural, two sternopleural, the posterior very long; a tuft of hypopleural bristles."

I need not enter here into details of the success of this new method of description, of which an anonymous contemporary said (*Entom. Nachr.*, 1883, p. 219): "It deserves the greatest publicity, because it opens entirely new prospects for the determination of Diptera, and will probably be soon universally adopted."

That, with sufficient material, it is possible to give a chaetotactic description of even very small Diptera, I have shown in a later paper, "On the Chaetotaxy of *Cucoxenus indagator* Lw." (125, 1891).

It might have been expected that my papers on Chaetotaxy would have been most welcome to Professor Brauer, when he began, soon afterwards, to work at his "Muscaria Schizometopa," the first instalment of which appeared in 1889. But, exasperated as he was against me on account of my criticism of his paper on *Notacantha* (89, 1882), he purposely abstained (of course, to his own detriment) from using chaetotactic characters. In his Preface he said: "Who once begins to subdivide the genera *Tachina* and *Dexia*, will be compelled to subdivide so far, that, for the definition of species he will be reduced to use characters of the lowest degree, as, for instance, the macrochaetae, etc."¹ In the same Preface, when speaking of macrochaetae, he sedulously avoided even the use of my name in connection with them, and mentioned only Rondani, Macquart, Loew, and Schiner. In his choleric outburst against my new distribution of Diptera which I published later (128, 1891), he denounced Chaetotaxy in the most unmeasured terms. This obstinacy could not last forever. Brauer soon became aware that he could not go on without using chaetotactic characters, especially those based on the position of thoracic macrochaetae. Protests and reclamations arose from different sides, and compelled him to relent. In his "Muscaria Schizometopa," Vol. IV, p. 3, at bottom (1895), he quoted a passage of my Chaetotaxy (*Trans. Ent. Soc.*, London, 1884, p. 513), in which

¹ "Wer anfängt die Gattung *Tachina* oder *Dexia* zu zerlegen, der ist gezwungen, so lange zu schalten, bis für die Arten nur ganz untergeordnete Charactere übrig bleiben, z. B. die Macrochaetenstellung."

I pointed out that "most of the *Calyptrata*, except the *Anthomyidae*, have a tuft or row of bristles on the hypopleura, which is destitute of them in the other families of Diptera." Brauer was candid enough to acknowledge that this family-character had been discovered by Osten-Sacken.¹ In his later publications Brauer became so far converted to Chaetotaxy as to make use of its terminology, which he had spurned before. In a notice about the genus *Aulacocephala* Gerst. in the *Anzeiger Akad. Wiss.* Wien, 1899, p. 238, he uses the terms: sternopleuralborsten and hypopleuralborsten. Nevertheless, in his "Obituary of Mik" (*Wiener Ent. Zeit.*, 1901, p. 4) Brauer could not resist the temptation of again venting his spite against me in the following sentence, which stands there *à propos de bottes*: "Die Lehre von der Borstenstellung wurde 1873, — lange bevor ein anderer den gelehrten Namen 'Chaetotaxie' dafür erfand — von Mik und Loew geübt, und erst in neuerer Zeit in ihrer Bedeutung gewürdigt (Girschner)." ²

Brauer would have been nearer the truth if he had named Rondani as the originator of the idea of Chaetotaxy. As to Brauer's statement about Girschner, although uttered *ab irato*, it is, nevertheless, perfectly correct: it was the merit of Mr. E. Girschner to give to Chaetotaxy a much greater development and application than it had had before, and to treat it as a *sine qua non* of descriptive dipterology. His enviable talent for drawing enabled him to illustrate his papers by diagrams more eloquent than any descriptions.

About the origin of the term *macrochaetae*, due to Rondani (1845), its further history, and about the use of characters borrowed from the position of bristles by Rondani and Rob.-Desvoidy, I have given an account in my Chapter XIX, "Rondani and Loew."

¹ The discovery of this important character has been, inadvertently, attributed to Mr. E. Girschner by Dr. Joh. Schnabl in the *Wiener Ent. Zeit.*, 1902, p. 128. Upon my friendly remonstrance (*in literis*) the very next number of the same periodical (on p. 184) published the necessary correction. Mr. D. W. Coquillett reproduced the same error in his remarkable paper, "A Systematic Arrangement of the Families of the Diptera" (*Proc. U. S. Nat. Mus.*, Vol. XXIII, p. 65, 1901).

² Translation: "The doctrine about the position of bristles has been used in 1873 by Mik and Loew, long before another invented the learned name 'Chaetotaxy' for it, and it is only recently that its importance has been fully recognized (Girschner)."

Rondani (*Nuov. Ann. Sci. Nat.*, Bologna, 1845), when introducing the name macrochaetae, did not exactly define the concept, but explained the name as follows: "A word which may be translated by another one: *crassisetae*, and thus the ordinary term *setae* will remain for bristles distinctly more slender ('piu esili'), and the term *pili* for those which are the most delicate ('sottilissime')."

In my "Essay" (*Trans. Ent. Soc.*, London, 1884, p. 501, at top), I went a step farther in describing macrochaetae "as *organs of orientation*, not unlike the whiskers of a cat." Such sensitive organs, it may be readily assumed, must be connected with the nervous system, the nerves penetrating to them through the chitinous integument of the fly, and this penetration is indicated by the *scar* which macrochaetae leave behind them when rubbed off. Anatomists may, by and by, go beyond these hints, and add many new facts to our knowledge about the rôle of macrochaetae as organs of orientation.

A still unrealized desideratum is a Chaetotaxy of the *legs* of Diptera, which would enable describers to define their position with exactness, as well as brevity.

The first attempt of this kind was made by J. Mik. In his "Dipterologische Untersuchungen," Wien, 1878, p. 3, footnote, he says: "On the legs I distinguish a front- and a hind-side; an upper- and an under-side. When we imagine the leg stretched out horizontally and perpendicularly to the longitudinal axis of the body, the front-side is that which is turned towards the head, and the hind-side that turned towards the end of the body; the upper- and under-side, in such a case, are self-understood."

I accept this proposal as expedient, but I would improve it by the addition of appropriate terms for intermediate positions. Assuming any femur or a tibia to have the shape of a more or less irregular cylinder, we may define the position of a bristle, or of a row of them, as being inserted on its upper (superior)-, or under (inferior)- side, or on its front (anterior)- or hind (posterior)- side. A bristle, or a row of them, may be situated on the line (edge) between the superior and anterior side; or between the anterior and posterior side, etc. In such a case I would describe them as inserted on the supra-anterior, the supra-posterior, the ante-inferior and post-inferior side.

For brevity's sake a mode of notation might be adopted, analogous to that used for the points of the compass: S (supra), I (infra), A (ante), P (post); and in combination: S. A. (supra-anterior), S. P. (supra-posterior), A. I. (ante-inferior), and P. I. (post-inferior).

For still closer definitions of position, the combinations S.S.P., or S.P.P. etc., might be used, just as for the points of the compass.

It is evident that the minuteness of such characters, their multiplicity, their unsteadiness, as well as the difficulty, in some cases, of obtaining specimens in a

sufficient state of preservation, are some of the obstacles to be met with in the study of the Chaetotaxy of the legs of Diptera. Nevertheless, difficulty is not an excuse for the complete neglect of useful characters, and repeated experience alone is apt to evolve the best method of utilizing them.

XXI AN ACCOUNT OF THE BREACH BETWEEN LOEW AND SCHINER

The cause of the rupture which occurred in 1858 between the two eminent dipterologists was the intended publication by the Austrian State Printing-Office of an *édition de luxe* of the European Trypetae ("Die europäischen Bohrfliegen") which appeared in Vienna in 1862 with the name of H. Loew as author, although, according to the original plan, it was to be the joint work of Loew and Schiner. Schiner published a separate paper: "Die österreichischen Trypeten" (*Verh. zool.-bot. Gesellsch.*, Wien, 1858), in the preface of which he explained that, by an adroit manipulation at headquarters, Loew had compelled him (Schiner) to withdraw from any co-operation with him in the publication of "Die europäischen Bohrfliegen." As this estrangement between two leading dipterologists has had some importance in the history of dipterology, I have in this chapter given a detailed account of its origin and progress. In my "Introduction" (p. 20) there is an allusion to this very regrettable dissension.

In one of his letters to me (March 17, 1857) Loew, in describing his occupations in general, had mentioned the intended publication in the following terms: —

"Finally I have compiled materials for a large work on *Trypetae*, and worked out a portion of the text. It will be printed in the Imper. and Roy. State Printing-Office in Vienna, with magnificent photographic figures of the wings; and will, after a rather long lapse of time, offer again to the public an entomological publication in *Atlas-folio*. Do not laugh at this enormous 'magnification' of the little Trypetae! The idea was not mine, but that of the State Printing-Office, which intended to issue samples of photographic plates ('Muster von photographischen Abbildungen'), and asked me to procure the necessary specimens ('Objecte') and to prepare the text. The first part embraces European *Trypetae*. The second will contain all the exotic species we can get hold of. In order to prepare the work for the second part, and, in general, to afford a basis for the determination of

the *Trypetidae*, figures of the types of Wiedemann and other authors will be made; most of the drawings are finished, and several already lithographed; the editing and the composition of the text has been undertaken by my friend Schiner in Vienna. It would afford me great pleasure to receive from you North American species for the great work on *Trypetae*," etc.

When Schiner's accusation appeared, Loew called my attention to it in a long letter of March, 1859 (the letter bears several successive dates), in which he gave me the following explanation of the occurrence:—

"In the last quarterly issue of the Vienna Zool.-bot. Gesellschaft you will find a very malignant attack of Schiner against me, in which he accuses me of several proceedings of such a nature that any decent man would henceforth feel disinclined to have anything to do with me. I should be ashamed if even one of his defamatory accusations was true. It is very distasteful to me to take part in such personal squabbles, and doubly distasteful when they are carried on with such dishonorable weapons as in the present case. I hope, however, to get out of this difficulty without any harm to myself ('Hoffentlich werde ich es, ohne mir etwas zu vergeben, auch hier ungehen können'). First of all, I have written to the head-quarters of the Society and drawn their attention to the fact that it is not customary to accept for publication in the 'Transactions' of a Society such a kind of invective without first hearing the attacked party; and next, by sending them Dr. Schiner's letters to me, I have proved that his statements are not only entirely unfounded, but that they are deliberate perversions of the truth and *lies* ('Lügen'). Finally, I have required that a publication from head-quarters should, in their own name, declare in some appropriate place in their Proceedings, that the produced letters of Schiner have convinced them of the unverity of his statements, and at the same time that they should express their decided disapproval of the untimely publication of his accusations in their 'Transactions.' I shall at first wait for an answer. Should the required satisfaction in a becoming form be refused, then I shall, of course, feel compelled to obtain it myself, and I shall certainly do it in the most effective manner ('eclatantester Weise'). You see that, even in peaceful entomology, it is often difficult to avoid strife, especially when one has to do with people who are less concerned about the substance of the question than about the satisfaction of petty personal vanities. I do my best not to be disgusted with the whole matter by such foulness ('Stänkereien')."

Loew's protest in 1859 does not seem to have had any further consequences. The only trace of it I find in literature is that Loew, from that time, conceived an intense rancor against Schiner,

and gave vent to it in a series of virulent attacks upon him personally, and against his publications. (Compare *Wien. Ent. Monats.*, 1864, p. 122-123, footnote; *ibid.*, p. 249-255; *Berl. Ent. Zeit.*, 1864, p. 334-346; *ibid.*, 1865, p. 356.) The first of these attacks especially is a curious exhibition of impotent rage exhaled in expressions and images in the worst possible taste. This seems to have been the whole amount of satisfaction which Loew received after his threat to obtain it in "eclatantester Weise," and after five years of excitement! Schiner's rejoinders are comparatively calm and dignified (*Wiener Ent. Zeit.*, 1864, p. 296-301; *Berl. Ent. Zeit.*, 1865, p. 125).

The malignant and most unfair criticism of the first instalments of Schiner's "Fauna Austriaca" in Gerstaecker's Bericht . . . Jahre 1859 und 1860, p. 277, was likewise written by Loew; but the first part of it, on p. 276, is the work of Gerstaecker himself, and is quite fair.

For Loew, after some time, the charm of novelty attaching to his Atlas-folio Trypeta-work had been exhausted. He, whose imagination in 1857 revelled in the preparation of a second volume, while the first had not yet been written, wrote me *four years later* (June 3, 1861) that the work was progressing slowly; "I have, unfortunately, only *four* sheets ready" ("Leider habe ich nur vier Bogen fertig"). The first volume was to contain *twenty-seven* sheets.

In the Preface of the Atlas-folio on *Trypetae* which is dated June 24, 1862, Loew says (line 9 from top): "Die gegenwärtige im December 1860 abgeschlossene Arbeit," etc. This statement is in glaring contradiction to the above-quoted passage in Loew's letter to me dated June 3, 1861 (that is, nearly six months later): "I have, unfortunately, only four sheets ready." The last letter deserves, of course, more credence than the Preface of June, 1862, and this contradiction shows to what straits Loew had been reduced by this unfortunate "Atlas-folio" business!

Between 1861 and 1862 Loew seems to have made a resolute effort, for, in a letter dated November 21-December 18, 1862, he wrote: "The tiresome Trypeta-work ('das leidige Trypeta-Werk') in Vienna is finally finished." Under such circumstances the publication of a second volume was out of question.

In justice to Schiner, I shall reproduce now *his* account of Loew's

behavior towards him in this instance. This account, which provoked Loew so much, appeared in the Preface of the paper "Die österreichischen Trypeten," published by Schiner in the *Verh. zool.-bot. Gesellsch.*, Wien, 1858, p. 635. Schiner says: —

"The present instalment treats of the Acalyptrate Genus *Trypeta*, an interesting group of the great family *Muscidae*. According to my former announcement, the *Dolichopodidae* should have followed now; this early publication of the *Trypetidae* is due to a particular cause, of which I shall give a brief account here.

"The experiments made at the Imperial State Printing-Office, with the co-operation of Dr. Ernst Heeger, for the purpose of reproducing by photography objects of natural history, magnified by the microscope, had been so promising and successful that Hofrath von Auer, Director of this department, conceived the project of publishing a larger work to show the practical advantages of microtypical illustrations of objects of natural history. It was upon my suggestion that the choice fell upon a Monograph of *Trypetae*, the wings of which offered the greatest variety of patterns for microscopical reproduction. Herr von Auer entrusted me with the compilation of the text, and acceded to my further proposition to invite for co-operation the well-known monographer of *Trypetae*, Director Loew in Meseritz. Loew accepted my invitation with pleasure and showed at first a great deal of interest in the undertaking.

"In the meantime the *Trypeta*-wings, furnished by me, and from my collection, had been reproduced by the microtypical process, and during the meeting of naturalists in 1856 the undertaking was so far advanced that it became possible to produce before this Assembly some prints of the magnified wings, as well as the general plan of the publication.

"Director Loew, who was present at the meeting, showed this time less interest in the undertaking, and did not even attempt to have an interview with Herr von Auer in regard to the matter. This made me the more eager to press the business, and to stimulate my co-operator for the cause. I insisted especially upon a division of the work between us; I would have undertaken the faunistic department, that is, all that concerns local occurrence and life-habits of the European and extra-European *Trypetae* and their synonymy, — in a word, the compilation of the materials. Loew would have had to provide the final systematic distribution and the drawing up of diagnoses and of descriptions.

"At the beginning of 1857 I had accomplished my share of the task of preparing the text, and had sent my finished manuscript to Loew. He gave me the assurance that at least a portion of the text, ready for the press, would be provided by him by the time when at least *three* plates with

the figures of the wings would have been finished. At the end of 1857 this condition had been fulfilled, the finished plates had received the approbation of Loew, and moreover sixty-three single wings, all from my collection, had been photographed. Notwithstanding this, Loew had, up to that time, not sent a single line of the text. Constant urging on my part had no other result than to call forth repeated apologies from Loew, who, in the meantime, without informing me of it, had applied directly to the Imperial Printing-Office, proposing an arrangement of the plates different from that which had been agreed upon between us, and, under the pretext that under my supervision the undertaking was not progressing, — had claimed the right of directing it alone from Meseritz!

“Under such circumstances I had to choose between two alternatives, either to insist upon the rights conferred upon me by the confidence of Hofrath von Auer, and to dismiss a co-operator whom I had myself proposed, and who now attempted to oust me; or to abandon the whole business to Loew alone, and to withdraw from it in quiet resignation. I preferred the latter course and imparted my decision to Mr. Hackel, who had been sent to me on this business. I wrote at the same time to Dr. Loew, telling him that I reserved to myself the right to make known to my colleagues the cause which had induced me to give up my co-operation in the Monograph, which had already been publicly announced as our joint work at home and abroad. To Herr v. Auer, who would have certainly sustained my claims in this case, I explained my resolution with the assurance that after my withdrawal I left the undertaking in the hands of Loew, which were the best possible for it.”

Schiner goes on explaining that after his manuscript had been returned to him by Loew, he decided to publish it in the form of an anticipated instalment of his “*Diptera Austriaca*.” He fore-shadows at the same time a future work on Diptera which would be, for the dipterological fauna of Austria, what Redtenbacher’s work was for Coleoptera. He fulfilled this promise a few years later by the publication of his “*Fauna Austriaca. Diptera*” (1862–1864).

This straightforward, detailed, and evidently truthful account of the whole transaction, given by Schiner, must have taken Loew by surprise. He probably had expected that his good-natured and modest friend would keep silent. The frenzied but impotent wrath which he gave vent to in his letter to me (and probably to other correspondents) was a sudden explosion. But that was all! He never attempted publicly to vindicate himself against Schiner’s

accusation, and took his revenge in vowing against him an intense hatred, the trace of which in literature will remain a blot on his memory!

Schiner took Loew's conduct deeply, perhaps too much, to heart, as it seems to have affected him during the rest of his life. In 1868, he sold his collection to the Imperial Museum in Vienna. He wrote me (May 7, 1868) (translation): —

“What I shall begin now, I do not know. I have devoted twenty years of my life to the dear Diptera, and sacrificed for their sake everything an honest inquirer can sacrifice, and have reaped the greatest ingratitude. Will it not be better for me to remain silent in future, and to enjoy nature and its secrets merely *sub camerâ*? I am weary, not from the work of research, but weary of all the struggle, and the attacks that after all had no other aim than whether A or B should have the supremacy! Had not some good and faithful friends, like Winnertz, Haliday, and yourself, my dear Sir, sent me from time to time an encouraging message, I should have desisted long ago from any further publication.”

Loew wrote me (December 22, 1868): “Schiner sold his collection to the Imperial Museum of Vienna for 2000 florins. At present he is said to be occupied with spiders.” Schiner died on July 7, 1873. Loew wrote to me (July 17, 1873) in the following terms: “You may have heard that Schiner died after a prolonged state of great suffering. I regret this new loss of a worker in dipterology which is so much in need of such, and I deeply deplore that so few new recruits are forthcoming.” There is a story of a German professor of great ability, who published a crushing criticism of a contemporary publication. It so happened that the author of it *died* very soon afterwards. Upon which the Professor observed, with great composure: “Das habe ich nicht gewollt!” (*That was not what I intended!*) Loew seems to have felt like making a similar remark, upon hearing of the death of poor Schiner. Schiner's merit as a popularizer cannot be appreciated enough; in his time he was the good genius of dipterology; without him this science, as it stood in 1860, would have very likely undergone, as to propagation among the public, a period of standstill. His self-denial, his perseverance, reserve, and conscientiousness deserve the highest praise. Nevertheless, for some, to me, inexplicable

reason he received but scant justice. I have not been able to discover any obituary notice of him, and it seems that none has been published. It is through my friend Kowarz that I ascertained the place and date of his birth (Fronsberg, in 1813); he was therefore sixty years old at the time of his death.

The Atlas-folio publication on *Trypetae* appeared in 1862, and it was in March of the same year that Loew, upon my asking his permission to describe some new North American *Trypetae* which I had discovered, and which I had a perfect right to describe without his permission, gave me this characteristic answer: "Your frank question I answer with an equally frank confession of a foible of mine, that, in regard to this particular family, I am *a little jealous* of the publications of others!" (Compare my "Introduction," p. 20.) And this "frank confession" affords the explanation of Loew's extraordinary conduct towards Schiner!

XXII BRAUER AND MIK

An Account of their Opposition to my new System of Diptera, followed by a Characterization of these two Dipterologists

Professor Brauer was very wroth about my Chaetotaxy (compare above, Chapter XX), but after the little paper in the *Ent. Monthly Mag.*, London, 1891, p. 35-39 (128, 1891), which threatened to upset his "System," his ire against the "Entomographer" and "Catalogue-maker," as he was pleased to call me, knew no bounds, and found expression in a paper, "On the Families of Diptera," which he read at the meeting of the Zoologisch-Botanische Gesellschaft in Vienna on the 6th of May, 1891. An abstract of this paper, published under that date in the Proceedings of the Society, contains the following theses, which I translate verbatim:—

"The incredible error of some entomographers [sic!], that the larvae of insects have no importance in classification."

"The erroneous opinion that the position, the presence or absence, of the so-called 'macrochaetae' have any importance in the definition of families of Diptera, their importance being confined to species and genera, as hairy and bristly forms occur in every family." (!)

"The systematic position of the *Ptychopteridae*. The characteristic

See Sketch of Brauer, by Anton Handlirsch, in the Appendix, p. 154, for Brauer's side of the case. (Verh. K. K. z.-b. Ges. Wien, 1905, 129-166, with portrait.)

transverse furrow, pointed out by Osten Sacken, is not homologous to that of the *Tipulidae*, as it corresponds to the anterior furrow of the thorax, that of the *Tipulidae* to the posterior one. *Ptychoptera* has all the characters of the *Eucephala*."

Brauer, in this connection, always speaks of the genus *Ptychoptera* alone; if he had examined the thorax of *Tanyderus* and *Idioplata*, which also belong to the section *Ptychopterina*, he would have at once perceived that his pretended distinction between the anterior and posterior furrows has no foundation whatever. The mesothoracic suture, characteristic of the *Tipulidae*, is exactly in the same place in the section *Ptychopterina* as in the other sections. The genus *Ptychoptera* has furrows which have nothing to do with that suture, but disguise it to an experienced eye. In *Tanyderus* and *Idioplata* these deceptive furrows do not exist, and the mesothoracic suture is distinctly visible at its usual place.

Finally, Brauer protested against "compilers of catalogues" who, without further proof, permit themselves to introduce changes in the system, and upset the opinions of entomologists of "established reputation" ("gewiegte Entomologen").

The notion that *Ptychoptera* is not a Tipulid, and is related to the *Culicidae*, seems to have been a particular hobby of Brauer. He reverts to it on more than one occasion. In his article "Ueber die Verwandtschaft und systematische Stellung der *Blepharoceriden*" (*Wiener Ent. Zeit.*, 1882, p. 1-4), a short paper which, by the way, abounds in very dubious assertions,¹ Brauer says: "To the tribe *Eucephala* belong also the *Ptychopteridae*, a relationship which Osten Sacken has always contested, because he takes them for *Tipulidae*."

To sustain this proposition, Brauer adduces Adolph's theory of the venation of Diptera (*Wien Denkschriften*, 1882, p. 94, at bottom), and also some allegations about the number of Malpighian vessels of *Culex*, *Psychoda*, and *Ptychoptera*, the importance of which, in this particular case, is still questionable. Since 1882, the position of Mik, between Brauer and myself, became very difficult. In that year,

¹ It is refreshing, as well as instructive, to compare this article of Brauer with the six pages of Eduard Becher in the same volume of the *Wiener Ent. Zeit.*, p. 49-54, modest in tone, but pregnant with facts most suggestive of future developments. About the position of *Phora*, however, I cannot agree with Becher.

his paper, "Zu Osten Sacken's Chaetotaxie der Dipteren" (*Verh. zool.-bot. Gesellsch.*, Wien, 1882), provoked the ire of Brauer. But when in the same year my criticism of Brauer's paper on *Notacantha* appeared (89, 1882), Mik felt that he had to choose between us. He stood by Brauer, for the very plausible reason that it was to his advantage to be on good terms with the very sensitive Custodian of the dipterological department of the Museum in Vienna. Conscious of having hurt Brauer's feelings by his publication on my Chaetotaxy, he did his utmost to atone for this failing. Brauer's paper on the *Notacantha* (1882), which I had demolished by my criticism, Mik praised to the skies in an elaborate article (*Wiener Ent. Zeit.*, 1882, p. 176-177). Of Brauer's system of Diptera he became a staunch supporter, especially of the group *Eucephala*, and of the position of *Ptychoptera* within it. He became most ingenious in his arguments. In the *Wiener Ent. Zeit.*, 1882, p. 319, he said: "Brauer has satisfactorily proved that *Ptychoptera* must be excluded from the *Tipulidae*; any unprejudiced observer of a species of *Ptychoptera* in life will at once recognize in its whole behavior ('in ihren ganzen Benehmen') an evident resemblance to the *Mycetophilidae* (!) and not to the *Tipulidae*. What is the use of this persistent resistance against facts that are obvious?" etc. Mik would not have dared to utter such an absurdity, if he had seen the flight of the American genus *Bittacomorpha*, which, although it belongs to the section *Ptychopterina*, is, in its mode of flight, unlike any known Tipulid or Mycetophilid!

With regard to Beling, Mik observes (*Wiener Ent. Zeit.*, 1886, p. 287): "He still continues to place *Ptychoptera* among the *Tipulidae*!"

Mr. Verrall's "List of the British Tipulidae" (*Ent. Monthly Mag.*, 1886-1888) suggests to Mik the same remark (*Wiener Ent. Zeit.*, 1889, p. 136): "What astonishes me is, that Mr. Verrall, in his 'List of *Tipulidae*,' comprises also the *Dixidae* and *Ptychopteridae*; — probably from mere deference ('Pietät') for the old, but quite unjustifiable systematic position of these two families of *Diptera eucephala*."

A whole page of discussion in another place (*Wiener Ent. Zeit.*,

1888, p. 226-227), concludes thus: "To return to *Ptychoptera*, I recommend the comparison of the figures of the wings of *Culex*, *Dixa*, and *Ptychoptera* in Van der Wulp's 'Diptera Neerlandica,' Tab. X, f. 2, 9, and 10. A glance at these figures shows us, that the wing of *Dixa* has the greatest resemblance to that of *Culex*, and that the wing of *Ptychoptera* discloses a near relationship to that of *Dixa*. And *Dixa* is a *gnat* and not a *crane-fly*!" It seems natural to ask, why Mik recommends the study of a plate of Van der Wulp, and not that of the natural objects? The reason is this: Brauer, at the end of his paper, "Vergleichende Untersuchungen des Flügelgeäders der Dipteren-Familien nach Adolph's Theorie" (*Wien Denkschriften*, 1882), introduced the following singular "captatio benevolentiae": "As the best existing figures of wings, which are also applicable to Adolph's theory, we can recommend those of Van der Wulp in his 'Diptera Neerlandica.'" Evidently Mik wanted to please Brauer in referring to these same plates.¹

In 1890, a change seems to have taken place in the disposition of Mik towards Brauer, in consequence of the publication in that year of Brauer's first instalment of his *Muscaria Schizometopa*. I noticed this change in his correspondence with me, and it is also evident in Mik's references to Brauer's new publication (in the *Wiener Ent. Zeit.*, 1890, p. 155 and 159; 1892, p. 55); in the acrimonious reply by Brauer and Bergenstamm (*ibid.*, 1892, p. 108); and in the rejoinder of Mik (*ibid.*, p. 110-113).

It is at that time that Mik, in an unguarded moment, praised the systematic essay I had published in the *Berl. Ent. Zeit.*, 1892 (130, 1892) in the following terms (letter of December 10, 1892): "I thank you for sending me your interesting systematic study. It suits me better than Brauer's publications on the same subject. There are some points, of course, in which everybody will not agree with you. The system itself is certainly more natural, and less one-sided than that of Brauer. I rejoice in the expectation of the second part," etc.²

¹ Similar efforts of Mik in favor of Brauer will be pointed out in my *List* (93, 1883), where he supported Brauer's astounding assertions concerning the relationship between *Scenopinus* and the *Mydaiidae*.

² "Ich danke für die Zusendung Ihrer interessanten systematischen Studie. Sie conveniert mir mehr als Brauer's Arbeiten über das gleiche Subject. Natürlich sind

This confession notwithstanding, Mik did not muster up courage to appear with his true opinion before the public. My above quoted 130, 1892, which he had praised in his letter, is not mentioned at all in his "Dipterologische Referate." Two years later, in his notice on Kowarz's "List of the Diptera of Bohemia" (*Wiener Ent. Zeit.*, 1894, p. 31), Mik made an obscure deprecatory allusion to my work in the following terms: "It has agreeably surprised us that the author has not been misled ('sich nicht beirren liess') by new researches which still require confirmation ('einer Klärung bedürfen'), or which he had not had the opportunity to verify himself." The fact is that Kowarz had approved my distribution in a letter to me, but he was of course not bound to introduce it immediately into his "List."

All these tergiversations did not prevent me from maintaining my good relations with Mik as long as he lived, even when, in the matter of *Paraocera* (156, 1897), I felt bound to criticize him rather severely. This also speaks for the good nature of Mik, and merely proves the fatal influence upon him of Brauer.

In criticizing Brauer's reconstruction of the system of Diptera, I never intended to disparage his merit in another branch of entomology. His real vocation was the biology of insects. As early as 1850, he began the publication of a series of excellent papers on the early stages and the habits of Neuropterous insects (*Chrysopa*, *Osmylus*, *Panorpa*, *Mantispa*, *Myrmeleon*, *Bittacus*, *Ascalphus*, *Boreus*). The climax of his success was reached in the investigation of the history of the *Oestridae*, which became his life-work, and attracted universal attention.

The beginning of Brauer's study of the *Oestridae* is told by himself, in his first publication about them: "Die Oestriden des Hochwildes," etc. (*Verh. zool.-bot. Gesellsch.*, Wien, 1858, p. 385-714). Brauer's friend, the dipterologist Dr. Egger, was surgeon at the Austrian Court, and attended the Imperial hunts, and in this capacity became very useful in procuring opportunities, as

Einzelheiten, mit welchen nicht jeder übereinstimmen wird. Das System ist aber sicher natürlicher und nicht so einseitig als das Brauer'sche. Ich freue mich auf den zweiten Theil," etc.

well as material, for Brauer. Loew wrote me (October 1, 1858): "In dipterology, we have nothing new to offer. The greatest interest is afforded by the minute and thorough investigations of Brauer in Vienna on the European *Oestridae*. Zeal, ability, and entire freedom from all other preoccupations are combined in him, and enable him to produce work of the highest excellence. The number of the known European species has now risen to *twenty*, and probably more are to follow. One of the most interesting among the new species comes from Russia, and was for a long time known only in a single specimen in my collection. It was discovered many years ago by Pastor Büttner, of Schleck, in Curland (*Gastrophilus lativentris* Loew)."

Those who want to appreciate Brauer at his best, should read, as a specimen of his powers, the account of the minute and persevering observations on the common bot-fly of the horse (*Gastrophilus equi*) in the "Monographie der Oestriden," p. 56-66, 1863. In the course of these studies, Brauer came to investigate, and to describe with more accuracy than had been done before, the mode of pupation of the Diptera, and of the emergence of the imago (compare *ibid.*, p. 32-34; and also Brauer's communication to Schiner, *Verh. zool.-bot. Gesellsch.*, Wien, 1864, p. 209). He proposed very happily chosen names for the two forms of transformation which he had defined. Although his ideas were criticized by an experienced dipterologist, Dr. Gerstäcker, I adopted them at once, for, having bred many Diptera from larvae, I was fully prepared for his generalization. But Brauer went too far when, later, he imagined he had *discovered* the two primary subdivisions of the Diptera. His *Cyclorrhapha* with their peculiar barrel-shaped pupa were long ago defined by Latreille and called *Athericera* (Fam. Naturelles, 1825, p. 425). A still greater error of Brauer was the suppression, as suborder, of the *Nemocera* Latr., which was even a better defined suborder than the *Cyclorrhapha*. For this reason, in my new subdivision, instead of *two* suborders, I adopted *three*, and gave them double names, borrowed from Latreille and from Brauer; these names had the advantage of being descriptive of the characters derived from the mode of transformation of the larva, as well as from the antennae, the principal organ

of orientation of the imago: *Orthorrhapha Nemocera*, *Orthorrhapha Brachycera*, and *Cyclorrhapha Athericera*.

In the already quoted passage in the "Monographie der Oestriden," 1863, p. 32-34, Brauer announced the result of his researches in natural and unassuming language. "I am too little acquainted with all the divisions of Diptera to introduce a new system, and it would lead me too far at present. I merely intend to call attention to something which, so far as I know, has not been mentioned before." But since 1863, elated by the excessive praise, amounting to adulation, of his friends, Brauer gradually underwent the transformation which became apparent after the death of Loew, and his own appointment as Custodian of the dipterological collection in the Museum of Vienna. In 1880 began the publication of his quartos, in which he displayed his entire incapacity for systematic dipterology, and thus, for a *mirage* of ambition, marred his fame for a brilliant and, in its way, unique career in the biology of insects!

This chapter on Brauer had been written long ago when, in June, 1901, the Vienna Zoological Botanical Society did me the honor to send me the volume: "Botanik und Zoologie in Oesterreich in den Jahren 1850 bis 1900," published on the occasion of the fiftieth anniversary of its foundation. In this monumental work, intended for history, the compilation of the paragraph on Diptera (p. 344-348) was of course entrusted to Brauer. With his usual carelessness, he apparently accomplished this task off-hand, from memory, and without consulting any literary documents. About Schiner, his compatriot and collaborator, he has this: "Any one who, like the writer of these lines, was acquainted with Schiner in 1850 and knew that at that time Diptera were entirely unknown to him (he studied ornithology and botany), will appreciate the energy and application that enabled him, in 1862, to publish the first volume of his "Fauna Austriae, Diptera."

I find in Hagen's "Bibliotheca Entomologica," Vol. II, p. 124, that Schiner's first publication is dated 1851, and consists of a "List of beetles, new for the Austrian fauna, and not contained in Redtenbacher's work." The date of 1851 of this publication

presupposes for Sehiner a study of several, perhaps many years, at any rate before the date 1850, assigned to it by Brauer. In a letter to me, dated 1868, and quoted by me on p. 163 above, Sehiner mentioned his *twenty* years of occupation with Diptera, which shows that he must have begun it about 1848. This instance suffices for the rest, as an example of Brauer's inaccuracy.

But the principal task for Brauer should have consisted in giving an account of his own work on Diptera. Unfortunately, with antecedents like his, it was easy to foresee that this task would prove impossible for him, and the result has justified this prevision. As, in this "Report," Brauer could not forego the temptation of a renewed attack upon me, I feel bound to take notice of it here, even at the risk of repeating myself.

One of the principal events of Brauer's scientific career was the publication, in 1883, of his new "System of Diptera." His "*Report*" does not contain a single word about it. Brauer was evidently ashamed to recall this hastily constructed, incoherent scheme, in which the preposterous group *Eucephala* embraced the following medley of families: *Mycetophilidae*, *Bibionidae*, *Chironomidae*, *Culicidae*, *Blepharoceridae*, *Simuliidae*, *Psychodidae*, *Rhyphidae*, and even the *Ptychopterina*, which were thus separated from the *Tipulidae*! This silence of Brauer about his "System" saved him from the disagreeable duty of reporting the doleful tale of its complete collapse. The intervention of Prof. *L. C. Miall* (of Leeds), with his statement about the *position of the brain of Chironomus*, demolished at one stroke the edifice of Brauer's pretended "Untersuchungen" and "Thatsachen!" Whether this important omission was consistent with Professor Brauer's duty towards the learned Society which had entrusted him with the task of preparing the Report on Diptera, is another question.

The history of the collapse of Brauer's "System" is told in my article in the *Berl. Ent. Zeit.*, 1893, p. 378-379: "Rejoinder to Professor Brauer's 'Thatsächliche Berichtigungen,'" etc.; in Brauer's "Bemerkungen," etc. (*ibid.*, 1894, p. 235), in which, with his usual haste and negligence, he quoted a statement from Weismann, that he believed to refer to *Corethra*, while it referred to *Chironomus*; and in the short reply, of seven lines only, of Professor Miall (*ibid.*, 1894, p. 447), stating that "the quotation from Weismann related to *Chironomus* and not to *Corethra*, which has no place in this discussion," etc.

Conscious of his failure, Brauer, in his "Report," attempts to save at least his subdivision of the order Diptera into two suborders, *Orthorrhapha* and *Cyclorrhapha*, which he considered as a great discovery. As I had pointed out the expediency of adopting *three* suborders, instead of two, Brauer had to attack me. He did so without once mentioning my name, in the following pompous but obscure passage (*loc. cit.*, p. 346): —

"Die gefundenen Thatsachen werden standhalten; denn sie werden dadurch nicht umgestossen, dass jemand auf Grundlage ganz derselben Charaktere drei oder mehr Gruppen festhält und in der Einbildung lebt, ein neues System zu machen, indem er die Schlussstriche der Gruppen vermehrt. Ein System muss sich aus der Untersuchung der Formen ergeben, nicht aus willkürlichen Abstractionen des Geistes, und das Wahre wird durch die Natürlichkeit desselben bekräftigt."

"Mit der Einteilung der Dipteren in *Orthorrhapha* und *Cyclorrhapha* deckt sich die von Weismann acceptirte Einteilung nach den extremsten Formen: Typus *Culex* und Typus *Musca*."

The following is an attempt at an *à peu près* translation: "The discovered facts will remain; they will not be upset when *somebody* adopts three or more groups based upon exactly the same characters, in the vain conceit of having founded a new system by merely multiplying the lines of division between the groups. A system must be the result of the study of forms, and not of arbitrary abstractions of the mind; what is true will then be strengthened by its appearing natural."

"The division of Diptera in *Orthorrhapha* and *Cyclorrhapha* coincides with the division adopted by Weismann, based upon the two extreme forms: the type of *Culex* and the type of *Musca*."

I am not aware that the first part of the above passage, intended as a criticism of my method of classification, can be fairly accepted as such. As to the *second* part it contains most decidedly an argument for *my* subdivision, and not for that of Brauer. My *Nemocera vera* (*Culicidae*, *Chironomidae*, *Tipulidae*, etc.) represent indeed the type of *Culex*. But my *Nemocera anomala* (*Bibio*, *Simulium*, *Blepharocera*, *Rhyphus*, *Orphnephila*), with their holoptic heads, well-developed pulvilli, etc., do not reproduce that type in the least! The rest of Brauer's *Orthorrhapha* — that is, the great majority of them (*Tabani*, *Bombylii*, *Asili*, etc.) — have nothing to do with the type of *Culex*, and reduce Brauer's assertion *ad absurdum*!

I maintain my conviction that "the true end of classification is

an *easier survey of affinities*, a temporary aid to memory. In space and time all divisions become convergent and finally confluent" (*Berl. Ent. Zeit.*, 1895, p. 160; also reproduced in my "Introduction," p. 14). Brauer's suborder *Orthorrhapha*, instead of being a help to memory, is, on the contrary, a confusing concept, as it tends to obliterate the breach between Macquart's *Nemocera* and his *Brachycera*, a discontinuity which, as I have shown repeatedly, and lately in my "Introduction" (p. 11), is greater than that between the latter and Latreille's *Athericera* (which correspond to Brauer's *Cyclorrhapha*). When Brauer, after Loew's death in 1879, began to consider himself as his natural successor, he was so little prepared for general dipterology that he was entirely unacquainted with the existence of such a breach. As late as 1883, in his "Zweifl. d. K. Mus. in Wien," Vol. III, 1883, p. 9 (in the middle) he has this passage: "Let dipterologists, as a matter of convenience, continue to speak of *Nemocera* and *Brachycera*, such natural groups do not exist, and it is impossible to define them with natural characters." About this thesis Brauer wrote a long dissertation (*Sitzungsb. Akad. Wiss.*, Wien, 1885, p. 385-413) which is rather *heavy reading*, and consists of a compilation of most startling propositions (for instance on p. 411 about the analogies in the venation of *Rhyphus*, *Leptis*, etc.).

My three suborders are a reproduction of the arrangement of Latreille, and I believe to have done justice enough to Brauer in connecting his well-chosen names, descriptive of the early stages of these insects, with Latreille's names, derived from the structure of the antennae.

At the end of his "Report" (p. 347 at bottom), Brauer, who never forgave me my guilt of Chaetotaxy, could not help venting his spite once more in reiterating, like an unavoidable *refrain*, the concluding passage of his "Obituary of Mik" (*Wiener Ent. Zeit.*, 1901, p. 4): "The doctrine of the distribution of bristles has been in 1873 put in practice by Mik and Loew, — long before *another* invented for it the learned term *Chaetotaxy*!" (Compare above, p. 156.)

Brauer avoids calling me by my name, as if it was that of the Evil One. Here he calls me *another* ("ein anderer"), and in a passage quoted above he speaks of

me as *somebody* ("jemand"). If in my references to Brauer's ill-will towards me I have not always been able to maintain the tone suitable to scientific gravity, the fault is not on *my* side!

Prof. Josef Mik's greatest merit, as dipterologist, consisted in his intimate knowledge of the dipterological fauna of central Europe, as far as it is represented in the "Fauna Austriaca" of Schiner. Within this department, and especially during the earlier part of his activity, Mik contributed a considerable amount of useful descriptive work to nearly all the families of Diptera. In addition to this, during his whole career he never ceased to produce papers on the biology of Diptera, carefully worked out, and provided with well-drawn illustrations. In both respects, in descriptive as in biological work, he deserves an honorable place among the specialists in this order of insects. Another merit of his consisted in his correspondence with many dipterologists in different parts of Europe, in his correct naming of specimens for them, and his naturally obliging disposition in assisting them. He has been very useful in diffusing the study of Diptera in France, Italy, England, and even in North America; and this merit, so far as I have been able to observe during my travels, has been duly appreciated everywhere.

Loew, in a letter to me, dated from Guben, March 20-22, 1875, that is, twelve years after the appearance of Mik on the scientific stage and twenty-five years before the end of his career, spoke of him as follows: "Mik, who is perfectly capable of good work, is prevented by his scrupulosity, and other peculiarities of his character, from making as much progress as one could wish."¹

What Loew meant by "scrupulosity" I do not quite understand, unless it refers to Mik's exaggerated attention to trifles, and his principal defect, the propensity to scatter his working power, instead of concentrating it on some well-defined task. It was especially since 1882, when the *Wiener Entomologische Zeitschrift* began to appear, that an unlimited facility for publication was offered to Mik, and he soon displayed a foible which may be called, to use a mild term, an *excess of self-assertion*, stimulated by

¹ "Mik, der Gutes zu leisten alle Fähigkeit hat, kommt in Folge seiner Scrupulosität und anderer Charaktereigenthümlichkeiten, nicht so vorwärts, als wohl zu wünschen wäre."

a regrettable vanity. Mik gradually grew up in his own self-esteem as a critic, and allowed himself to express opinions and to lay down rules which far exceeded his intellectual endowment. He thus became guilty, as I am going to show, of many untenable assertions, and involved in glaring inconsistencies.

Under the general headings of "Dipterologische Notizen" (*Wiener Ent. Zeit.*, 1883, p. 34, 64), and later of "Dipterologische Miscellen" (*Wiener Ent. Zeit.* from 1886 up to 1900, the year of Mik's death), Mik published more than *two hundred* disconnected, often long-winded notices, criticisms, observations, corrections, etc., of every kind, of which, from time to time, he gave a list ("Inhaltsübersicht"). Supposing even that the multitude of facts, thus offered, are useful and trustworthy (which is by no means certain), the difficulty alone of getting access to them makes them almost useless. In the "Introduction" to this "Record" (p. 18) I said: "Entomological literature would become intolerable if it were considered every one's duty to criticize and contradict in print the many errors which continually rise to the surface." Mik has been a sinner in this respect, and the reproach of egotism cannot be withheld from him.

The same defect of obnoxious self-assertion appears in the habit of Mik of introducing a *multitude of generic names* for the sake of having the pleasure of appending his *mihi* to them. As the fauna of central Europe, Mik's specialty, did not offer him many legitimate opportunities for exercising this fancy, he had recourse to two expedients: the first of them consisted in the unnecessary subdivision of well-established genera. In Mik's "Dipterologische Untersuchungen" (Vienna, 1878) *twelve* new genera of *Dolichopodidae* are introduced, with the following preamble: "Occupied with a revision of the *Dolichopodidae* I take occasion to introduce several new genera, which I make public in the following pages, before the publication of the other results of my revision." Mik has never published this promised "Revision," and thus his genera remained incompletely defined.

In the *Verh. zool.-bot. Gesellseh.*, Wien, 1881, p. 320-327, the genus *Clinocera*, which, upon Mik's own showing (*ibid.*, p. 320), contained at that time only forty palaearctic species, was subdivided

by him into *nine* genera. Among the alleged reasons for this unnecessary subdivision he has the following very questionable one: "If I do not introduce these genera, sooner or later somebody else will!" As an apology for the incompleteness of his definitions he promises a monograph of the genus, which he has in preparation (*ibid.*, on p. 320 twice); a promise which, like the one mentioned above, he never kept.

In my paper on *Paracrocera* Mik (*Berl. Ent. Zeit.*, 1896, p. 323) I have shown that this genus, as well as *Symplectomorpha* Mik and *Alloconeurus* Mik, are superfluous subdivisions, which merely tend to defeat the mnemonic purpose of classification. *Olbiosyrphus* Mik (*Wiener Ent. Zeit.*, 1897, p. 66), it seems to me, belongs to the same category. By the way, in the "Namenregister" of the same volume, p. vii, in consequence of a *lapsus calami* the genus is called *Olbio-gaster*, a generic name introduced by me for a new form of *Rhyphus* (*Biol. Centr.-Amer. Diptera*, p. 20).

The other opportunity for introducing new names (generic as well as specific), and an occasion for the display of his *mihis*, was afforded to Mik by the *nomina bis lecta* of other authors. Such opportunities were rather numerous, and it would be useless to reproduce all of them here. In the volume of the *Wiener Ent. Zeit.*, 1886, alone, I find *Thamnodromia* Mik (*Empidæ*) proposed for *Phyllodromia* Zett., p. 278; *Lepidostola* Mik (*Syrphidæ*) for *Lepidomyia* Loew, p. 279; *Monoclona* Mik (*Mycetophilidæ*) for *Staegeria* v. d. Wulp, p. 102; *Allocotocera* Mik (*Mycetophilidæ*) for *Eurycera* Dziedz., 1887, p. 269; *Microtricha* Mik (*Tachinidæ*) for *Stylomyia* v. d. Wulp, etc.

Specific names, when found to be *bis lecta*, were, "pour dorer la pilule," dedicated, by Mik, according to the received custom, to the original author: *Beckeri mih*i (*ibid.*, 1894, p. 166), *Eggeri mih*i (*ibid.*, 1897, p. 66), *v. d. Wulpii mih*i (*ibid.*, 1899, p. 143), *Willistonii mih*i (on the same page), *Sharpii mih*i, *Strobltii mih*i (*ibid.*, 1900, p. 148), etc. At the same time Mik, with a remarkable but quite characteristic inconsistency, praises Dr. C. Kértesz "for having refrained from introducing new names for *nomina bis lecta*" ("zu loben, dass er sich enthalten hat *nom. b. l.* umzutaufen," *ibid.*, 1900, p. 224)!

Most of these changes of names, as I have shown, have appeared in the *Wiener Ent. Zeit.* And yet, in the very same year when this periodical was started, Mik had expressed (*Wiener Ent. Zeit.*, 1882, p. 208) an opinion about the treatment of *nomina bis lecta* which was in direct opposition to his practice in regard to them. He said: "It is unnecessary nowadays so anxiously to conform to the rule of nomenclature which requires such a change of names; the consistent application of it would involve frequent changes, and enable the first comer to introduce them with a 'Nomenclator Zoologicus' in hand." A year later (*Verh. zool.-bot. Gesellsch.*, Wien, 1883, p. 184) Mik, intolerant as he was towards others, develops still further this rule of nomenclatorial toleration: "As Robineau-Desvoidy's generic name *Eloceria* is grammatically incorrect, I propose the amended name *Helocera* for it . . . without caring to inquire whether such a name is preoccupied for some other animal or not: a genus of Diptera of that name does not exist." In an article, "Nomenclatorische Fehltritte" (Nomenclatorial Errors), published five years later in the *Verh. zool.-bot. Gesellsch.*, Wien, June 6, 1888 (*Sitzungsberichte*), Mik expressed the same view: "A time will certainly come when the same [generic] name will be allowed to be used several times, as long as it is not used in the same order of the system."

Mik is confusing here a rational toleration with an explicit permission. If such a broad rule as he advocates were adopted, any entomologist, when looking for a new generic name, would be permitted to select for his genus a well-sounding name in another order of insects. The inconvenience which would result from the large number of *nomina bis lecta* thus introduced into the different orders of insects is obvious! At present, with the yearly publication of entomological reports, containing alphabetical lists of new genera, any author, with a little diligence, can reduce to a minimum the risk of hitting upon a generic *nomen bis lectum*. But the truth is that instances are frequent of a total neglect of such precautions, even on the part of entomologists most conveniently situated for availing themselves of them.

Another instance of Mik's excess of self-assertion (as I have mildly called it) is found in the paragraph entitled, "*Eurymyia*

Bigot," in the *Wiener Ent. Zeit.*, 1897, p. 115. Mik amends this name into "*Eurinomyia mihi* (nom. nov.)." He acknowledges at the same time that Schiner, thirty-three years before, in his "Catalogus Systematicus," etc., 1864, p. 108, had proposed the name *Anasimyia* for the same generic concept. It would have been proper for Mik to retain this name, but he rejected it, under the futile pretence "that it was a mere Catalogue name" ("ein blosser Catalogsname"), which is by no means the case! Schiner ("Fauna Austriaea," Vol. I, p. 338, 1862) had isolated the two species *Helophilus transfugus* Linn. and *H. lineatus* F. at the very beginning of his analytical table of this genus by the character: "Lower half of the face projecting, cone-shaped, and pointed in profile." Two years later in his "Catalogue of Austrian Diptera," which contained nothing but names and no definitions, Schiner had a perfect right to propose a name for a generic concept *which he had defined before*, the more so as he referred to it the two typical species connected with it in his previous publication. As to Bigot, when he received from a friend a single female specimen of *Helophilus lineatus* F., captured north of Paris, he did not even recognize this species in it, but, being struck by the projection of its face, not unlike that of the genus *Rhingia*, he immediately described it as *Eurymyia rhingioides*, gen. et sp. nov. It is in this blunder of Bigot that Mik found another opportunity for a *mihi* of his own (compare above), and for discarding under a false pretence the generic name *Anasimyia* proposed long ago by Schiner!¹

Mik's general criticisms of the publications of other authors abound in instances of arbitrariness and egotism. On one side, popularity is courted by profuse compliments and "eaptationes benevolentiae" towards authors not deserving them; on the other side, most unreserved criticisms, and sometimes in very bad taste, are showered upon deserving entomologists who had the misfortune to disagree with Mik. In such cases Mik often displays the prevailing defects of his style, diffuseness and verbosity. A redundant and unjust example of this (*Verh. zool.-bot. Gesellsch.*, Wien, 1886, p. 479-483) is directed against Dr. Dziedzieki's method of utilizing

¹ Mr. G. H. Verrall's account of this case in his "Syrphidae," London, 1901, p. 524, is incorrect and requires emendation.

characters founded upon the structure of the forceps (*hypopygium*) of *Mycetophilidae* for generic and specific descriptions. An undeserved praise is bestowed on Bigot's very insignificant paper on *Acanthomeridae* (*Ann. Soc. Ent. France*, 1882, p. 453-460): "This paper offers a clear account of this remarkable family of Diptera." A slight criticism which Mik ventured against Bigot (*Wiener Ent. Zeit.*, 1883, p. 65) is immediately redeemed by praise: "A zealous and meritorious dipterologist, whom I highly esteem, and intend to prove by having the honor to dedicate to him an interesting species of the genus *Dolichopus*." But after Bigot's death we find Mik describing him (*ibid.*, 1897, p. 170) as being "in systematic questions very badly informed" ("dass er gerade in der Systematic sehr schlecht orientiert war").

My attempt to introduce a psychological and moral element into entomological criticism found no favor with Mik. However, he expressed his opinion with great caution and in a rather disguised manner. In a notice of his (*Wiener Ent. Zeit.*, 1898, p. 71) about Abbé Kieffer's attack upon Mr. Rübsaamen, Mik introduced a passage that evidently refers to a paper of mine, published a short time before, concerning the two editions of Loew's first Essay ("Erstlingsarbeit") on the Diptera of Posen (Osten Sacken, in the *Berl. Ent. Zeit.*, 1896, p. 279-284; **153, 1896**). The comparison of these two editions convinced me that Loew had been prompted to publish the *second* (almost identical) edition in Oken's *Isis*, in consequence of a feeling of rivalry towards Zeller, whose admirable first paper on Diptera had just appeared in the same periodical. That some of the expressions in Mik's passage, which I reproduce here, are covert allusions to my paper nobody will contest who examines the matter with some attention. This quotation I must give in the original German, as its obscurity, amounting to ineptitude, prevents me from making an adequate translation: —

"Bei dieser Gelegenheit wollen wir nicht unerwähnt lassen, dass in neuester Zeit in der entomologischen Wissenschaft Streitschriften fast rein persönlicher Natur in bedenklicher Zahl zu Tage treten. Man bemerkt darin leider eine förmliche Sittenrichterei, die sich herausnimmt, dem Character der Personen nahe zu treten! Ja selbst die Todten werden nicht geschont! Der aufmerksame Leser der entomologischen Literatur wird mir

recht geben, auch wenn ich keinen Namen nenne. Wir fragen aber, soll denn so etwas die Sache der Wissenschaft nützen?"

In his notices about my papers Mik more than once fell into error in consequence of his very limited knowledge of the English language (compare *Wiener Ent. Zeit.*, 1884, p. 27, about *Apiocera*; *ibid.*, 1897, p. 32, about *Curupira* and *Snowia*, etc.). Such slips will be mentioned at their proper places in this "Record." One of them especially deserves attention, Mik's mistake about the meaning of the term *piracy*, in the sense of *plagiarism*. He took it in the literal sense, and resented that I had called him a *buccaneer* ("Seeräuber"). My explanation and his reply to it (*Wiener Ent. Zeit.*, 1897, p. 212) may be considered as a slight contribution to the "gaiety of nations" (156, 1897).

Mik's numerous publications are open to many other criticisms; but, as my intention was to describe Mik's manner rather than his matter, what I have given here will be sufficient for my purpose. It will belong to others to pass a final judgment upon the merit of Mik's work on the Diptera of central Europe. That is not my specialty. In this notice I have attempted to prove that Mik, with the advantages which he had, might have done much more for dipterology if it had not been for the peculiarities of his character. Loew was therefore right in his estimate of Mik, which I have reproduced above (p. 174).

XXIII NOTICE ON THE CIRCUMSTANCES OF THE PUBLICATION OF TWO PRINCIPAL WORKS (1830, 1863) OF ROBINEAU-DESVOIDY

Robineau-Desvoidy was twenty-seven years old when he presented to the Academy of Sciences of Paris his voluminous manuscript of the "Myodaires," that had cost him several years of labor. The competent members of this institution were at once struck with the great merit of the work, and with the new method of study which it introduced. A committee was appointed, which consisted of the greatest zoological authorities of that time: *Latreille* (1762-1833), then aged sixty-five, and at the height of his

reputation for his work on the Invertebrata; *C. Duméril* (1774–1860), a celebrated zoölogist and comparative anatomist, who, among other works, had published in 1801 an “Exposition d’une méthode naturelle pour l’étude de la classification des Inscetes”; *Blainville* (H. M. Ducrotay de) (1777–1850), an able zoölogist, student of Cuvier, and especially devoted to classification. After a careful study of the manuscript by these distinguished men, Blainville was entrusted with the task of drawing up a “Report,” which, signed by the members of the committee and countersigned by Baron Cuvier, Perpetual Secretary of the Academy, Commander of the Royal Order of the Legion of Honor, was read in the Academy on October 2, 1826, printed in pamphlet form (24 pages in 8vo), and distributed among the members of the Academy.

This Report, although referred to by Rob.-Desvoidy himself in his Preface (1830), seems to have attracted very little attention on the part of dipterologists. It appeared at a time when *Meigen* had but recently published his fourth (1824) and fifth (1826) volumes, containing the Calyptrata; when *Macquart* was issuing the early parts of his “Diptères du Nord” (1823–1826), containing the Nemocera and a portion of the Orthorrhapha Brachycera; and when *Fallén* had completed his whole work with its last instalment (“Supplementum Dipt. Sueciae,” 1826). In England, neither *Halliday* nor *Curtis*¹ had appeared on the stage. In Sweden, *Zetterstedt* had begun his publications, but they as yet contained nothing on Diptera.

I consider this Report as a very important document for the history of dipterology of that time, and for this reason I shall give a detailed summary of its contents, with abundant verbatim extracts.

The Report begins with a circumstantial account of the history of dipterology, describing its gradual progress since Linné’s “Systema Naturae” of 1748, explaining the different methods and nomenclatures adopted by *Fabricius*, *Latreille*, and *Duméril*, and the temporary confusion which resulted from the

¹ John Curtis’s earliest issue of his “British Entomology” is, however, dated 1823.

want of agreement between them. *Meigen's* work and his merit in using characters borrowed from the venation are mentioned. It is shown how Rob.-Desvoidy, having brought together more species of Muscidae than all the earlier authors taken together, found it necessary to study this group much more in detail ("d'étudier ce grand genre d'une manière bien plus minutieuse"), and here the new characters, introduced by Rob.-Desvoidy, are enumerated and critically appreciated.

(Report, p. 6.) Attention is called to the nomenclature adopted by Rob.-Desvoidy for the different parts of the head and face; it is noticed at the same time that these terms do not quite agree with those adopted by earlier authors. The structure of the antennae had been studied by Rob.-Desvoidy in great detail, and it is acknowledged that he, with perfect reason ("avec juste raison"), considered the arista as the prolongation of the antenna (*vide* Myodaires, p. 11) and not as a mere bristle.

(Report, p. 7.) The attention given by Rob.-Desvoidy to the study of the squamae was something almost new (here the Report has a footnote saying that Latreille had already made some use of them). Rob.-Desvoidy has introduced the very happy generalization that the development of the squamae is more or less in harmony with the habits of the flies; that the most active, the largest, the most colored, and the high-flying ("haut vol") Muscidae have the largest squamae, while these organs are small, or rudimentary, in those species which remain in the vicinity of their birthplace.

Rob.-Desvoidy, with much reason, had also observed that the coloration of flies shows a certain relation with their surroundings; that those living in the open air, in the rays of the sun, have more brilliant colors than the "timid species living under the protection of rotten mushrooms." The general structure of the body, that of the abdomen and of the ovipositor, are brought into relation with the life-habits, etc.

In a word, the Report shows that the study of Diptera in life is the leading idea of Rob.-Desvoidy's classification.

Rob.-Desvoidy understood very well "that everything in an organization holds together, and that the necessity of a fact" (by

which he probably meant a condition of life) "leads directly to the necessity of organs that produce that same fact" ("ainsi, tout se tient dans l'organisation, et la nécessité d'un fait conduit directement aux nécessités d'organes qui produisent ce même fait," Myodaires, p. 23).

The next part of the Report (p. 8-20) contains a complete enumeration of the numerous divisions and subdivisions introduced by Rob.-Desvoidy. Not only were names given by him to the families and tribes, but each name is followed by a short characterization of the group, including not only characters and life-habits, but the number of genera which the families or tribes contain. That close attention was paid by the members of the commission to this distribution is proved by the criticisms which they appended to it, and which will be reproduced below.

From this enumeration we learn that the original plan of the distribution, as it was developed in Rob.-Desvoidy's manuscript, was somewhat more extensive than that which appeared later in the volume on the Myodaires (1830). Even in this volume the distribution indicated on p. 20 is not entirely carried out, and the families eight, *Micromyidae*, and nine, *Muciphoreae*, are not found farther on in the letter-press. (The words of Rob.-Desvoidy in the *Ann. Soc. Ent. France*, 1844, p. 5, evidently refer to this omission: "Les deux premiers tiers des Myodaires se trouvent imprimés en 1830; le dernier reste encore inédit.") In the Report, the subdivisions (tribus) of the *Micromyidae* (six tribes) and *Muciphoreae* (four tribes) are enumerated with short diagnoses. Moreover, the Report mentions a tenth family, which does not appear at all in the 1830 volume, viz., the *Cephalemyidae*, which was to contain the *Pipunculidae*, and of which it is said "their habits are entirely unknown, and various characters make one suppose that they do not belong to the Myodaires." This was a presentiment which has been fully justified by further experience.

The rest of the Report contains criticisms and conclusions which I deem interesting enough for a verbatim reproduction: —

"Such is the exact analysis, too short, its length notwithstanding, of the great work which Mr. Rob.-Desvoidy has submitted to the judgment of the

Academy, a work almost entirely finished ('*rédigé presque entièrement*'), and accompanied by five synopses ('*tableaux*') of all the species Diptera of the family Muscidae observed and collected by him within the small area of the Département of the Yonne.

"The total number of species is about eighteen hundred, of which fourteen hundred are new, or newly defined: seventeen or eighteen exotic genera are added.

"Your Commissioners have attentively examined the work of Mr. Rob.-Desvoidy, without, however, having been able to extend their examination to all the genera, and *a fortiori* to all the species, which, being desiccated, and for the most part very small, could not be analyzed.

"The number of new species has appeared to them in reality very considerable. It is possible, however, that Mr. Rob.-Desvoidy, not having yet formed a sufficiently positive notion of the concept of a species, which is fully established only when it is well characterized by appreciable differences in the genital organs, has considered as distinct species mere casual varieties in size, villosity, and intensity of coloring.

"Anyhow, in taking account of all these species, and in distributing them according to different points of view, Mr. Rob.-Desvoidy has been necessarily led to group them around the principal species, and that brought about the establishment of families and tribes that appear to us to a certain extent natural, although sometimes not quite distinctly characterized.

"Distinctive characters, derived from the basal joints of the antennae, from the joints of the arista, from the glabrousness or villosity of the unjointed portion of the latter, have been used for the introduction of genera which appear to us too numerous, the more so as they seem but in rare cases to be supported by characters derived from the wings and from the structure of the proboscis, organs that, unfortunately, have been somewhat neglected in the work of Mr. Rob.-Desvoidy. In examining, for instance, a certain number of genera which constitute the first family, the Calyptrata, we have become convinced that they are merely based upon but very minute differences in the proportion between the second and the third joints of the antennae. In some cases even the genus does not show the character belonging to the tribe.

"It was undoubtedly a happy thought to introduce a connection between the characters of the classification of insects and their mode of life and habits, and thus to form families founded upon the food in the larval state, as well as in that of the imago. But with this method there is a danger of going too far, and of adopting genera and species the only distinction of which would depend upon the plants, or upon the medium in which they live, and not upon their organization.

"In general, the whole systematic arrangement of the work seems to us to suffer from a too great multiplicity of subdivisions of the first, of the second, and even of the third order. The number of genera, for instance, is too great, so that, on the average, each of them hardly contains three species. We can conceive that Mr. Rob.-Desvoidy required such a scaffolding for disposing of so many and of so closely connected species, but he might, at least, have omitted a part of them.

"The family, the tribal, and even the generic names are well formed, short, and euphonic. A few of them only must be rejected on the ground of preoccupation in other families. Much less satisfactory are the names derived from men more or less celebrated in natural sciences, and especially in entomology, because they are not always brief and easy to pronounce. At the same time, they may sometimes cause the slight inconvenience of connecting the name of a distinguished man with some disagreeable epithet.

"Your Commissioners, nevertheless, do not consider the work of Mr. Rob.-Desvoidy, in the shape in which it was brought before them, as finished : —

"1st. Because he does not seem to have sufficiently limited his subject by distinctly defining it, and that he could only do by giving a preliminary survey ('tableau') of all the Diptera.

"2d. Because he has not prefixed to his work a somewhat detailed terminology, which requirement is of an absolute ('rigoureuse') necessity, although difficult to accomplish. He would, in such a case, have to take into account the two pairs of inferior palpi which he admits in the genera Phorophylla and Phyto. Indeed, he would have perceived that all the Diptera have these same organs, only less distinct, and that they constitute the lips ('lèvres') of the proboscis, the larger size of these four palpi being probably due to the prolonged extremities of each lip. He would have likewise seen that the study of the wing-veins, which he has neglected, and which have been used by *Meigen* to great advantage, might have tended to confirm several of his principal divisions. He would perhaps have been also led to use characters borrowed from the proboscis, which, when well analyzed, seem to afford good tests of affinity, although of a difficult application ('d'un difficile emploi').

"3d. Because he has not established a synonymy with recent authors who have been specially occupied with the same subject, such as *Fallén*, and, above all, *Meigen*; a gap difficult to fill, no doubt, a matter perhaps apparently of little importance, but which we strongly invite him to attend to, in the first place in a spirit of justice, and next in order not to embarrass science again by new names imposed upon the same species and the same subdivisions. Considering that *Meigen*, in taking in the work of

Panzer, of *Fallén*, of *Wiedemann*, and his own, described about three hundred species from Germany that are not included in the 'Systema Antliatorum' of *Fabricius*, it is impossible not to believe that several of Mr. Rob.-Desvoidy's species will turn out to be synonyms.

"4th. Because Mr. Rob.-Desvoidy has not, by any means, visited and studied all the collections in Paris, a task which it is very important that he should continue to pursue (for he has already begun it), in order that his work, founded on the broad basis of complete and positive observations made from life upon our French species, may be extended by means of a well-grounded analogy ('par une analogie bien conduite') to all the species collected in different parts of the world, and may later serve for philosophic conclusions on the geographical distribution, and for a final determination of the species ('à la détermination définitive des espèces'). We are even able to give the assurance that Mr. Rob.-Desvoidy, who must necessarily have had in mind a similar work, is actively occupied with it, and that at present the number of species of flies, including those observed in the Parisian collections, reaches beyond three thousand.

"Your Commissioners are also of the opinion that, whatever final form ('redaction') he gives to his work, Mr. Rob.-Desvoidy will do well to adapt his style to the subject, and to have, in a publication of this kind, no other ambition than that of clearness and simplicity of description, and to restrain himself to a strict analysis of facts, instead of offering generalizations which, in some cases, have appeared to them a little pretentious; an important defect, evidently to be attributed, however, to the youth of the author, and therefore excusable.

"These slight criticisms notwithstanding, which may rather be considered as hints towards future improvement, we estimate the work of Mr. Rob.-Desvoidy to be of a very great intrinsic value ('d'une très grande valeur intrinsèque'), because it contains observations and distinctive characters of four or five times more species of flies (*monches*) than were known to the most recent authors; it also bears witness to a mental quality and tendency, a perseverance and a patience of observation unfortunately very rare nowadays in zoölogy, where it seems to be much easier to indulge in general considerations, before even knowing a small number of specialties. The happy combination which Mr. Rob.-Desvoidy has succeeded in bringing about between his botanical and entomological knowledge—we mean to say between plants and insects—has given to his work a character of novelty which he intends to apply to other departments of entomology susceptible of it.

"Your Commissioners, in consequence, recommend you to accept the work of Mr. Rob.-Desvoidy for publication in the serial of the 'Savants étrangers,' a distinction that, in their opinion, it fully deserves. They

would even suggest that this publication should be facilitated and accelerated by all the means at your disposal. Otherwise, it might happen that, not finding a publisher who would incur the expense, the author would feel that five or six years of diligent work had been lost for his reputation, and, discouraged by such an unfortunate beginning, would suspend his entomological labors; a result which, in our estimation, would be a real loss for science as well as for the glory of France, the zoölogy of which is still very little advanced ('encore si peu avancée')."

This Report, with its genuine appreciation of the merit of a young man and a beginner, expressed by masters in science in the most considerate and encouraging terms, does, it seems to me, the greatest honor to the enlightened spirit which prevailed at that time in the Academy of Sciences in Paris. It was about this very time that Goethe, in his *retraite* of Weimar, with marked disregard of politics and other questions which absorbed contemporaries, used to follow attentively the discussions of Cuvier and Geoffroy St. Hilaire on the highest problems of biology.¹ And this enlightened point of view stands in most striking contrast with the short-sightedness of the specialists in dipterology, contemporaries of Rob.-Desvoidy, who attempted to ignore him and to suppress his work.

The first chapter of Rob.-Desvoidy's "Essai sur les Myodaires" (1830) begins with this characteristic Introduction:—

"The Royal Academy of Sciences, in its meeting of October 2, 1826, accepted for publication my 'Essai sur les Myodaires du canton de Saint-Sauveur, Dépt. de l'Yonne.' In order to render myself worthy of this honor I felt bound to revise the whole of my work, to establish it on a broader basis ('l'assurer sur de plus larges bases') and to avail myself of the criticisms of the committee on the Report. I had foreseen, what actually happened, that I was working upon a subject that was endless. Natural-

¹ Many passages in J. P. Eckermann's "Gespräche mit Goethe" refer to science and literature in France between 1827-1831, especially in connection with the periodical "Le Globe," of which Goethe was a constant reader. They are enumerated in the alphabetical Index of the latest edition of Eckermann's work (1885), under the vocable "Globe." The particular passage about the discussions between Geoffroy St. Hilaire and Cuvier will be found in Vol. II, p. 239 (December, 1831), of that edition. Mr. Demogeot, in his "Hist. de la littérature française," 10th edit., 1869, p. 621-624, gives an account of the importance of "Le Globe" as the organ of the best intellects in France at that time, and adds some very interesting references to the opinions expressed by Goethe in his "Colloquies" with Eckermann.

ists will therefore not expect perfection from me, the ideal of which, the more one studies, appears more distant. To grasp the number of specimens I describe it must be understood that I had the principal collections of Paris at my disposal. I owe to Messrs. *A. Le Peletier de Saint-Fargeau*, *Audinet-Serville*, and *Blondel* the knowledge of a large number of Parisian species, some of them with details concerning their habits. Mr. *Carcel* has communicated to me the result of his excursions in the ancient provinces of the Dauphiné and Anjou. From Mr. *G. Cuvier* I obtained the obliging permission to describe the exotic species sent to the 'Jardin du Roi' by the travellers of this Institution. Finally, the richest and most interesting collection of Myodaires which I know of, and which for many years M. *Latreille* has been pleased to increase, the collection of Mr. le comte *Dejean*, has been opened to me with the courtesy and the particular attentions by which this celebrated entomologist is so honorably known. On all sides I met with willingness to be useful to science. But, before entering upon my subject, I beg Messrs. *Latreille* and *de Blainville* to accept the public testimony of my gratitude for the zeal, the advice, and the wise criticism with which their well-known ability has never ceased to encourage my work."

The above-quoted passage proves that, in consequence of the advice which Rob.-Desvoidy had received in the Report, he had introduced some changes in his manuscript. In his first chapter he gives a definition of the Myodaires¹ (p. 45), and explains the terminology he had been using for the different parts of the head, the antennae, the proboscis, the venation, and the squamae. He says that the peculiar nomenclature of the veins and cells of the wings which he adopted was recommended to him by M. *de Blainville*. He attempted to harmonize his work with the works of *Fallén* and *Meigen*. He did not succeed in getting access to the work of *Fallén*; but he was acquainted with his classification. About *Meigen* he says (p. 18): "Mr. *Meigen*, in Germany, published a very estimable work on the European Diptera. One who has studied these insects with as much care as this able naturalist

¹ I do not understand why Rob.-Desvoidy describes the amphipneustic character of the larvae of the *Muscidae* in terms which seem rather to refer to peripneustic larvae ("larves à stigmates respiratoire situés le long du corps"; Myodaires, p. 4). The same terms are reproduced in Latin on the following page 5. They are repeated in the "*Histoire Naturelle*" (1863), p. 66. The latter passage occurs in the paragraph which is merely copied from the Myodaires, and which I consider as an interpolation of the Editors. (Compare below.)

will understand how much labor his genera *Tachina* and *Anthomyia* must have cost him. Without allowing myself the slightest criticism of a work so laborious and containing so much of new material, I will merely observe that I have too often followed a different course, and therefore cannot be accused of having worked according to his methods. Mr. Meigen excels in the art of describing Diptera, nevertheless it is certain that the possibility of establishing a perfect synonymy, based upon his characters and descriptions, may perhaps not exist ('n'existe peut-être pas'). I merely describe species which I have seen and studied. This author mentions many that I do not know, and about which, for fear of error, I prefer to remain silent."

Rob.-Desvoidy was not the only one to criticize Meigen's descriptions; there is a very instructive passage in Loew's work on the *Asilidae* (*Linnaea Entomologica*, Vol. III, 1848, p. 410-413), about the difficulties he and Zeller met with in attempting to recognize species of *Asilidae* and *Bombyliidae* in Meigen's descriptions. Even specimens labelled in Meigen's handwriting are not implicitly to be trusted, because he sometimes did not recognize his own species. This is not intended as a reproach to Meigen, but merely as an expression of the difficulty of the subject.

As Rob.-Desvoidy's *Myodaires* is the principal work which, hitherto, has been in use among dipterologists, and as his reputation has been principally grounded upon this volume, I have thought it worth while, in the preceding pages, to publish the result of my investigations of the circumstances under which it was prepared, so far as the sources were accessible to me. But Rob.-Desvoidy continued his indefatigable researches upon Diptera for twenty-five years longer, up to his death; as he said himself: "Je crois que je mourrai en loupant un diptère" ("Histoire Naturelle des Diptères," 1863, p. 33). He left an almost finished manuscript, which he considered fit for publication (*ibid.*, p. 44), and which was published six years after his death. His own opinion concerning the relation of these two works is expressed in the following terms:—

"Thus I shall enjoy the happiness of having been twice the father of the same work. The 'Essai sur les Myodaires' (1830) will recall my first flights ('élancements') in the field of science, my warm aspirations towards

the study of nature; it will afford a proof of the difficulties and the inexperience of my first steps; it will bear witness to a young, vigorous breath, which did not recoil before the amplitude, or the breadth, of any turn of sentence ("d'un souffle jeune et vigoureux, qui ne reculait devant l'ampleur ni la largeur d'aucune période").¹ But this last book, the manifest ("irréçusable") progeny of a mature age, will show more moderation in its progress; it will replace brilliancy by solidity, it will contain more of zoölogy than of natural history. It will please less, but it will be more profitable, because it will show a better understanding of the conditions exacted by Nature from those who are eager to apprehend her. One has a difficult task to accomplish if one has had the misfortune to hit upon a zoölogical family of the extent and of the infinite ramifications of which he had, at the outset, no adequate conception! To be able to mark off both ends of one's career by the prosecution and the reproduction of the same work is an advantage that but few naturalists have enjoyed. It is the result of a whole life consecrated to the same study. It bears witness to a rare spirit of patience, of perseverance, and of abnegation, conditions indispensable for such an undertaking. I can therefore affirm, with some pride, that I have resolutely faced all obstacles which I met in the accomplishment of my work."

This effusion of feeling continues for some pages in the same strain, but I have given enough of it to prove the great importance Rob.-Desvoidy attached to the progress achieved in his second work. As I have never paid special attention to the *Muscidae*, I am incompetent to pronounce any opinion upon the reality and upon the extent of this progress. Professor Brauer, in his "Bemerkungen zu Osten-Sacken's Rejoinder," etc. (*Berl. Ent. Zeit.*, 1894, p. 238, at bottom) has this passage: "We have tried to understand his [Rob.-Desvoidy's] descriptions as much as possible. No other dipterologist has made such an extensive use of Robineau's posthumous work as we have, and it seems to us that our work will be of great use for the understanding of Robineau's writings." This may be so, but I have not found in MM. Brauer and Bergenstamm's volumes any general statement about the progress achieved by Rob.-Desvoidy between his two principal works; and such a statement, in this case, was essential in order to prove that Rob.-Desvoidy had told the truth about his progress.

¹ I am translating the peculiar style of Rob.-Desvoidy as faithfully as I can.

Rob.-Desvoidy's work of 1863 was not quite finished when, after his death, it came into the hands of his editors. They have introduced into it some interpolations and changes in order to fill up the blanks they found in it. It belongs to the critic to discern such additions. In my paper, "On the Terms Calypteratae," etc. (in the *Berl. Ent. Zeit.*, 1896, p. 329 and p. 335-336), I have shown that the term *Acalypterata*, introduced in the posthumous work on page 81, was an interpolation of the editors, and that it has never been used by Rob.-Desvoidy in his works. In examining the same volume since, I found that the generalities concerning the "Entomobies" (p. 86 to p. 91, down to line 9 from top) are word for word reproduced from the "Myodaires" (1830). What follows upon them (lines 10 and 11) is apparently also an interpolation for the purpose of introducing the statement about synonymy in the footnote. The passage reads, "It is unnecessary to add that I have given all possible care to the exactitude of the synonymy." And the ominous footnote, appended to this passage, reads thus: "We shall take care that the gaps left in the manuscript on the subject of synonymy, and which the author's death alone prevented him from filling, are filled in a manner entirely satisfactory to entomologists. Therefore all the descriptions of species without synonymy should be considered as new, and as not having been mentioned anywhere before." It is evident to me that the whole interval between pages 86 and 91 was a blank, left unfilled by Rob.-Desvoidy, and that the editors filled it by simply introducing the corresponding passage from the earlier edition. Rob.-Desvoidy would certainly not have reproduced, word for word, the same generalities after thirty years of renewed studies! Rob.-Desvoidy's text begins again on page 91, with line 12: "L'historique des Myodaires," etc., and his picturesque style will be easily recognized. Here he gives vent to his wrath against *Meigen*, who, having been influenced by *Macquart*, had entirely ignored Rob.-Desvoidy's existence. I quote this short but pregnant passage:—"Nothing is more pitiable than this elaboration [of Meigen's *Tachinae*, in Vol. VII, 1835], which had no reason to exist ('qui n'avait pas sa raison d'être'). But it was necessary, above

all, to prevent my name from appearing in science ! Special orders had been received from Paris never to quote me !”

My critical review of Rob.-Desvoidy's two principal publications contains data which, I believe, have not been published before, and which nevertheless are indispensable for the proper understanding of these works. The pointing out of the interpolations of the editors of the posthumous work of 1863 will save some trouble to conscientious workers of the future. My detailed study of Rob.-Desvoidy's Prefaces has convinced me that this author does not deserve the reproach often urged against him of having neglected the work of his predecessors, Fallén and Meigen. He had done what he could to study and assimilate their publications, especially Meigen's, but he is not to be blamed if he failed to achieve the impossible.

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386 At the end of my paper, "Two Critical Remarks," etc. (*Berl. Ent. Zeit.*, 1893, p. ~~330-336~~), I have given a biographical notice of Rob.-Desvoidy. The last paragraph of this paper, beginning with the words, "Rob.-Desvoidy makes on me the impression," etc., must be cancelled, because the disparaging remark on his character which it contains is unfounded, and was based upon my, at that time, rather superficial knowledge of his works.

XXIV AËRIAL AND TERRESTRIAL DIPTERA

An attempt to define the adaptive structural modifications produced by the two life-habits of Diptera

A first attempt at a definition of the adaptive characters peculiar to the two life-habits of Diptera was made by me in my publications on Chaetotaxy ("Noch ein Paar Worte über Chaetotaxie," etc., *Wiener Ent. Zeit.*, 1882, p. 91 ; 85, 1882 ; "An Essay on Comparative Chaetotaxy," etc., *Trans. Ent. Soc.*, London, 1884, p. 499-502 ; 102, 1884). The principal application, however, of this contrast, as a new aid towards the classification of Diptera, I introduced later, in my "Preliminary Notice of a Subdivision of the Suborder Orthorrhapha Braehyeera on Chaetotactic Principles" (*Berl. Ent. Zeit.*, 1896, p. 365-373 ; 158, 1897).

The contrast between the two types of aërial and of terrestrial life-habit exists among all the groups of Diptera, although it is more

marked among the *Brachycera*, where the corresponding two types of flies are distinguished by the presence or absence of holoptic heads and of macrochaetae. As in the *Nemocera vera* these two characters are wanting, the contrast between the two types is less evident, nevertheless (as I am going to show in the course of this paper), it exists; but, in the absence of holoptic heads and of macrochaetae, the contrast is characterized by modifications in the structure of other organs, such as antennae, wings, legs, etc.

The modifications of the different organs in accordance with the requirements of one or the other life-habit are merely adaptive and, although they may be very prevalent in a group, and characteristic of it, they are not very deep-seated, and may change or disappear among those of the members of the same group the life-habit of which is aberrant from the prevailing type. The phylogenetic characters, in such cases, are left untouched, and the aberrant members of the group remain in the same family, or superfamily, in spite of their sometimes quite conspicuous adaptive characters. Such adaptive characters may therefore be considered *as tools in the battle of life*, tools which are easily modified, when the conditions of the struggle are altered, although the phylogenetic characters remain the same.

Adaptive structures are useful either for *orientation* (such are the adaptations of the eyes, of the antennae, or of the macrochaetae) or for *action* (the adaptations of the wings or legs). In some cases, organs of orientation may be developed in such a manner as to be also useful for action; for instance, in certain *Nemocera* long antennae may be used, like the long legs, for regulating the flight.

The males of *aërial* Diptera, owing to their holoptic heads, are better able to scan, during high flight, a wide horizon. Aërial Diptera show a minimum of macrochaetae, sometimes none whatever, because the requirements of orientation are less complex for aërial than for terrestrial life. *Holopticism* is usually connected with a wing-structure that is especially adapted for a more perfect control of the motions; and this faculty of control, in such cases, is not confined to the holoptic male, but exists in both sexes. Such a wing-structure enables the male to hang, poising in the air,

and the female (although dichoptic) to hover over a flower, even to touch it, without alighting. The legs of aërial Diptera are principally fitted *for alighting*, and much less for any other purposes; they are, for this reason, less strong and less specialized than those of the terrestrial type.

Terrestrial Diptera show the opposite characters. The *heads in both sexes* are generally *dichoptic*. The macrochaetae are much more numerous and stronger. The flight is more headlong, less subject to control. The legs are fitted for running, grasping, digging, and other exercises, and are for this reason stronger and provided with bristles, hairs, and other useful and sometimes ornamental appendages.

Little has been done as yet to establish a causal connection between the different modes of flight of Diptera and the mechanism of their wings. I have attempted to pave the way for such researches in my two papers, "On the Terms Tegula, Antitegula, Squama, and Alula" (in the *Berl. Ent. Zeit.*, 1896, p. 285-288), and "On the Terms Calypteratae and Acalypteratae, Calypta and Calyptra, as they have been used in Dipterology" (in the same serial, 1896, p. 285-288). But these papers of mine were merely a beginning of such an investigation, and especially a contribution to the nomenclature and literature of the subject.

On the relation between the venation and the mode of flight, a happy hint has been thrown out by Zeller, more than sixty years ago, in the following passage: "The mode of flight of the true *Bombylius* is not unlike that of *Syrphus*. They hover for some time in the same place, jerk off suddenly, and then gradually sink, and thus approach the flowers they intend to visit." Such a control of the power of flight is secured in both of these genera by a similar structure of their venation; the fourth and fifth veins, just before reaching the margin of the wing, form an angle, the distal sections of which run parallel to the margin, and end in the preceding vein. A similar parallelism of veins with the margin secures the same control of the motions of the *Mydaidae* and the *Nemestrinidae*. But the mechanics of many other peculiar modes of flight of certain Diptera still remain unexplained. On what structures, for instance, of wings or veins, depends the singular to

and fro pendulum-motion of the *Homalomyiae* in our rooms? Or the perpendicular up and down motions of *Rhyphus*, *Hybos*, and *Phora*? In this respect there is much room for investigation yet.

Having done with the generalizations on my subject, I proceed now with their application in the different groups and families of Diptera.

I begin my exposition with the *Brachycera*.

In each of the superfamilies of the *Brachycera*, one of the above-defined types is prevalent. But at the same time, as I said above, each of the superfamilies contains exceptional forms, showing a distinct tendency towards the life-habits of the opposite type. I repeat, that in such cases the modification of structure runs parallel with a change of life-habit: a Dipteron, although belonging to a prevaillingly *aërial* superfamily, assumes under such a change of circumstances some of the characters of *terrestrial* Diptera, and *vice versa*.

The most numerous and thriving family among the *Tromoptera*, the *Bombyliidae*, are principally *aërial*; they have holoptic heads in the male, well-developed wings, a remarkably regulated flight, comparatively weak legs, and a moderate development of almost imperceptible macrochaetae. But among the *Bombyliidae* there is the heavy *Toxophora*, a *pedestrian* among *aërial* forms, with comparatively short wings, but stout legs, and showing an unusual development of stout macrochaetae on the thorax, and even a pair of ocellar bristles on the head. In complete contrast with *Toxophora* is another Bombylid, *Systropus*, *aërial* to the core, slender in shape, entirely bare of hairs and bristles, but, as if in compensation, holoptic (I may say) with a vengeance, because the eyes offer the rather rare instance of a complete contact of the eyes in both sexes. (Compare my paper 158, 1897, "Preliminary Notice," etc., *Berl. Ent. Zeit.*, p. 368-369, for details about *Bombyliidae* and the exceptional position, among the *Tromoptera*, of the small and specialized families of *Nemestrinidae* and *Cyrtidae*.)

The superfamily *Energopoda* are essentially *pedestrian*, and hence the species are usually *dichoptic* in both sexes; they have abundant macrochaetae and strong, variously specialized legs. But

among them the *Empidæ*, although the number of their macrochaetae, the development and the remarkable specialization of their legs, and other characters, prove them to be properly *pedestrian*, have *aërial* habits, which bring along with them the frequent contact of the eyes in the males and, in some genera (as *Hybos*, *Syneches*, *Cyrtoma*), even holopticism in both sexes. Among the *Empidæ* themselves, however, exceptions in the opposite, *terrestrial*, sense occur (*Hemerodromia*, *Clinocera*, *Tachydromia*), and in these genera diehopticism reappears in both sexes. *Hilara*, in this respect, is singular; although aërial in its habits, it is diehoptie in both sexes.

Of the other families of the *Energopoda*, the *Asilidæ*, so far as I know, are diehoptie without exception, and the *Dolichopodidæ* likewise, with rare exceptions (*Diaphorus*).

The third superfamily of the *Orthorrhapha Brachycera*, the *Eremochaeta*, afford a peculiar interest, as they are entirely wanting in macrochaetae. This want seems to be amply compensated by the great development of a variety of structures of the antennae. No other superfamily offers anything equal to it, and this peculiarity I have characterized as *morphological restlessness* (*Berl. Ent. Zeit.*, 1892, p. 427; **130, 1892**).

Another peculiarity of the *Eremochaeta* is, that they contain a considerable number of *archaic* forms. Like the *Marsupialia* among the Vertebrata, the *Eremochaeta* seem to represent a collection of survivals of bygone zoölogical horizons. And this generalization is confirmed by the fact that genera of this superfamily are more abundant in countries the whole fauna of which is of a more ancient origin than the European fauna (North America, Chili, Australia).

For a detailed characterization of the *Eremochaeta*, I refer to my above-quoted paper (**130, 1892**). At present I shall confine myself to some generalizations tending to show that, although the great prevalence of holopticism and the total absence of macrochaetae make of the *Eremochaeta* aërial Diptera, the terrestrial and pedestrian tendencies which occur among some of them are indicated by the very same characters that distinguish terrestrial and pedestrian forms in other superfamilies: the reappearance of diehop-

tic heads in the male sex and the specialization of the legs. I begin with the family *Stratiomyidae*.

In the section *Beridina* we have the European *Actina* and the Australian *Exaireta*, both with dichoptic eyes in the male sex; while the closely allied genera (*Beris*, *Allognosta*, etc.) have holoptic males. In *Exaireta* the pedestrian tendency is distinctly indicated by the development of the legs.

In the section *Sargina*, the genus *Histiodroma*, described by Schiner ("Novara," p. 68, tab. II, f. 8), has a broad front in both sexes. He places it in a separate section, *Raphiocerina*. Not having had an opportunity to examine specimens, I cannot judge whether the dichoptic male of *Histiodroma* shows, besides this, any other pedestrian tendency.

In the section *Hermetina*, the genus *Hermetia* shows a broad front in both sexes, while its Australian close relative *Massicyta* Walk. (*Lagenosoma* Brauer, "Notacantha," p. 81), according to Brauer, Walker had only females, has an holoptic male, which proves again that holopticism in this case, as in many others, is not a deep-seated character.

In the section *Stratiomyina* (*Odontomyina* Loew) I am not aware of any exception to the rule of male holopticism. In some genera only (*Euparhyphus*, *Lasiopa*) the eyes of the male are subcontiguous.

Among the *Pachygastrina* subcontiguous eyes likewise occur; but I find only *Chauna* Loew, from Cuba, which is described as having, in both sexes, "a broad, smooth front."

In the family *Tabanidae* I have not discovered any exception to the rule of holopticism; it may occur nevertheless, as many genera and species are described in the female sex only; and as, in some generic descriptions, the holopticism of the male is not distinctly mentioned, and probably taken for self-understood.

It is in the predaceous family *Leptidae* that a number of genera assume, in their general appearance, as well as in the details of their organization (in a dichoptic head in both sexes; in the structure of eyes and antennae; in their slender body and legs), a striking resemblance to the *Asilidae*, mimicking them in such a degree that several of them were actually placed in that family by otherwise experienced entomologists. The Leptid *Pheneus* was

described by Walker as an *Asilid*, and Westwood, who drew the figure (in the "Insecta Saundersiana"), evidently shared this error. *Lampromyia* Maeq., a Leptid, was redescribed by Perris as a new genus of *Asilidae*, *Apogon*. The genus *Leptynoma* Westwood (from the Cape), described by him as an *Asilid*, is either the same as, or closely allied to, the Leptid *Lampromyia* Maeq.

I have compared the *Eremochaeta* with the *Marsupialia* among the Vertebrates. The *Marsupialia* contain animals foreshadowing in their general appearance more recent forms. The above-mentioned group of genera of *Leptidae*, mimieking *Asilidae*, seem to represent an analogous case, and a further study of the remarkable superfamily *Eremochaeta* may disclose still other resemblances of the same kind.

Another very interesting instance of the juxtaposition in the same group of two opposite types of life-habit is likewise found among the *Leptidae*, in the genera *Coenomyia* and *Xylophagus*. Although the imagos show important differences in the head, antennae, scutellum, venation, etc., the comparison of the larvae proves that the two genera are very closely related, and do not belong to two different families, as was formerly believed (by Schiner, "Fauna," Vol. I, p. 26, and by myself, in "Catalogue of North American Diptera," 1878; Loew placed them in the same family, but with a doubt, "Monographs North American Diptera," Vol. I, p. 16, 1862). *Coenomyia* is holoptic in the male, *Xylophagus* distinctly diehaptic in both sexes, and, in agreement with these characters, *Coenomyia* has more aerial habits and flies better than the pedestrian *Xylophagus* (Schiner, *loc. cit.*, says of the latter: "finden sieh an Baumstämmen . . . wo sie munter auf und ab rennen"). In Europe, there are no transitions between these two genera, but in North America, with its older fauna, the interval between *Xylophagus* and *Coenomyia* is filled up by the genus *Arthropeas* Loew, and several allied genera.

The case of *Xylophagus* and *Coenomyia* reminds me of the two species of the coniferous tree *Sequoia* in California, *S. sempervirens* and *S. gigantea*, the former occurring along the seashore, the other high up in the mountains. Prof. Oswald Heer of Zurich, the great student of fossil plants, enumerated twenty-four fossil *Sequoiae*, occurring in Tertiary and Cretaceous strata and spread over Europe, Asia, and North America, as far north as Spitzbergen (78° of latitude), and as far south as

Spezia and Sinigaglia in Italy. These fossil species represent two extreme types, the interval between which was filled up by numerous transitional forms of the Tertiary period. The two Californian species, the only living remains of the genus, are representative forms of those two extreme types ("es sind zwei sehr scharf geschiedene Typen," says Heer), all the intervening genera and species in the three parts of the world being extinct.

Bold as the juxtaposition may seem of these two cases of the animal and the vegetable kingdom, it may nevertheless serve as a well-founded illustration of the working of the law of evolution. (About *Sequoia* my source of information is O. Heer, "Ueber die Sequoien," Vortrag in der bot. Section d. Schweiz. Nat. Gesellsch., reproduced in the periodical *Gartenflora*. My separate has no date, but it cannot be later than 1879.)

The *Mydaiidae*, an ancestral superfamily among the *Orthorrhapha Brachycera*, seem to hold an intermediate position between the aërial and pedestrian types. They are dichoptic in both sexes, and most of them have well-developed legs; on the other hand they have macrochaetae, and their venation, with the curved veins running parallel to the hind margin, indicates a superior power of regulating their flight. My experience with living *Mydaiidae* is confined to *Leptomydas pantherinus* Gerst., which I have watched in 1876 about Lone Mountain, in San Francisco. As far as I remember, its flight was rather slow, steady, and well-regulated, not far above the soil, and not unlike that of a *Bombylius*.

The two types of life-habit exercise the same influence on the organization of the *Cyclorrhapha Athericera*, as they do on that of the *Orthorrhapha Brachycera*. Representatives of the former sub-order are subject to similar modifications of structure in consequence of their adaptation to external influences.

The *Syrphidae* are pre-eminently aërial and holoptic, showing a minimum of macrochaetae, and possessing a remarkable power of regulating their flight. At the other end of the series of *Cyclorrhapha*, the so-called *Acalyptrata* are decidedly terrestrial and pedestrian, with dichoptic heads in both sexes, more or less numerous and characteristic macrochaetae, a diminished power of flight, etc. Between the two extremes of *Syrphidae* and *Acalyptrata* are the *Calyptrata* or *Muscidae*, which show a prevalent terrestrial and pedestrian character in the abundance of macrochaetae, in the dichoptic heads in both sexes, and in the headlong character of their flight. But among the *Calyptrata*, the group of *Anthomyiidae*

plays a part not unlike that of the *Empidæ* among the *Energopoda*: although terrestrial in their general appearance and their leading characters, they display an aërial tendency in their power of regulated flight, necessary for their aërial dances, and in the structure of the head in the male, which I have called *pseudoholoptic* (*Berl. Ent. Zeit.*, 1896, p. 367) in order to distinguish them from genuine holoptic heads.

To return to the aërial *Syrphidæ*, there are forms among them which show a *pedestrian* tendency in the use and development of their legs. Such are *Xylotæ* running upon leaves with their stout hind legs, and showing a minimum of contact of the eyes in the male. *Syritta*, a relative of *Xylota*, is seen rummaging with its strong hind legs among stamens of flowers; it also hovers around them, but is not a high flier; it does not scan a wide horizon, and therefore is not provided with an holoptic head. Other *Syrphidæ* exhibit the same pedestrian and terrestrial tendency in the development of comparatively numerous macrochaetae. Conspicuous among these is the genus *Hammerschmidtia* (allied to *Brachyopa*) which has the aspect of a pedestrian fly and resembles an *Helomyza*; the macrochaetae, for a Syrphid, are unusually abundant, and the contact of the eyes in the male is reduced to a minimum.

Male holopticism, although very common among *Syrphidæ*, is, in this family (as well as in others), not a very deep-seated character. Some phylogenetically closely allied genera differ from each other in regard to holopticism. The connection between this character and the mode of flight is, in some cases, not easy to explain. We can readily understand the correlation between the head of the heavy *Microdon*, dichoptic in both sexes, and its short wings; both characters are indicative of a Dipteron that is not a high flier. But it is more difficult to interpret *a priori* why most *Helophilæ* (including the genus *Dolichogyna* Maeq. from Chili), why *Ascia*, *Sphagina*, *Psarus*, *Pelecocera*, etc., are likewise dichoptic in both sexes. Observation in life alone will solve such questions.

The *Nemocera vera* are essentially aërial. But as holoptic heads do *not* occur among them, the influence exercised by their life-habit on their organization does not appear in the structure of the head, but in that of other organs, and especially of the antennae, the

principal organ of orientation in this superfamily. As if in compensation for their small heads and eyes, the *Nemocera vera* show a large development of the antennae. It is in them, and not in the conformation of the eyes, that the cephalic secondary sexual character finds its expression (130, 1892, p. 434). We have seen that, among the *Diptera Brachycera*, the holoptic head of the male is generally an index of the power of regulating flight. Among the *Nemocera vera*, in the absence of holoptic heads, it is the *long antennae* and the *long legs* that regulate flight. The antennae, in such cases, besides their function as organs of orientation, make themselves useful as organs of action (for this distinction, compare above, p. 193). All the dancing and swarming *Nemocera vera* have either long or otherwise much specialized antennae, and, at the same time, long legs (*Culicidae*, *Chironomidae*, *Tipulidae*).

Although *Culicidae* are prevailinglly aërial, their relatives, the *Psychodidae*, with their shorter and stouter body and comparatively short legs and wings, have the habits of pedestrian Diptera. And, indeed, I have observed small *Psychodae* running round in a circle (on a glass-pane) with a remarkable rapidity.

Among the *Chironomidae*, which are generally aërial, some *Ceratopogons* possess decidedly terrestrial characters in their comparatively short and strong legs, their femora armed with spines or bristles, and their tibiae and tarsi displaying a variety of adaptive structures.

Among the *Nemocera vera*, the majority of which, as we have seen, are aërial, the *Mycetophilidae* are remarkable for their terrestrial and pedestrian habits. Rob.-Desvoidy, in his picturesque style, described them well as "the timid species living under the protection of rotten mushrooms" (Rob.-Desvoidy, "Myodaires," 1830, Preface). Their antennae have no verticils, and are of a simple structure in comparison with that of the antennae of other *Nemocera vera*; their legs are stouter and stronger. Nevertheless, among these terrestrial *Mycetophilidae* there are genera which preserve the generally aërial character of the superfamily *Nemocera vera*; *Macrocera* and *Bolitophila*, with their long legs, their long, filiform antennae, and their slender, elongate bodies, have the appearance of *Limnobiae* and share their aërial habits.

The terrestrial *Mycetophilidae* and the aërial *Cecidomyiidae* offer in their general appearance a complete contrast, although phylogenetically they are rather closely allied.

Among the aërial family *Tipulidae*, the apterous *Chionea*, with its stunted antennae and stout legs, affords a very instructive example of a terrestrial and pedestrian Tipulid.

The *Nemocera anomala*, which I consider as ancestral forms, are often provided with holoptic heads in the male (differing in this from the *Nemocera vera*, where such heads never occur). This seems to prove that holopticism among Diptera is a character of very old standing. The males of *Bibio* are holoptic and aërial. *Plecia* (including *Penthetria*) is more terrestrial in its habits, and therefore shows a minimum of holopticism in the male (sometimes distinctly separated eyes), and, in agreement with this character, the wings are often reduced in size. It has been noticed that among fossil remains different forms of *Pleciae* abound, while a true fossil *Bibio* has not been discovered yet. This may be due to the aërial habits of the male *Bibio*, which saved it from submersion.

In the *Simuliidae* the male is holoptic and aërial; the female dichoptic and terrestrial, and shows a peculiar development and activity of the legs (130, 1892, p. 452, footnote). This is a singular instance of a disseverance of sexes. The males swarm and dance high in the air; the females occur in myriads on riversides, and torment men, cattle, and horses. It is not astonishing at all that Schönbauer (1795), author of a monograph of the celebrated "Columbatez" *Simulium*, never met with a male of this species, and mistook some females for it. Skuse's statement, that in Australia *Simuliae* seem to be "rare and local" (130, 1892, p. 454), requires confirmation. Prof. L. C. Miall ("Aquatic Insects," p. 188, 1895) says just the contrary: "*Simulium* have greatly annoyed certain Australian exploring expeditions."

Rhyphus has an holoptic male, but in his American relatives, *Lobogaster Philippi* and *Olbiogaster* O. S., the male is dichoptic. As we have no data about the life-habits of the last two genera, we do not know how far this difference of structure corresponds to a difference of life-habit.

The *Blepharoceridae*, although pre-eminently aërial, offer the peculiarity of having the eyes of a nearly identical structure in both sexes. That is, in different genera, the eyes in both sexes are either holoptic or dichoptic. We do not know yet whether this difference is connected with a difference in the behavior of these genera. The eyes of this family offer some other exceptional and unexplained structures (including bisection).

The *Orphnephilidae*, provided with holoptic heads in both sexes, have the characters of aërial Diptera, although they do not fly very high. Their flight is rather slow, and appears to be well under their control, as becomes holoptic Diptera.

To conclude: The study of the two types of life-habit of Diptera, in connection with corresponding modifications in their structure, of which I have given a very imperfect outline, I consider as a necessary complement of the study of Chaetotaxy as a method of description. Investigations in that direction will, at the same time, open new avenues for the inquiry into the evolution of Diptera during geological ages. A hint of this kind is introduced by me in this chapter in the paragraph on *Xylophagus* and *Coenomyia*, two apparently very different types, the phylogenetic relationship of which is, however, explained by their evolutionary history.

In my "Introduction" (p. 22) I expressed regret that I had "never had the good fortune to reside for any sufficient length of time in a place in which, or within a reasonable distance of which, there was a large dipterological collection," containing forms of all continents; and it is in preparing the present chapter, replete as it is with generalizations, that I feel how much I have lost by such an isolation, and how much more satisfactory my work would have been if I had been able to illustrate it by numerous examples of exotic forms.

Before concluding this chapter I deem it useful to state distinctly that, although a mere dilettante, I firmly adhere to that group of naturalists who believe all phenomena of life and organic growth to be susceptible of being *described*, but not *explained*. For this reason in my "Introduction" (p. 15) I compared the evolution of organic forms with the "Growth of the tree of Life," because

comparison is a mode of description, and not an explanation. I said: "What is Life? On this question I adhere, at least in the present state of our knowledge, to the humble avowal of Prof. Du Bois-Reymond: 'Ignorabimus'!"

The distinguished French thinker Rivarol (1754-1801), in his "Maximes et Pensées," has formulated the same idea in the following sentence:—

"Dès qu'on a nommé la nature, il n'y a plus problème, mais mystère; il ne s'agit plus d'expliquer, mais d'exposer."

CHARLES ROBERT v. D. OSTEN SACKEN

AT THE AGE OF 74 (IN AUGUST 1902).

RECORD OF MY LIFE WORK IN ENTOMOLOGY.

PART THIRD.

LIST OF MY ENTOMOLOGICAL PUBLICATIONS FROM 1854 TO 1904.

FIRST PERIOD.

WORK IN ST. PETERSBURG. 1854—1856.

1854.

1. Dipterologisches aus St. Petersburg.

Stett. Ent. Z. 1854, p. 203—204, w. a. plate.

1. Proposal for a new classification of the *Tipulidae brevipalpi*, based upon a detailed study of their male genital organs. These ideas were carried out by me in my later publications of 1859, 1868 etc.

2. Detached remarks on other *Limnobia*, especially about a remarkable species discovered by me near St. Petersburg, which Loew described under the name of *L. imperialis* n. sp. but which, ultimately, turned out to be the *Tipula annulata* Linné. Its congener, *L. caesarea* O. S., I discovered and described at the same time.

1857.

2. Ueber *Tipula annulata* Linn.

Stett. Ent. Z. 1857, p. 90—91.

During a visit to London in 1856, I found in the original collection of Linné, preserved in the Linnean Society (Burlington House) that the species which Loew had described as *Limnobia imperialis* n. sp. (compare above sub No. 1, 1854) was identical with the long-misunderstood *Tipula annulata* Linné.

In 1868 I established the new genus *Trochobola* for *T. annulata* and *L. caesarea* O. S. *Trochobola* is a very peculiar form, with a wide distribution over Europe, North America, Australia and Tasmania, all the species showing the same pattern of ocellate spots on the wings (compare Mon. N. A. Diptera IV, p. 97—98).

3. (In Russian.) Otcherk sovremennago sostojanija poznanija entomologitcheskoi fauny okrestnostei St. Peterburga.

In the "Jurnal Ministerstva Narodnago Prosvestchenija", St. Petersburg, 1857.

(Translation): Review of the present condition of our knowledge of the Entomological Fauna of the environs of St. Petersburg, 1857.

NB. The separate copies bear the date of 1858, and contain 166 pages. This paper was rather carelessly printed in St. Petersburg after my departure for the United States, and contains innumerable misprints.

(Biographical.) Of the beginnings of my entomological career I have given an account in the "Introduction", p. 1—2.

In the early spring of 1856, I was appointed Secretary of the Russian Legation at Washington and landed in New York about the middle of June. Of my peregrinations during the two months intervening between these two dates I have given an account in the same Introduction, p. 2—3. (Continued on p. 207.)

SECOND PERIOD.

WORK IN THE UNITED STATES.

1856—1877.

1858.

4. Catalogue of the described Diptera of North America.

Smithsonian Miscell. Collections, Vol. III, Jan. 1858; pp. XX and 192, in 8°. — *Appendix*, *ibid.* Oct. 1859 (three pages).

NB. This is a mere compilatory Catalogue which, twenty years later, was replaced by the critical Catalogue of 1878 (compare below, No. 61, 1878).

1859.

5. Ueber die St. Petersburger Insektenfauna.

Wien, Entom. Monatschr. 1859, p. 50—56.

NB. A short notice of the contents of my No. 3, 1857, which had been published in Russian.

6. Notes furnished by me for Dr. Le Conte's edition of "*The complete writings of Thomas Say on the Entomology of North America*". New York, 1859.

NB. Dr. Le Conte, in his Preface, makes mention of these Notes on Diptera, as well as of those by Mr. Philip Uhler on *Orthoptera*, *Hemiptera* and *Neuroptera*.

7. New genera and species of North American *Tipulidae* with short palpi, with an attempt at a new classification of the tribe.

Proc. Acad. Nat. Sc. Philad. August 1859, p. 197—255, w. two plates.

NB. A short notice of this publication will be found in the Stett. Ent. Z. 1860, p. 87—90.

(*Biographical.*) In the summer of 1859 I made a journey to Europe, and visited the principal Museum-collections in different cities. The winter 1859—1860 I spent in St. Petersburg, and returned to the United States in the spring of 1860. (Continued on p. 209.)

1860.

8. Appendix to the paper entitled: *New genera and species of N. A. Tipulidae w. short palpi* etc. (see above, No. 7, 1859).

Proc. Acad. Nat. Sc. Philad. Jan. 1860, p. 15—17.

1861.

9. Description of nine N. Am. *Limnobiaceae*.

Proc. Acad. Nat. Sc. Philad. Sept. 1861, p. 287—292.

10. Entomologische Notizen.

Stett. Ent. Z. 1861, p. 51—52; continued 1862, p. 127—128.

11. Ueber die Gallen und andere durch Insekten hervorgerach-
te Pflanzendeformationen in Nord-Amerika.

Stett. Ent. Z. 1861, p. 405—423; continued 1862, p. 80.

12. (Without title.) Communication concerning the sexes of *Cynips*.

Proc. Acad. Nat. Sc. Philad. July 1861; *ibid.* Sept. 1862, p. 249.

13. On the *Cynipidae* of the N. Am. Oaks and their Galls.

Proc. Entom. Soc. Philad. I, 1861, p. 47—72.

1862.

14. Descriptions of some larvae of N. Am. *Coleoptera*.

Proc. Entom. Soc. Philad. I, 1862, p. 105—130; w. a plate.

(A short Additional Notice, *ibidem* IV, 1865, p. VIII—IX, refers to the *luminous* larvae of different kinds, described and figured in Vol. I, p. 125—130. These larvae proved afterwards to belong to different genera of *Lampyridae*; see below, No. 29, 1868.)

15. Notes on the Diptera in the third edition of T. W. Harris's "*Treatise on some of the Insects injurious to Vegetation*". Boston, 1862.16. Characters of the larvae of *Mycetophilidae*.

Proc. Entom. Soc. Philad. I, 1862, p. 151—162, with a plate.

NB. I have published a reprint of this paper, with some additions in Heidelberg, in 1886. Comp. No. 113, 1886 of this List.

17. (Without title.) Notice on a new character, separating the *Tipulidae brevipalpi* from the *longipalpi*, and introduction of a *third* division of the *Tipulidae* of the same importance with the two others: the *Ptychopterina*.

This appeared in a footnote to Loew's: "Sketch of the Systematic arrangement of Diptera" etc. in the *Monographs of N. A. Diptera*, I, p. 11—13, 1862.

18. On the N. Am. *Cecidomyiidae*.

In Loew and O. Sacken's *Monographs of N. Am. Diptera*, 1862, I, p. 173—205, with a plate and woodcuts.

19. Additions and Corrections to the paper entitled: "On the *Cynipidae*" etc. (see above, No. 13, 1861).

Proc. Entom. Soc. Philad. I, 1862, p. 241—253.

1863.

20. *Lasioptera* reared from a gall on the golden-rod.

Proc. Entom. Soc. Philad. I, 1863, p. 368—370.

21. Contributions to the natural history of the *Cynipidae* of the United States and of their galls. Article *third* (comp. No. 19, 1862).

Proc. Entom. Soc. Philad. II, 1863, p. 33—49.

1864.

22. On the Diptera of the Amber Fauna.

Silliman's American Journal of Science and Art, Vol. XXXVII, 1864, p. 306—324.

NB. This is a translation of Loew's paper on the same subject, read at the meeting of the German Scientific Association at Königsberg (Prussia) in 1860. To this translation is added a "List of the Diptera common to Europe and N. America" prepared by Loew for this translation.

23. Description of several new North American *Ctenophorae*.

Proc. Entom. Soc. Philad. III, 1864, p. 45—50.

24. Ueber den wahrscheinlichen Dimorphismus der *Cynipiden*-Weibchen.

Stett. Ent. Z. 1864, p. 409—413.

NB. Brief account of the hypothesis of B. D. Walsh, expounded by him in the Proc. Entom. Soc. Philad. March 1864 (but which, in the end, proved fallacious).

1865.

25. Contribution to the Nat. Hist. of the *Cynipidae* of the United States and of their Galls. Article *fourth* (comp. above, No. 21, 1863).

Proc. Entom. Soc. Philad. IV, 1865, p. 331—380.

26. Description of some new Genera and Species of North Am. *Limnobia*.

Proc. Entom. Soc. Philad. IV, 1865, p. 224—242.

(*Biographical.*) After the Smithsonian conflagration, in Jan. 1865, which destroyed the manuscript of my "Monogr. of the *Tipulidae brevipalpi*" (comp. this "Record", p. 73), I made in summer a journey to Europe, visited many Museums and dipterologists, and thus collected very useful materials for re-writing the lost manuscript. At the end of June, I visited Loew in Meseritz. In Turin, I made the acquaintance of Professor Luigi Bellardi. Born in Genoa in 1818, he became Professor of Natural History in Turin, and died there in September 1889. His principal work was in *palaeontology*, but dipterology was his favorite pastime. A diligent collector of diptera and a charming man! His "Diptera Mexicana" was his only publication on this Order.

In the same year 1865 I returned to the United States late in autumn. The cholera having broken out in Paris about that time, the steamer, to the great annoyance of the passengers, was detained in quarantine for three days in the bay of New York (continued on p. 210).

1866.

27. Two new North American
- Cecidomyiæ*
- .

Proc. Entom. Soc. Philad. VI, 1866, p. 219—220.

1867.

(*Biographical.*) I spent the winter of 1867—68 in rewriting the manuscript of Vol. IV of the Monographs etc. which had been burned (compare above, No. 26, 1865). The new manuscript was delivered to the Smithsonian Institution in May 1868, and went through the press during the summer and autumn of the same year. The title-page is dated January 1869; see below, No. 30 (1869). (Continued on this page, below.)

1868.

28. Description of a new species of
- Culicidæ*
- (
- Aedes sapphirinus*
- n. sp. — Middle States).

Trans. Amer. Entom. Soc. II, 1868, p. 47—48.

29. (Without title.) Note on certain luminous larvae.

Canad. Entomologist, I, p. 38 (only half a page).

NB. Larvae of *Phengodes* (compare above, No. 14, 1862).

1869.

30. On the North American
- Tipulidæ*
- . Part first:
- Tipulidæ brevipalpi*
- .

Loew and Osten Sacken's "Monographs of N. Am. Diptera, Vol. IV, Washington, Jan. 1869. Pages XI and 345; four plates.

NB. This is the reproduction of the volume the manuscript of which was consumed in the Smithsonian conflagration of 1865. The at first intended Part II, about *Tipulidæ longipalpi* was given up, because Loew had undertaken the task of describing a large number of species of this group in his *Centuries* (1861—1872). Nevertheless later, during my residence in Europe, I published the paper: "Studies on *Tipulidæ*, Part I, Review of the published Genera of the *Tipulidæ longipalpi*". See below, No. 115, 1886.

31. Report on North American Dipterology in 1868.

Prepared for A. S. Packard's: "Record of American Entomology for 1868", p. 18—25.

32. Biological notes on Diptera (galls on
- Solidago*
-).

Trans. Amer. Entom. Soc. II, 1869, p. 299—305.

(*Biographical.*) In the spring of 1870 (April 23) I again sailed for Europe and landed in Bremen (May 5). I made a tour of visits to several scientific men

and institutions: Prof. Rosenhauer in Erlangen, Professors Siebold and Gemminger in München (June 3—9), R. Schiner and Dr. Frauenfeld in Vienna (June 9—15), the Museum in Berlin (August 10—12). As the Franco-German war had broken out at that time, I felt it my duty to return to my post of Consul-General in New York, which I reached Sept. 6 1870. The news brought by the pilot at our arrival was that of the surrender of Sedan! (Continued on p. 212.)

1870.

33. On the transformations of *Simulium*.

Amer. Entomologist and Botanist, II, p. 227—231; 1870, w. woodcuts.
NB. A popular exposition of the subject, with but a few original observations.

34. Biological notes on Diptera. Article second (comp. No. 32, 1869).

Trans. Amer. Entom. Soc. III, p. 51—54; March 1870.

35. Contributions to the Natural History of the *Cynipidae* of the United States. Article *fifth*; compare No. 25, 1865.

Trans. Amer. Entom. Soc. III, p. 54—64; 1870.

36. Report on American Dipterology in 1869.

Prepared for A. S. Packard's "Record" etc. for 1869, p. 30—37
(compare No. 31, 1869).

37. Letter to the Trustees of the American Museum of Natural History in New York, dated Jan. 21 1870.

Printed in the *First Report* of this Museum.

NB. In this letter I informed the Trustees of the gift I had made for the projected Museum of *my general collection* of North American insects of *all orders* (excepting *Lepidoptera*), which I had formed during my residence in the United States. This collection consisted of about 3800 specimens, determined by the best authorities in each Order. My principal aim in making this gift was thus to form a nucleus of a collection of native insects *always open to the public*; a collection in which any schoolboy might determine at sight the more common species. I believe that this my intention was actually carried out in the Museum soon after its erection, but I have not heard of it since. The Diptera were represented by the more common species only, my principal collection of this Order having been deposited in the Museum of Zoology in Cambridge, Mass.

1871.

38. Biological notes on Diptera. Article third (compare No. 34, 1870).

Trans. Amer. Entom. Soc. III, p. 345—347 (Dec. 1871).

1871 (August) to 1873 (September)

Years of travel.

(*Biographical.*) In 1871 I had resigned my post of Consul General of Russia in New York, remaining, nevertheless, in diplomatic service (*en disponibilité*). In this capacity I still had some official duties to perform, which kept me travelling for nearly two years. During these years of locomotion I had many opportunities of visiting Museums and entomologists, as well as meeting my relatives, scattered in different localities. I shall now give a brief account of these peregrinations.

After my resignation, I sailed for Europe (August 5 1871), and, by way of Bremen, came to Berlin, and then to Baden-Baden (the residence, at that time of my mother); then to Switzerland, where I travelled in different directions through the whole month of September and a part of October, returning finally to Baden-Baden. In November, by way of Frankfurt o/M. and Leipzig, I paid a visit to Loew in Guben (Novbr. 3—5), after that, I visited Warsaw (Novbr. 6—10) and went to St. Petersburg, where I stayed for nearly two months. I left it at end of December, and came to Berlin on the 1st of January 1872. After depositing there an official parcel (with which I had been entrusted by the Foreign Office in St. Petersburg) I continued my journey on the same day towards Baden-Baden, where I spent a week with my mother. Then by way of Paris, I reached Brest, embarked in the "Pereire" and landed in New York (Febr. 4) after a cold and stormy passage. After spending five months in the United States, dividing my time between New York and Cambridge, Mass., I sailed from New York (July 11) by the Hamburg steamer "Silesia", and arrived in Berlin (July 24). Here my official peregrinations came to an end, and I was henceforth free to travel for my own pleasure. I visited successively Denmark (from Kiel, over the Islands Funen and Zealand to Copenhagen, July 26 — August 2); then, by way of Malmö, Lund¹, Gothenburg, Trolhattan, Wenersborg to Stockholm (Aug. 7—11) and Upsala (Aug. 12); from Stockholm by steamer to Lübeck (Aug. 16), by Berlin (Aug. 23—26), Guben (with Loew, August 23—35), München, Salzburg, to Gastein, where I spent three weeks (Aug. 31 — Sept. 21), taking the water-cure, much needed by me after all my exertions. Much restored by Gastein, I made up my mind to spend the winter in Italy. I began with the delightful Riva, on the Lago di Garda (Sept. 27 1872) and remained in Italy till the spring of 1873, visiting successively, and making more or less prolonged residences at Verona, Venice, Padua, Bologna, Ravenna, Lucca, Perugia, Pisa, Florence, Parma, Rome, Naples, Messina, Taormina, Catania, Syracuse, Palermo, Genoa, Turin, Pavia etc. Be-

¹ In Lund, I did myself the honor to pay a visit to Zetterstedt, but did not see him; he excused himself on account of his old age (at that time 87!), but invited me to examine his collection. I did not, however, accept this offer, as it would have detained me too long. At Upsala, I paid a visit to the venerable mycologist Fries, at that time 78, a very amiable old man.

sides sight-seeing, and enjoying the most pleasant society which I was fortunate to meet almost everywhere, I did not neglect to visit my scientific correspondents and to make new acquaintances: Rondani in Parma, Bellardi in Turin, the zoologist Professor Henry Giglioli in Florence, Dr. R. Gestro in Genoa, Prof. Achille Costa in Naples etc. By way of Milan, May 26 1873, I left Italy in the direction of Bozen, and proceeded through Klagenfurth, Buda-Pesth, Vienna (3—15 of June), Ischl, Salzburg, Augsburg, Lindau, to Switzerland (June 26), where I remained, visiting the principal localities, through the whole of July. From Geneva (Aug. 2) I went to Paris (Aug. 3—10) and then by London (Aug. 10—22) to Liverpool, where I embarked once more for New York, landed there on Sept. 3, 1873, and settled in Cambridge, Mass. for the next *four* years, devoting myself to the study of North American Diptera. Professor Louis Agassiz, who greeted me most cordially, was complaining already at that time of the trouble in his head which carried him off in December. During these years of travel, I did not publish anything. In the meantime (in August 1873) I had definitively retired from diplomatic the service.

Although the personal details I have thus given offer but little scientific interest, I have compiled them, not without some trouble, from different sources, and put them on record here for future reference.

In December 1873, the long-expected *third* volume of the Monographs of North American Diptera appeared in Washington. The story of the long delay of this publication has been told by me in this "Record", Chapter X, p. 69. (Continued on p. 215.)

1874.

39. Description of the larva of *Pleocoma* Le Conte.

Trans. Amer. Entom. Soc. V, p. 84—87, September 1874.

NB. Dr. Le Conte gave me this coleopterous larva, which I described at his request (on p. 87, line 10 from top, for *real*, read *seul*). Dr. Gerstäcker contested the view of Le Conte on the systematic location of this insect (Stett. Ent. Z. 1883, p. 436; translated in the periodical "Entomologica Americana" III, p. 202). Le Conte's views were conclusively vindicated by Dr. Horn, in the same periodical, III, p. 233.

40. A List of the *Leptidae*, *Mydaiidae* and *Dasypogonina* of North America.

Bulletin Buffalo Soc. Nat. Hist. October 1874, p. 169—187. Compare No. 45, below.

Additions and Corrections, in the same Bulletin, December 1875, p. 71.

41. Report on the Diptera collected by Lieut. W. L. Carpenter in Colorado during the summer 1873.

In Dr. Hayden's U. S. Geol. and Geogr. Survey of Colorado for 1873. Washington, 1874, p. 561—566.

42. Notice on the Galls collected by Lieut. W. L. Carpenter in Colorado.

In the same publication as above, 1874, p. 567.

1875.

43. Prodrôme of a Monograph of the North American *Tabanidae*.

Memoirs of the Boston Soc. of Nat. Hist. II, 1875—1878.

NB. This "Prodrôme" appeared in *three* parts: Part I, published in April 1875, contains the Genera *Pangonia*, *Chrysops*, *Silvius*, *Haematopota*, *Diabasis*; Part II, published April 20 1876, contains the Genus *Tabanus*; Part III, published in March 1878, gives a Supplement.

The *second* Part of this "Prodrôme" (*Tabanus*) having been printed during my journey to California, some *errata* occur in it, besides those noted on p. 560 of the Supplement.

44. Report on the present condition of the Collection of Diptera of the Museum of Comparative Zoology in Cambridge, Mass.

In the Annual Report of the Trustees of the Museum to the Legislature of the State, for 1874. — Boston, 1875.

45. A List of the North American *Syrphidae*.

Bulletin Buffalo Soc. Nat. Hist. December 1875, p. 38—71. *Corrections*, *ibid.* May 1876, p. 130—131.

NB. This *List*, as well as its companion, the List of *Leptidae*, *Mydidae* etc. No. 40, 1874, above, was published by me principally as specimens of the arrangement I intended to introduce in my new edition of the Catalogue of N. Am. Diptera. Both No. 40 und No. 45 are therefore superseded by the new Catalogue.

46. Note on some Diptera of the Island Guadeloupe, collected by Mr. E. Palmer.

Proc. Boston Soc. of Nat. Hist. XVIII, Oct. 1875, p. 133—134.

47. On the North American species of the Genus *Syrphus* in the narrowest sense.

Proc. Boston Soc. of Nat. Hist. XVIII, Oct. 1875, p. 135—153.

NB. About this paper, and the influence it had on my relations with Loew, compare in this "Record" Chapter XII, p. 89—90.

48. On a supposed case of seasonal dimorphism among Diptera.

Psyche, I, p. 113—115, November 1875.

NB. This is a short account of my remarks on seasonal dimorphism, contained in the preceding paper, No. 47.

49. Three new galls of *Cecidomyiæ*.

Canadian Entomologist, VII, November 1875, p. 201—202.

50. Report on the Collection of Diptera made in portions of Colorado and Arizona during the year 1873.

In Lieut. Geo. M. Wheeler's "Report upon Explorations and Surveys West of the 100th meridian"; V, Zoology, p. 805—807, 1875, in *quarto*.

1876.

51. Report on the Diptera brought home by Dr. Bessels during the Arctic Voyage of the "Polaris" in 1872.

Proc. Boston Soc. of Nat. Hist. XIX, Dec. 1876.

(*Biographical*.) As I said above (p. 213) I devoted nearly *four* years, between Sept. 1873 and June 1877, to the continuation of my work on N. American Diptera. I began it by spending two winters (1873—74 and 1874—75) in working up the Diptera of the Museum of Zoology in Cambridge, Mass. (where I had deposited my collections during my absence) and in preparing materials for the projected new edition of my *Catalogue* (compare in this "Record", my Introduction, p. 7). After finishing this work, I undertook a prolonged journey through the Western States, as far as California, as much for the purpose of seeing the country as for collecting Diptera.

Between December 1875 and September 1876, I travelled through California as far South as San Diego and San Bernardino, spent a couple of weeks on top of the Sierra Nevada (Lake Tahoe, Webber Lake etc.) and, on my return, spent some time in the Rocky Mountains (Colorado Springs, Gray's Peak, Denver etc.). On my return East, in September 1876, I selected Newport, Rhode Island, for my winter quarters. This choice was influenced by the temperate winter-climate and the agreeable, intellectual local society of that place, as well as by the proximity of Cambridge, Mass., where I had left my library. After nearly *six*, rather laborious months, spent in working up the cream of the entomological booty brought home from my Western journey, my manuscript went rapidly through the Government press in Washington, and my "Western Diptera" was issued at the end of April, after which, in June 1877, I departed for Europe, and this time *sans esprit de retour*!

Brief account of my journey to California in 1876.

Leaving Cambridge Mass. on November 27 1875, by way of New York I arrived in San Francisco December 20, after a short stay at Salt Lake City. After spending some time in San Francisco, I went by steamer to Santa Barbara and to Los Angeles, where I made a more prolonged stay

during which I undertook excursions to San Diego and San Bernardino. Here, I had the good chance to make the acquaintance of the enthusiastic botanist J. G. Lemmon. We started together for a very primitive watering-place called Crafton's Retreat, near Milk Creek, from which we had a magnificent view of Mount Sant Bernardino, with its snow-flag (it was in March). For my return journey to San Francisco, I went by land. Leaving Los Angeles at night, on top of a stage-coach, I had a most fatiguing drive of 24 hours up and down hill across the Coast-Range, then through the Mojave Desert (at times with herds of antelopes in sight), over the Tachichi Pass, finally arriving at a place called, if I remember right, *Caliente*, the terminus of the railway then in construction. I took a sleeping-compartment, and arrived at San Francisco next morning. I was much struck by the glory of the meadows through which we passed! Their original green was entirely hidden under a mass of flowers, distributed in large patches of different shades of blue, pink, red, yellow and white. I spent April and May principally in San Francisco and its environs; in June I started for the Yosemite Valley. At that time this excursion was made in a very uncomfortable, usually crowded, stage-coach, starting from the railway station Merced. I and my companion (the then Russian Consul General at San Francisco) hired a private carriage and we travelled at leisure, stopping twice to spend the nights at Mariposa and Clark's Ranch. From the latter place, we made on horseback the excursion up the mountain to the celebrated Mariposa grove of the gigantic *Sequoia*-trees, or Redwoods, as they are called. We saw the celebrated "Big-tree" of about 300 feet in height and 30 feet in diameter. I remained about two weeks in Yosemite Valley, making excursions on foot or on horseback, in different directions, among them to the top of the mountain called "Cloud's Rest", said to be 10,000 feet high. The summit was snow-clad, although it was mid-summer. I returned to San Francisco by another route (Chinese Camp, Stockton etc.), by stage and rail. The heat was intense (I remember, at one of the stations, reading 98° Fahrenheit, at 6 A. M.), but as *dry* heat is not oppressive, and as I kept quiet and did not drink (except a sip of water at rare intervals), I felt quite comfortable, while my fellow travellers, big Californians, drinking all the time, looked very red and seemed to suffer agony.

The rest of June and a part of July, I spent North of San Francisco, principally in Sonoma Co. and on top of the Sierra Nevada, near Lake Tahoe, Webber Lake etc. (8000 feet above sea-level), where I collected a fine booty of Diptera. At the latter place, I remember witnessing a singular natural contrast. In a shady place in the woods I observed a large heap of snow, a relic of the masses which fall in that mountain range in winter, and from which the name of these mountains, Sierra Nevada, is derived. At the same time I could hear a humming-bird (the Californian sp.) humming over my head!

I cannot dismiss the subject of my journey to California, without paying my tribute of deep gratitude for the generous hospitality I enjoyed during that excursion. In many households I spent several days, and was thus enabled to explore the environs to my heart's content. Sherman

P. Stow, with his amiable wife, were my hosts at Goleta, S. Barbara Co.; Mrs. W. Hood, in Guilcos, Santa Rosa Co.; W. D. Bliss, in Petaluma, Sonoma Co.; the family Maillard in San Rafael, Marin Co. All these names are connected in my memory with the brightest recollections of the unrivalled beauties of climate, flora, and nature in general. In San Francisco I enjoyed the society of the entomologists D. H. Behr, of the amiable lepidopterist Henry Edwards, of James Behrens (in Sancelito), of the botanist Henry N. Bolander, and of many other entomologists and men of science. At Los Angeles, I made the acquaintance of a distinguished man, Mr. Hansen, a civil-engineer from Trieste, Austria. He was very useful to me in guiding me during some of my excursions, and pointing out whatever was of interest to see. During one of these excursions he made me visit a colony of Spanish-Mexicans, not far from Los Angeles, where he spent several days in surveying a property which had to be divided between a number of heirs. Here I had the opportunity of witnessing an original spectacle, a Mexican horse-race. At that time I also visited *Anaheim*, a German colony, founded by a number of well-to-do Germans who had emigrated from Europe during the stormy year of 1848. Anaheim appeared to me singularly backward in comparison with the surrounding American settlements. (I have been told in 1902, by an eye-witness, that it has remained in the same quiescent condition since.)

Another travelling companion whom I had for some time in South-California was the botanist Dr. C. C. Parry, from Davenport, Iowa. With him I spent one of the most enjoyable days in my life. The Southern Pacific R. R. was being built at that time through the desert beyond San Geronio Pass, and we spent the night at the so-called *Camp*, where the work was going on. The place is, I believe, now called *Seven Palms*. We started the next day on foot across the desert, and roamed for some hours in the San Jacinto Mountains. Coming down to the plain, we visited an Indian settlement near a warm spring, and recrossed the desert again on foot, in the dusk of the evening. The rosy and violet tints of desert and mountains at sunset, the pure, dry air, the perfect stillness of nature, interrupted at times by the rush of the large desert hare, left on me an indelible impression! As a memento of this day, I brought home two fine baskets of Indian workmanship (one of them 37 centim. in height, and 60 in diameter), which I carried six miles over the desert in a kind of hammock of *Yucca*-fibre, likewise of Indian workmanship, suspended across my shoulders on my back.

On my return-journey from California towards the East by the Pacific R. R. (in August 1876) I stopped at the Humboldt Station (Nevada) for the purpose of exploring the Alkaline Desert surrounding it. After roaming over the desert during the day, and meeting several very large rattle-snakes and nobody else, I returned to the station, spending the evening in labeling the specimens I had collected. In the small room which I occupied the heat was intense. My occupation excited the curiosity of a young Chinese servant who was in attendance. After I had explained to him my object, he patted me softly on my shoulder, and said: "You are a good man, you are a wise man". This expression of respect from a poor Chinese for an occupa-

tion which had often brought upon me derision from persons of my own class, confirmed me in the high opinion I have always entertained for the antique civilization of China!

On my further journey I enjoyed the hospitality of the Officers of the U. S. Army at Fort Bridger, Wyoming, a place, if I remember right, situated at an altitude of about 8000 feet above sea-level. My principal guide in that locality, the amiable Dr. James Van Allen Carter, took me to the marvellous fossiliferous region, called "Les Mauvaises terres".

At Davenport, Iowa, on the Mississippi, I was met at the station by the young naturalist Joseph Duncan Putnam and his family, in whose hospitable home I spent about a week, and formed a life-long friendship with them all. The premature death of young Putnam, a victim of scientific self-sacrifice, closed too early a career of the greatest promise. From Davenport, I went up the Mississippi, by steamer, to St. Paul and Minneapolis, and crossed overland to Duluth on Lake Superior, where I took the steamer and went along the Lakes to Buffalo and from there by the N. Y. Central and the Hudson River Railroads to New York. It was in September, and I recollect the vivid impression which the magnificent scenery along the Hudson River and the bright-green vegetation produced upon me as contrasted with the beautiful, but quite different sights I had enjoyed in California!

1877.

52. Extract from a letter of Baron Osten Sacken to Dr. Hagen on some specimens of *Termites* found in California.

Proc. Boston Soc. of Nat. Hist. XIX, Jan. 1877, p. 72—73.

NB. Published by Dr. Hagen, it refers to some observations new to science which I had made not only in California, but on the high altitudes of the Rocky Mountains.

53. Western Diptera, descriptions of new genera and species of Diptera from the region West of the Mississippi, and especially from California.

Bulletin U. S. Geolog. and Geograph. Survey of the Territories; III, April 30 1877, p. 189—354.

NB. This paper contains *eleven* new genera and 137 n. sp. The authorities of the Geological Survey in Washington not having allowed me to add a separate "Table of Contents" to this publication, I had one printed at my own expense and had it distributed with my Author's Copies. The hurry of the publication of the Western Diptera, I am sorry to say, left its traces in many misprints and other inaccuracies.

THIRD PERIOD.

WORK IN HEIDELBERG.

1877—1894.

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54. A singular habit of *Hilara*.

Entom. M. Mag. London, XIV, Nov. 1877, p. 126—129.

55. *Tachina*, parasitic on *Phasmidae*.

Psyche, II, 1877, p. 23 (only five lines).

56. *Blepharoptera defessa* n. sp.

In Prof. A. S. Packard's paper: "On a new Cave-fauna in Utah", which appeared in the Bullet. U. S. Geol. and Geogr. Survey III, p. 168, April 1877.

NB. The bad figure, appended to this description was published without my knowledge and consent.

1878.

57. Note on luminous Diptera.

Entom. M. Mag. London, XV, p. 43; 1878.

58. Report on the Diptera collected by Capt. Feilden and Mr. Hart, between 78°—83° of latitude.

In Mr. McLachlan's Report on the Insects of that Expedition, in the Journ. Linnean Soc. Zoology, XIV, p. 116—118.

59. Dipteren Larven.

Katter's Entom. Nachrichten, Jan. 1878, p. 5 (half a page).

NB. Short notice on two larvae, mentioned as unknown in the same periodical in 1876, p. 31. From internal evidence, I supposed this to be the, as yet unknown, larva of *Triogma* (*Cylindrotomina*). Compare in this "List" No. 164 (1897).

60. Bemerkungen über *Blepharoceriden*.

Deutsche Ent. Zeit 1878, p. 405—416.

NB. This paper has been entirely superseded by my later publications on the same subject, compare in this "List" No. 124 (1891) and No. 141 (1895).

61. Catalogue of the described Diptera of North America.

Second Edition. Washington, Smithsonian Institution, 1878, p. XLVIII and 276.

NB. Professor Spencer F. Baird published a detailed Notice about this *second* edition in the Smithsonian Report of 1878, p. 17—18. To characterize the difference between it and the *first* edition, he said: "This new edition is not merely a revision of the Catalogue published twenty years ago, but it is an entirely new one, prepared on a different plan. The difference between *eleven* and *sixty-six*, the number of species of the one genus *Trypeta*, represents the addition made to our knowledge during the interval between the two Catalogues" etc.

1879.

62. The *Tanyderina*, eine merkwürdige Gruppe der Tipuliden.

Verh. Z. B. Ges. Wien 1879, p. 517—522.

63. Ueber einige Fälle von *Copula inter mares* bei Insekten.

Stett. Ent. Z. 1879, p. 116—118.

64. (Without title.) On an immense accumulation of Coleoptera, observed by me on top of M. Washington in N. America in July 1865.

Bullet. Soc. Ent. Ital. 21 Dec. 1879, p. 26.

NB. They had been carried up by the ascending current of air.

In the same year 1879 I published;

Analytische Tabelle zum Bestimmen der nordamerikanischen Arten der Tipuliden-Gattung *Pachyrrhina* von Dr. H. Loew, mitgeteilt von C. R. Osten Sacken (Verh. Z. B. Ges. Wien, 1879, p. 513—516. (On p. 515, line 7 from bottom, after *suture* add: *tinged*. On the same page, line 9 from top, after *colored with black*, add: *at both ends*.)

1880.

65. Ueber einige merkwürdige Fälle von Verschleppung und Nichtverschleppung von Dipteren nach anderen Welttheilen.

Stett. Ent. Z. 1880, p. 326—332.

NB. This paper has been superseded by a later one, which is more complete. Compare No. 101 (1884) of this List.

66. Dr. Fritz Müller's discovery of a case of female dimorphism among Diptera (*Blepharoceridae*).

Entom. M. Mag. London, XVII, 1880, p. 130—132; continued ibidem p. 206, Febr. 1881 (comp. below, No. 72, 1881).

67. Habits of *Bombylius*.

Entom. M. Mag. London, XVII, 1880, p. 161; continued ibidem Febr. 1881, p. 206—207.

NB. Observations on the parasitism of the larvae of the Bombylidae *Systoechus*, *Triodites* etc. in egg-cases of grasshoppers.

68. About *Phora* being merely a scavenger and not a true parasite.

American Entomologist, III, November 1880, p. 277.

69. The red clover and hive-bees.

Entom. M. Mag. London, XVII, Nov. 1880, p. 142.

NB. An *item* of folk-lore in Mecklenburg, Germany.

70. Besprechung der: "Souvenirs Entomologiques", von J. H. Fabre, Vol. I.

Stett. Entom. Z. 1880, p. 136—138.

71. Note on American *Trypetidae*.

Psyche, III, April 1880, p. 53.

NB. *Trypetae* bred from galls in North America.

1881.

72. Dimorphism of female *Blepharoceridae*.

Entom. M. Mag. London, XVII, p. 206, February 1881.

NB. Compare above, No. 66 (1880).

73. On the use of the forceps of *Forficula*.

Canad. Entom. April 1881, p. 80.

NB. The forceps is used for disengaging the wings from under the wing-covers.

74. *Thyreophora antipodum* n. sp. (New Zealand).

Entom. M. Mag. London, XVIII, p. 35, July 1881.

75. On the larva of *Nycteribia*.

Trans. Ent. Soc. London, Sept. 1881, p. 359—361 (with a plate, drawn by Professor J. O. Westwood).

76. (In French, without title.) Diagnoses of new genera of *Ortalidae*.

Bullet. Soc. Entom. France; 10 Aug. 1881, p. XCIX—C.

NB. Preliminary notice for my publication on the Diptera of the Philippine Islands (compare in this List, No. 83, 1882).

77. Enumeration of the Diptera of the Malay Archipelago, collected by Professor O. Beccari, Mr. L. M. d'Albertis and others.

Annali Mus. Civico di Storia Naturale di Genova, XVI, p. 393—492,
Febr. 1881; — Supplement, XVIII, p. 10—20, Febr. 1882.

NB. *Four* new genera and 38 n. sp. are described.

78. A brief notice of Carl Ludwig Doleschall, the dipterologist.

Entom. M. Mag. London, XVIII, Oct. 1881, p. 114—116.

79. Verzeichniss der Entomologischen Schriften von Camillo Rondani.

Verh. Z. B. Gesellsch. Wien, 1881, p. 337—344.

NB. This list is superseded by my later, more complete list, comp. below No. 106, 1885.

80. An Essay of comparative *Chaetotaxy*, or the arrangement of characteristic bristles of Diptera.

Mitth. d. Münchener Entom. Vereins, V, p. 121—138, 1881.

NB. A *postscript* to this paper was printed and distributed privately by me; it was reproduced later in the paper: "Noch ein Paar Worte" etc.; comp. No. 85, 1882, in this List.

The term *Chaetotaxy*, proposed by me, has been used in this my No. 80, 1881 *for the first time*.

My *second* paper on *Chaetotaxy* (compare in this List No. 102, 1884) being more elaborate, has superseded the present one.

As it was expressed by a contemporary at that time, this improved method of *describing* Diptera has opened "new avenues" for this Order ("hat neue Bahnen eröffnet"; Entomologische Nachrichten, 1881).

81. A relic of the tertiary period in Europe: *Elephantomyia*, a genus of *Tipulidae*.

Mitth. d. Münchener Entom. Vereins, V, p. 152—154; 1881.

NB. Mr. Hieudlmayr, dipterologist in Munich, found in the Bavarian Alps a single specimen of a Tipulid in which I recognized the North American *Elephantomyia Westwodii* O. S. hitherto never found in Europe. This genus also occurs in amber.

1882.

82. Synonymica concerning exotic Dipterology.

Wiener Entom. Z. I, p. 19—21; January 1882.

83. Diptera from the Philippine Islands, brought home by Professor C. Semper.

Berl. Entom. Z. 1882, p. 82—120, and p. 187—251.

NB. Contains *seven* new genera and *sixty* new species.

84. List of the butterflies collected by me on the Pacific Coast, and principally in California, in 1876, with notes on their localities and habits.

"Papilio", New York, Febr. 1882, II, p. 29—31.

NB. This List evokes in me the most delightful recollections of some of the beautiful captures, mentioned in it. At Webber Lake, Sierra Co., California, at an altitude of about 8000 feet above sea-level, I hoped to find the alpine genus *Parnassius*, which I had never met with in America before. Being acquainted, from my European experience, with the peculiar mode of flight of this beautiful butterfly, it was with intense delight that I saw one slowly approaching me with its "hesitating flight". (The expression is Schiller's: "Der zweifelnde Flügel"; I find it in H. v. Kiesenwetter's: "Physiognomik einiger Insekten", Berl. Ent. Z. I, 65, 1857. Among all my entomological acquaintances Kiesenwetter struck me, more than many others, as "un homme d'esprit!")

85. Noch ein paar Worte über Chaetotaxie.

Wiener Ent. Zeit. I, 1882, p. 91—92.

NB. Compare above, No. 80, 1881.

86. Ueber das Betragen des Californischen flügellosen *Bittacus*.

Wien. Ent. Zeit. I, 1882, p. 193.

87. Referate über einige in russischer Sprache erschienene dipterologische Schriften.

Wien. Ent. Zeit. I, 1882, p. 149—151; p. 171—173.

NB. These publications were: B. A. Jaroscheffski, Verzeichnisse der Dipteren Kharkoff's; Portchinski, Die Bombus-ähnlichen Dipteren; A. P. Fedtchenko, Dipteren Moskau's.

88. Priorität oder Continuität? Ein dipterologischer Beitrag.

Wien. Ent. Zeit. I, 1882, p. 191—193.

NB. Protest against the mischievous zeal of some pseudo-entomologists who push *priority* in nomenclature to the extreme, at the expense of *continuity* and *stability*. They forget that "l'exactitude est le sublime des sots"!

89. On Professor Brauer's paper: "Versuch einer Charakteristik der Gattungen der *Notacanth*".

Berl. Ent. Zeitschr. 1882, p. 363—380.

NB. This paper was the rock upon which my friendship with Brauer split! When, after the death of Loew (1879), Brauer began to work on the same

lines with me (on the Classification, Nomenclature etc. of Diptera). I could not move without hitting him. "Collidebantur in utero matris parvuli"! (Genesis, XXV, 22.)

Although my criticisms were expressed in the most measured terms, they exposed the utter incompetence of the author for the task he had undertaken. This publication seems to have taken Brauer by surprise. In evident haste, he issued a Rejoinder entitled: "Offenes Schreiben, als Antwort auf Herrn Baron Osten Sacken's *Critical Review* (sic!) meiner Arbeit über die *Notacanthæ*". Vienna, 1883, 11 pages. At that time, I did not take the slightest notice of this publication, as it was evidently an explosion of impotent wrath without any scientific value, a succession of flat denials without any proof. As an instance, I quote the passage (on p. 11): "Die von Herrn Baron Osten Sacken so sehr hervorgehobenen Fehler sind gar keine Fehler, und das wegwerfende Urtheil ist somit aus der Luft gegriffen". Brauer seems to have been under the spell of a hallucination when he said (p. 10): "Osten Sacken hat meiner Arbeit einen degradirenden anderen Titel gegeben; ich unterlasse es, die "critical review" des Herrn Baron Osten Sacken bei ihrem wahren Namen zu nennen" etc. This expression "critical review", which seems to have haunted Brauer, *does not occur at all* in my paper¹, and even if I had used it, there is nothing *degrading* in it. On p. 8, with evident glee, Brauer points out a supposed omission of mine, which he imagined to have discovered: "*Hirnoneura clausa* O. S. hat, nach meiner Untersuchung von sechs Exemplaren aus Colorado, einen langen dünnen Rüssel, der bis zu den Hinterhüften reicht. — *Osten Sacken erwähnt den Rüssel gar nicht*". Although I immediately became aware of the blunder Brauer was committing here, I waited for *fourteen years*, until the pointing out of that *lapsus* became unavoidable. In the Berl. Ent. Z. 1897, p. 149, I showed that the whole paragraph in Brauer's "Offenes Schreiben", p. 8 about *Hirnoneura clausa* O. S., *Rhynchocephalus* and *Parasymmictus* Bigot is *simply nonsensical*, because it is based, not upon any error of mine, but upon the fact that the six specimens from Colorado, which Brauer had had before him were not *Hirnoneura clausa* O. S. at all, but another species. Far from *making no mention of the proboscis*, I had distinctly described it: "Face densely covered with pale yellowish hair, through which a short, reddish proboscis is hardly visible". Of a "long proboscis, reaching the hind coxæ", there is no question.

If I have insisted upon this incident at some length, it was because it became the origin of the unmitigated rancor which Brauer since that time bore against me, a rancor which affected his reputation. This did not prevent me from giving in this "Record", Part II, p. 168, a glowing account of Brauer's success in his true vocation, the biology of Insects, and especially of his work on *Oestridæ*.

¹ My paper 89 (1882) begins with the words: "A comparative critical survey" etc.

90. Bemerkungen zu Prof. Weyenbergh's Arbeit: *Trypeta Scudleri*, n. sp.

Verh. Z. B. Ges. Vienna, 1882, p. 369—370.

91. *Ants* and *Aphides*.

Psyche, III, May 1882, p. 343.

1883.

92. La deformazione del *Cynodon dactylon*, prodotta del dittero *Lonchaea lasiophthalma* menzionata pel primo da Francesco Redi.

Bull. Soc. Entom. Ital, 1883, p. 187—188.

93. On the genus *Apiocera*.

Berl. Entom. Z. 1883, p. 287—294.

NB. Compare my two later articles on the same subject: No. 109 (1886) and No. 123 (1891). Discussion concerning the position of this genus in the System. I prove that it is an *Asilid* and not a *Mydail*.

94. Synonymica concerning exotic Dipterology; No. II.

Berl. Entom. Z. 1883, p. 295—298. (Comp. above, No. 82, 1882.)

95. A singular North American fly: *Opsebius pterodontinus* n. sp.

Berl. Entom. Z. 1883, 299—300.

96. Zur Lebensgeschichte der Gattung *Hirmoneura*.

Wien. Entom. Z. 1883, p. 114.

1884.

97. List of the Diptera of the Island of Madeira, so far as they are mentioned in entomological literature.

Entom. M. Mag. London, July 1884, p. 32—34.

NB. Prepared at the request of one of the Editors of the E. M. M. Compare the notice about it by Mr. Mik, in the Wien. Ent. Z. 1884, p. 285—286.

98. Berichtigungen und Zusätze zum Verzeichniss der entomologischen Schriften von Camillo Rondani.

Verh. Z. B. Ges. Wien, 1884, p. 117—118.

NB. This and the preceding No. 79 (1881) are superseded by the later, complete List, published by me in the Boll. Soc. Entom. Ital. Compare below, No. 106, 1885.

99. (Notices about dipterological papers of different authors, published by me in the paragraphs "Literatur" of the Wiener Entom. Zeitung, 1884.)

a) On two publications of Mr. Portchinsky in the Horae etc. Rossicae, 1884: 1. *Sarcophaga Wohlfahrti*, Monographia. 2. Diptera Europaea and Asiatica nova etc. Wien. Ent. Zeit. 1884, p. 254—255.

b) B. A. Jaroscheffski. Nachträge zum Verzeichnisse der Dipteren Kharkoff's (compare above, No. 87, 1882). Wien. Entom. Zeit. 1884, p. 284—285.

c) W. F. Kirby. Diptera, collected during the expedition of the "Challenger" (Ann. & Mag. of N. H.; June 1884). Wien. Entom. Zeit. 1884, p. 315—316.

NB. This is a severe criticism of Mr. Kirby's performance.

100. *Phalacrocerca replicata* De G.

Entomologische Nachrichten, 1884, p. 311.

NB. A few lines only, pointing out a description of an aquatic larva described by Prof. Grube, in the Schles. Gesellsch. etc. 1867, p. 59, which, to all appearances is the same as De Geer's larva.

101. Facts, concerning the importation and non-importation of Diptera into distant countries. (Compare No. 65, 1880.)

Trans. Ent. Soc. London, 1884, p. 489—496.

102. An Essay of comparative *Chaetotaxy*, or the arrangement of characteristic bristles of Diptera.

Trans. Ent. Soc. London, 1884, p. 497—517.

NB. This is the development of the No. 80 (1881) above.

The proof-readers in London have in two cases quite arbitrarily substituted the term *plumose*, which has no sense in that connection, for *stubble-shaped*, which means a bristle *truncate at tip*, and is the translation of the German *stoppel-förmig*, which means resembling the stumps of a cereal left on the ground after harvest. These two cases occur on p. 507, line 14 from bottom; and on p. 514, line 9 from bottom.

103. On the New Zealand Dipterous Fauna.

New Zealand Journ. of Science, Sept. 1884, p. 198—201.

NB. As I have not received any extra-copies of this paper, I had it re-printed in Heidelberg in 1902 (see below, Nr. 174, 1902).

104. Verzeichniss der entomologischen Schriften von H. Loew.
Verb. Z. B. Ges. Wien 1884, p. 455—464.

NB. An *item* has been omitted in this *List*, and must be inserted thus:

- 151 bis. Ueber die lebendig gebärenden Dipterenlarven, welche in den letzten Jahren beobachtet worden sind.

Berl. Ent. Zeitschr. 1864, at the end of the volume, under the heading:
"Neuere Literatur".

Another paper, in the same "Verzeichniss", has been mentioned twice:
No. 205 must for this reason be struck out.

1885.

105. Bericht über eine in russischer Sprache erschienene dipterologische Arbeit (Stepanoff, über die Verwandlung einiger *Bombyliden*. Transactions of the Nat. Hist. Soc. in Kharkoff, Vol. XIV, 1881).

Wien. Ent. Z. Jan. 1885, p. 9—10.

NB. A *Systoechus*, destroying the egg-capsules of a grasshopper.

106. Elenco delle pubblicazioni entomologiche del Prof. Camillo Rondani.

Bull. Soc. Ent. Ital. 1885, p. 149—162.

NB. This complete List supersedes the previous one, No. 79, 1881.

1886.

107. Eine Beobachtung an *Hilara*.

Entomol. Nachrichten, 1886, p. 1—2.

NB. Reproduction of the article: Ent. M. Mag. London, 1877, p. 126 (see above No. 54, 1877).

108. Dipterologische Notizen.

Wien. Entom. Z. 1886, p. 42.

109. Correction to my article about *Apiocera* (compare above, No. 93, 1883).

Berl. Ent. Z. 1886, p. 139.

110. Notes towards the history of *Scenopinus fenestralis*.

Ent. M. Mag. London, Aug. 1886, p. 51—52.

111. The *Diptera Orthorrhapha* in Godman and Salvin's "Biologia Centrali-Americana". A quarto of 216 pages; with three colored plates.

NB. My share of this work was published in London, between July 1886 and March 1887. It contains 130 new species and *seven* new genera.

112. Some new facts about *Eristalis tenax* L.

Ent. M. Mag. London, Octob. 1886, p. 97—98.

NB. It is in this paper that I stated for the first time that the so-called *bees*, which issued from the lion killed by Samson (Book of Judges, 14, 8—9) were not bees at all, but the common drone-fly, *Eristalis tenax* L. (compare *Gelow*, Nos. 133, 142, 147).

113. Characters of the larvae *Mycetophilidae*.

Heidelberg, 1886; 30 pages and a plate.

NB. This is a reprint, with some additions, of my No. 16 (1862) above.

114. A luminous insect-larva in New Zealand.

Ent. M. Mag. London, Oct. 1886, p. 133—134; 1887, p. 230—231.¹

115. Studies on *Tipulidae*. Part I. Review of the published genera of the *Tipulidae longipalpi*.

Berl. Ent. Z. 1886, p. 153—188.

1887.

116. On Mr. Portchinski's publications on the larvae of *Muscidae*, including a detailed abstract of his last paper; "Comparative biology of the necrophagous and coprophagous larvae".

Berl. Ent. Z. 1887, p. 17—28.

117. Some American *Tachinae*.

Canad. Entom. 1887, p. 161—166.

NB. This paper must have been printed from a manuscript notice sent by me to one of my correspondents. I do not remember having authorised this publication.

118. Studies on *Tipulidae*. Part II (comp. above No. 115, Part I). Review of the published genera of the *Tipulidae brevipalpi*. Addenda and Corrigenda to Part I.

Berl. Ent. Z. 1887, p. 163—241.

NB. On the title-page of the *separate* copies, read *brevipalpi* for *longipalpi*.

1888.

119. Bemerkungen zu Herrn Th. Becker's Aufsatz über Dipteren-Zwitter.

Wien. Ent. Z. 1888, p. 94.

NB. A few lines of reference to a hermaphrodite of *Dilophus*, described by Zett.

120. *Chloropisca prolifica* n. sp. (United States).

In the "Fourth Report on the injurious and other insects of the State of New-York", by J. A. Lintner, State Entomologist; Albany, N. Y. 1888, p. 70—72.

NB. A fly occurring in houses in enormous numbers like *Chloropisca ornata* Loew in Europe, but specifically different. About the European species, compare "Entomologische Nachrichten", 1881, p. 17, where the species is called *Chl. nasuta*. I discovered later that my *Chl. prolifica* is the same as *Chlorops variceps* Loew, Centuria III, 86. I had been misled by the erroneous location of the latter species among the *Chlorops sensu stricto*. It has a flat scutellum and is a *Chloropisca*. In my Catalogue of 1878, the locality for *Chloropisca variceps* should be *Pennsylvania*, and not *Sitka*.

1889.

121. Letter about *Haematobia serrata* Rob. Desv.

"Insect-Life" Washington, Vol. II, Dec. 1889, p. 191.

NB. The specific identity of the American and European specimens of this fly has been confirmed, after a careful comparison, by the experienced dipterologist Ferdinand Kowarz.

(*Biographical.*) Feeling unwell at the end of February 1889, I went South for rest and recreation, with the intention of visiting Spain, where I had never been before. But in Montpellier my state became such that I returned in great haste to Heidelberg, by way of Genoa. Although, owing to my excellent constitution I recovered soon, I was advised to abstain from work for a certain length of time. For this reason I did not publish anything of consequence during this and the following year.

1890.

122. *Hilarimorpha* Schin. is a *Leptid*.

Berl. Ent. Z. 1890, p. 303—304.

NB. MM. Mik and Williston considered it an *Empid*.

1891.

123. Second notice on the *Apiocerina*.

Berl. Ent. Z. 1891, p. 311—315 (see above, No. 93, 1883).

124. Synopsis of the described genera and species of the *Blepharoceridae*.

Berl. Ent. Z. 1891, p. 407—411. (Compare, No. 60, 1878, and No. 144, 1895.)

125. On the Chaetotaxy of *Cucoxenus indagator* Loew.

Berl. Ent. Z. 1891, p. 411—413.

NB. A complement to Loew's description of this fly; I show that my *chaetotactic* method is applicable even to very small flies.

126. On the Synonymy of *Antocha* O. S. and *Orimargula* Mik.

Berl. Ent. Z. 1891, p. 413—416.

NB. *Orimargula* is nothing but an *Antocha* with an open discal cell.

127. Additions and Corrections to the Catalogue of the described species of South-American *Asilidae*, by S. W. Williston, (in the Trans. Amer. Ent. Soc. XVIII, 1891).

Berl. Ent. Z. 1891, p. 417—428.

128. Suggestions towards a better grouping of certain families of the Order Diptera.

Entom. M. Mag. London, Jan. 1891, p. 35—39.

This publication was intended to foreshadow my No. 130 (1892) of this List, and gave rise to a succession of attacks against me by MM. Brauer and Mik.

1892.

129. (Without title, in the Bull. Soc. Ent. de France, April 27 1892.) Answer to a query of Mr. E. Abeille de Perrin concerning an Appendix II, No. 6, p. 600 to the "Species Insectorum" of Fabricius, which does not exist in many copies of this work. I refer him to the article of Mr. Schmidt Goebel in the Stett. Ent. Z. 1881, p. 330.

130. On the Characters of the three Divisions of Diptera, *Nemocera vera*, *Nemocera anomala* and *Eremochaeta*.

Berl. Ent. Z. 1892, p. 417—466.

An "Explanatory notice" to this paper has been published by me in the Ent. M. Mag. London, 1893, p. 149—150. (Comp. below, No. 132, 1893.) I have not found time to finish a detailed paper on the *Eremochaeta*, which I had in preparation; the principal feature of it, the suppression of the old family *Xylophagidae*, has been introduced by me incidentally ten years before my present paper, in my No. 89, 1882. (Berl. Ent. Z. 1882, p. 363—367.)

1893.

131. Singular swarms of flies.

"Nature", June 22 1893, p. 177.

Answer to a query under this heading in the same periodical June 1 1893. The flies were small *Chironomi*.

132. Explanatory notice of my views on the Sub-orders of Diptera.

Ent. M. Mag. London, July 1893, p. 149—150.

This notice refers to my No. 130, 1892, above. The passage p. 150, line 5 from top: "The position of the *Papipara* I leave an open question" must be struck out, as there is no doubt that *Papipara* belong to the Sub-order *Cyclorhapha Athericera*.

133. On the so-called "Bugonia" of the Ancients, and its relation to *Eristalis tenax*, a two-winged insect.

Bullet. della Soc. Ent. Ital. 1893, p. 186—217.

Offered to the Society in honor of the 25th anniversary of its foundation. (Reproduced, almost in extenso, in the "Annual Report of the Smithsonian Institution", Washington, D. C. 1893, p. 487—500.)

A second, much enlarged paper on the same subject was published by me later, as a separate pamphlet (see below, No. 142, 1894).

134. *Corrigendum* concerning the "Bugonia" etc.

Ent. M. Mag. London, 1893, p. 287 (two lines only).

I had mistaken the moth *Carpocarpus pomonella*, for *Trypeta pomonella* Walsh, a dipteran.

135. Zur Geschichte der sogenannten Brustgräte (*breastbone*) der *Cecidomyien*, nebst einer Erinnerung an Karl Ernst von Baer.

Berl. Ent. Z. 1893, p. 373—377.

A passage of this paper has been misunderstood by Mr. Williston who, in his *Manual* etc. of N. A. Diptera, New Haven, 1896, p. 9, has the erroneous statement: "This organ, *discovered by von Baer*, has been called *breast-bone* by Osten Sacken"!

136. Rejoinder to Prof. Brauer's: "Thatsächliche Berichtigung" etc. in the Berl. Ent. Z. 1892, p. 487—489.

Berl. Ent. Z. 1893, p. 378—379.

To this paper Brauer replied in his "Bemerkungen" etc. in the Berl. Ent. Z. 1894, p. 235, but this reply elicited immediately a short statement from Prof. L. C. Miall (of Leeds, England) *ibidem* p. 447: "Brauer's quotation from Weismann refers to *Chironomus*, and not to *Corethra*, which has no place in this discussion" etc. This was one of the *inadvertences* Prof. Brauer is apt to commit in his hasty rejoinders.

137. Two critical remarks about the recently published *third* part of the "Muscaria Schizometopa" of MM. Brauer and Bergenstamm, also a notice on Rob. Desvoidy.

Berl. Ent. Z. 1893, p. 380—385.

My excellent friend J. M. F. Bigot died April 14 1893 at his country-seat near Paris at the age of 74 years. (An Obituary Notice will be found in the Bulletin de la Soc. Ent. de France April 26 1893.) My acquaintance with Bigot was of long date. When I met him again after my return from the United States in 1877, and our relations were renewed, I felt it my duty, although treating him as a friend, to tell him the truth about his publications. I did so, during one of my visits to Paris (the year I do not remember), in telling him that he was doing a useful work in forming a large collection, especially of *exotie* Diptera; but that he should renounce *descriptive* work, for which he was not competent. I expressed my opinion in the strongest terms, concluding with the words: "If *all* your publications could be suppressed, it would be a *gain* for science!"¹ He winced a little, but resumed immediately his serene expression, and said: "Eh bien, cela m'amuse"! My frankness did not prevent us from remaining friends up to his death. In the most generous manner, he kept his rich collection and library at my disposal. Even when he was away from Paris (he usually spent his winters in Algiers) he left orders to his housekeeper to admit me at any time into his *sanctum*, and to light a fire for me in winter. I keep of my friend Bigot and of his family (wife and daughter) a pleasant and grateful recollection.

The share I had in the sale of Bigot's collection is explained in the Bulletin de la Soc. Ent. de France, June 14 1893. In this notice it should have been added that I had written to the Authorities of the Museum in Paris, urging them to purchase this important collection offered at a very moderate price (6000 francs). My advice, however, was not heeded, and the collection passed into the hands of Mr. G. H. Verrall of Newmarket, England, and will ultimately, as I understand, find its place in the British Museum.

It remains for me now to reproduce the opinion expressed by Mr. Bigot himself, at different times, about his own publications:

In the Annales etc. 1885, p. 225 he said: "J'avais antrefois (Ann. etc. 1852—1859) commencé la publication d'un travail intitulé: *Essai d'une classification générale et synoptique de l'Ordre des Insectes Diptères* etc. Cette oeuvre fut alors sévèrement et doctoralement critiquée dans le *Bericht de Gerstaecker*

¹ From this general condemnation I except of course those publications of Bigot which are illustrated by excellent colored plates (as his *Diptères du Madagascar* and *Diptères du Chili*).

(Berlin) par l'habile diptériste H. Loew. Je dois confesser que la plupart de ses objections m'ont paru tellement fondées que, reconnaissant la médiocre valeur de mes *Essais*, je me résigne à les abandonner à mi-chemin". Whereupon Mr. Bigot proposes a new plan for the distribution of the Diptera, into two Tribes: *Omalocerati* and *Anomalocerati* etc.

A further discussion between Bigot and G. H. Verrall, about questions of orthography and of *mutual courtesy* will be found in the Wien. Ent. Zeitschr. 1889, p. 168, 265 and 293. In the latter notice, Bigot renews his former recantation of his "*Essai*": "Si Mr. Verrall avait daigné lire mes opuscules, etc. il y aurait vu, maintes fois répété, que mon ancien travail, intitulé "*Essai*" etc. devait être désormais, et dans son entier, considéré comme nul et non avénu, déclaration qui m'absout naturellement des fautes que j'ai pu y commettre".

1894.

138. On the Atavic Index-characters, with some Remarks on the classification of Diptera.

Berl. Ent. Z. 1894, p. 69—76.

139. Synonymica about *Tipulidae*.

Berl. Ent. Z. 1894, p. 249—263.

In the first part of this paper I propose some rules to be observed in adducing synonymies, rules which have hitherto been generally neglected by authors (including myself).

140. Three *Trochobolae* from New Zealand and Tasmania.

Berl. Ent. Z. 1894, p. 264—266.

A *Correction* to this paper was published by me *ibid.* 1895, p. 170; but, on that page, read *australis* Skuse, for *australensis*.

141. A remarkable case of malformation of the discal cell in a specimen of *Liogma glabrata*.

Berl. Ent. Z. 1894, p. 267—268.

142. On the oxen-born bees of the Ancients (*Bugonia*), and their relation to *Eristalis tenax*, a two-winged insect.

Heidelberg, 1894, XIV and 80 pages (printing-office of J. Hoerning).

This is a new edition, much enlarged, of my No. 133, 1893. See also below, No. 147, 1895.

An interesting result of my identification of the *Oxen-born* bees of the ancients with the fly *Eristalis tenax* (drone-fly) is the very simple explanation of the story of Samson's bees which it affords. These pretended bees, which Samson found nesting in the carcass of the lion he had killed a short time before, were the common *drone-flies*, resembling bees. This story, which has been

the puzzle of interpreters for centuries, and in which Professor Max Müller saw a *solar myth*, thus turns out not to be a *myth* at all, but an actual occurrence. In my No. 142, 1894, p. 16—18 will be found a detailed explanation of this story.

1895.

143. *Eristalis tenax* in Chinese and Japanese literature.

Berl. Ent. Z. 1895, p. 142—147.

This paper, in a corrected form, has been incorporated in my *Additional notes* etc., No. 147, 1895, below.

144. Contributions to the study of the *Liponeuridae* Loew (*Blépharoceridae* Macq.).

Berl. Ent. Z. 1895, p. 148—169.

A "Correction" to this paper was published by me in the Ent. M. Mag. London, 1895, p. 118, and a "Supplement" in the Berl. Ent. Z. 1895, p. 351—355.

145. Western *Pediciae*, *Bittacomorphae* and *Trichocerae*.

Psyche, Vol. VII, April 1895, p. 229—231.

I take occasion to notice here that the specimen of *Bittacomorpha*, taken by Lord Walsingham at Pitt River, California, and mentioned in the Ent. M. Mag. London 1879, Vol. XVI, p. 70, was not the Eastern *B. clavipes*, as stated there, but the Western *B. occidentalis* Aldrich, described in 1895.

146. Appendix to Professor L. C. Miall's and Mr. Walker's paper: "The life-history of *Pericoma canescens*" (*Psychodidae*).

Trans. Ent. Soc. London, 1895, p. 147—152.

This *Appendix*, prepared by me at the request of Prof. Miall, contains an account of the literature concerning the early stages of *Psychodae*.

147. Additional notes in explanation of the Bugonia-lore of the Ancients.

Heidelberg, J. Hoerning, 1895 (23 pages). Comp. above, No. 142, 1894.

148. Fungoid disease of *Tipulae*.

Ent. M. Mag. London, 1895, p. 215.

149. *Midas* or *Mydas*? A contribution to entomological nomenclature.

Berl. Ent. Z. 1895, p. 345—350.

I prove that Fabricius's spelling *Mydas* was intentional, and not a misspelling, as was supposed.

150. Remarks on the homologies and differences between the first stages of *Pericoma* Hal. and those of a new Brazilian species (with two plates).

Trans. Ent. Soc. London, 1895, p. 483—487.

This paper is explanatory of Dr. Fritz Müller's paper in the same volume, which is entitled: "Contribution towards the history of a new form of larvae of *Psychodidae* (Diptera) from Brazil". The Rev. Alfred E. Eaton, at my request, prepared a *third* paper: "Supplementary notes on Dr. F. Müller's paper on a new form of larvae of *Psychodidae* from Brazil" (ibid. p. 489—493). This new form of larva of a Brazilian *Psychodid* is remarkable for *suctorial discs* on its ventral side, which are like those of the larvae of *Blepharoceridae*.

In the "Introduction", p. 16, line 13 from bottom, I have said: "A similar paper on *Cyrtidae*, which was nearly finished in 1895, is still unpublished". This passage requires an explanation. My intention at that time, was, before publishing my paper, to pay a visit to England and to verify some doubtful points in the Museums in London and Oxford. In the mean time Professor Dr. Karsch, of the Berlin Museum, with whom I was in correspondence, wrote me that one of the young *attachés* of the Museum, Dr. Benno Wandolleck, felt much disappointed when he heard of my purpose of a publication on the *Cyrtidae*, as he had entertained the same intention. Supposing that a trained zoologist of the Berlin Museum was much more capable than I to accomplish such a task, I did not hesitate a moment in offering him my manuscript for use. He expressed to me his most sincere gratitude in a letter of Sept. 22 1895. On January 4 1896 he wrote me that he was occupied with translating my manuscript, and would return it to me soon afterwards. On August 17 1896 he informed me of the progress of his work, that he had received materials from Vienna, and from Mr. Verrall in Newmarket.

Not hearing anything about the matter since, I made a new inquiry (in March 1904), and was informed that the author's private affairs had prevented him from continuing his work, but that he hoped, nevertheless, to take it up again, as soon as his circumstances would allow it. In the mean time he had returned my MSS. to me.

1896.

151. A new genus of *Cyrtidae* (Dipt.) from New Zealand.

Ent. M. Mag. London, 1896, p. 16—18.

Helle, gen. nov. An inadvertence occurred in the very first line of this paper: for *Megalybus* Westw., read *Megalybus* Philippi.

152. *Camarota*, as a noxious insect.

Ent. M. Mag. London, 1896, p. 257.

153. Bibliographische, und theilweise psychologische Untersuchung über die zwei Ausgaben der Erstlingsarbeit von H. Loew: "Ueber die Posener Dipteren".

Berl. Ent. Z. 1896, p. 279—284.

154. Notice on the terms: *tegula*, *antitegula*, *squama* and *alula*, as used in dipterology.

Berl. Ent. Z. 1896, p. 285—288.

155. The larval habits of *Baccha*.

Ent. M. Mag. London, 1896, p. 279.

NB. This is a mere reference to my notice on the same subject in the Stett. Ent. Z. 1862, p. 412.

1897.

156. Prof. Mik's genus *Paracrocera* (Cyrtidae), with a Postscript about the genus *Alloconeurus* Mik (Dolichop.).

Berl. Ent. Z. 1896, p. 323—327.

NB. The last part of this volume of the Berl. Ent. Z. appeared only at the end of May 1897; my separate copies of this and of the three following papers (Nos. 157, 158 and 159) had been distributed a short time before, that is, in 1896.

Mik's misunderstanding of the term "piracy", which I had used in this paper, and accusing me of calling him a *pirate* (Seeräuber), my explanation, and his reply to it (Wien. Ent. Z. 1897, p. 212), was an incident which I considered as "a slight contribution to the gaiety of nations" (compare above p. 180).

157. On the terms *Calyptratae* and *Acalyptratae*, *Calypta* and *Calyptra*, as they have been used in entomology (A supplement to my article No. 154, 1896: "Notice on the terms: *tegula*, *antitegula*" etc.). Berl. Ent. Z. 1896. 328—336

158. Preliminary Notice of a Subdivision of the Sub-order *Orthorhapha brachycera* on chaetotactic principles.

Berl. Ent. Z. 1896, p. 365—373.

NB. Two inadvertences have occurred in this paper: on p. 368, in the last line at bottom, and in line 11 from bottom, read *Achaeta*, for *Acheta*.

159. The genus *Phyllolabis* O. S. (Dipt. Tipul.), a remarkable case of disconnected areas of geographical distribution.

Berl. Ent. Z. 1896, p. 374—376.

160. Professor E. D. Cope as an Entomologist.

Psyche, Vol. VIII, p. 75.

NB. Publication of a short but interesting note, which this celebrated palaeontologist wrote me in 1867.

161. (The paragraph on *Diptera* in Dr. Lucas v. Heyden's: *Insecta*, in the Abh. d. Senckenb. Nat. Ges. XXIII, Heft IV, p. 589—590).

Enumeration of the species of a small correction of *Diptera* from Halmahera, Celebes and Borneo (in the Austro-Malayan Archipelago); 15 species in all.

162. Identification of two Genera of *Nemestrinidae*, published by Mr. Bigot, together with some remarks on Dr. Wandolleck's paper on that family.

Berl. Ent. Z. 1897, p. 145—149.

This is the paper in which, after fourteen years of silence, I called attention to the egregious blunder committed by Prof. Brauer in his "Offenes Schreiben" etc. of 1883. (Compare above, No. 89, 1882.) Dr. Wandolleck's paper on *Nemestrinidae*, in which he overlooked Loew's publication upon it, and his very lame excuse for this omission, are duly noticed. — The volume 1897 of the Berl. Ent. Zeit. was distributed, as stated on its cover, in Juli 1898; my extra-copies of No. 162 had been distributed by me *six months earlier*, in Dec. 1897, and therefore must bear that date.

163. *Amalopsis* Hal. (O. S.) versus *Tricyphona* Bergroth (not Zetterstedt).

Berl. Ent. Z. 1897, p. 150—154.

To my carefully prepared statement Mr. Bergroth made a reply of 18 lines (Wien. Ent. Z. 1898, p. 267).

164. Remarks on the literature of the earlier stages of the *Cylindrotomina*, a Section of the *Tipulidae*.

Trans. Ent. Soc. London, 1897, p. 362—366.

This paper was prepared at the request of Prof. L. C. Miall, to serve as an Appendix to his paper "The structure and Life-history of *Phalacrocer a replicata*".

1898.

During this year I was principally occupied with the manuscript of the "Record of my Life Work in Entomology" the preparation of which I had begun several years before. In

August 1898, having been compelled to vacate the lodgings which I had occupied in Heidelberg for 21 years, I purchased a house and garden in Bunsen Str. 8. The renovation of this house from top to bottom, the furnishing of it, the moving into it of my large library and of my various collections (and that at the age of *seventy*) resulted in a complete prostration, which prevented me from attempting any scientific publication for more than a year after.

1899.

165. (Without title). A note in Dr. S. H. Scudder's article: "An unknown tract on American Insects, by Thomas Say" (in *Psyche*, January 1899, p. 306—308). I expressed the opinion that *Trypeta trifasciata* Say, New Harmony, 1831, from Louisiana, must be either identical, or closely allied to *Chaetopsis debilis* Loew, Monogr. etc. III, p. 172, Tab. 9, fig. 20, from Cuba.

Towards the end of 1898, Dr. D. Sharp, of Cambridge, England, sent me the proof-sheets of the chapter on *Diptera*, for his work "On Insects", for the "Cambridge Natural History" series, Vol. VI, London 1899. My debilitated state notwithstanding, I sent to Dr. Sharp a detailed *Memorandum* of emendations and remarks. He expressed his thanks in a letter dated January 29 1899.

In April 1899, at the request of Mr. L. O. Howard, Entomologist of the U. S. Department of Agriculture in Washington, D. C. I sent him a *Memorandum*, frankly expressing my very unfavorable opinion of the publications of Mr. D. W. Coquillett on *Diptera*. My opinion produced, apparently, no effect whatever in Washington.

1900.

166. Notiz über die Erstlingsarbeit von C. Dumeril über entomologische Classification, mit besonderer Rücksicht auf die Gattung *Tetanocera*.

Verh. Z. B. Ges. Wien, 1900, p. 450—451.

1901.

167. On the new nomenclature of the family *Cecidomyiæ*, adopted by Mr. Rübsaamen and others.

Ent. M. Mag. London, Febr. 1901, p. 40—43.

168. A Prospectus of the "Record of my Life-Work in Entomology", as it exists in manuscript.

Heidelberg, June 1901. — A pamphlet of nine pages, printed by J. Hoerning in Heidelberg, which I distributed to my correspondents.

- 168 bis. Notice on the Synonymy of *Anopheles maculipennis* M.

Ent. M. Mag. London, Dec. 1900, p. 282—283.

I show that this is the correct specific name of this species, and not *A. claviger* Fabr. Syst. Autl. 1805, which has been adopted for it by recent authors.

169. An Introduction to my "Record of my Life Work in Entomology".

Printed in pamphlet-form in Cambridge, England, at the University Press. Later, this "Introduction" was reproduced without any change in Part I of my "Record" etc. which appeared in Cambridge, Mass. in October 1903.

170. The two methods of determining Diptera.

Ent. M. Mag. London, Dec. 1901, p. 295—296.

This formed a part of the manuscript of my "Record" which I published in advance. It will be found reproduced in the "Record" on p. 129—130.

171. Mosquito-swarms responsive to sounds.

Ent. M. Mag. London, Dec. 1901, p. 296.

1902.

172. Publication of the two first sheets of my "Record", Part II. They were printed in Heidelberg and distributed to my correspondents as a sample of what was coming. Three plates were added to them: the portraits of Loew and Haliday, and a plate with a facsimile of Loew's handwriting. The Preface is dated April 1902.

173. A remarkable instance of deliberation observed in an American ant.

Ent. M. Mag. London, 1902, p. 172.

174. On the New Zealand Dipterous Fauna.

A reprint, with a few emendations, of the paper No. 103, 1884 of this List, which appeared originally in the "New Zealand Journal of Science" Sept. 1884, p. 198—200.

175. On the position of *Phora* in the System of Diptera.

Ent. M. Mag. London, 1902, p. 204.

176. On the distinctive character of the imagos of the Suborders of Diptera: *Orthorrhapha Brachycera* and *Cyclorrhapha Athericera*, introduced by Latreille (1825), but overlooked by other authors.

Ent. M. Mag. London, 1902, p. 228.

1903.

177. Berichtigung zu Herrn Fr. Hendel's Notiz über meinen Aufsatz: "On the position of *Phora*" etc.

Wien. Ent. Z. 1903, p. 120.

A short rejoinder to the fierce attack which Mr. F. Hendel, evidently at the instigation of Prof. Brauer, made on my papers No. 175 and No. 176, 1902 (see above).

178. Record of my Life-Work in Entomology, Part I and II.

Cambridge, Mass., October 1903, University Press.

With the portraits of Haliday and Loew, and a *fac-simile* of the latter's handwriting.

This volume contains the Part I (Introduction) and the Part II ("Twenty-four Chapters" etc.) of the "Record". At the end of the volume I have inserted a "Postscript", printed in Heidelberg, Germany, expressing my profound gratitude to Mr. Samuel Henshaw, of the Zoological Museum in Cambridge, Mass. for seeing my "Record" through the press.

1904.

179. Record of my Life-Work in Entomology, Part III.

Printed in Heidelberg, Germany, in the University printing-office J. Hoerning.

fascicule / This ~~volume~~ contains the List of my Entomological Publications from 1854 to 1904. This List is, of course, arranged chronologically; short biographical paragraphs are intercalated in it from time to time, for the purpose of showing under what circumstances of my external life my successive publications had been issued.

This my "Record" of half a century (1854—1904) of entomological work I now conclude, at the age of 76, in good health and with unimpaired eye-sight.

Heidelberg, Germany, March 31 1904.

ADDENDA AND CORRIGENDA.

- p. 2, line 6 and 7 from top, should read: "Shatiloff's portrait is in Vol. I" etc.
- p. 60, line 15 from bottom. Mr. G. H. Verrall wrote me that "Curtis's Guide, Edit. II, p. 280, has *Malacomyza*", and not *Malacomyia*, as I had quoted it on my p. 60. I find now that in the copy of Curtis's "Guide", annotated by Haliday, which he had sent to Loew, and which is now in my possession, the original spelling *Malacomyza* is, by Haliday himself, converted into *Malacomyia*, by the erasure of the *z*, which is replaced by an *i*. I had not noticed this change before, and was, therefore, puzzled by the spelling *Malacomyza* in another passage (compare the same page 60, six lines below).
- p. 61, line 8 from top, read 1867, for 1847.
- p. 142, line 6 from bottom, read 1855 for 1885.
- p. 144, at top. Mr. G. H. Verrall called my attention to the fact that, "as Zetterstedt missed pages 5100 to 5999 in his work, there were only 5709 pages, which make an average of 408 per volume". A corresponding change must, therefore, be made in my letter-press. I find now that Zetterstedt himself has called attention to this omission. At the *very end* of his work, Vol. XIV, p. 6609, I find the passage: "*Observandum*: In Tomo XIII, paginis 6000, 6001, et sic porro; lege: 5000, 5001 et sic porro". But here Zetterstedt commits a new mistake; it should be: 5100, 5101 etc. up to 5999.
- p. 145, line 9 from bottom, read 1845 for 1885.
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POSTSCRIPT.

To make my "Record" complete, it should have been provided with an Index, *prepared by myself*, because that alone would have answered my purpose. But I feel that such a task, at present, would be beyond my strength. The detailed Table of Contents which I have given at the beginning of Part II, must, in the meantime, supply the absence of a good Index.

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AUTHOR

Osten-Sacken, Carl Robert

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