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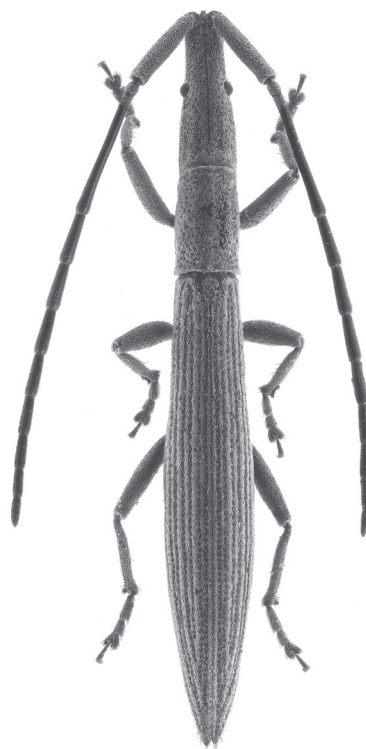


Кавказский Энтомологический Бюллетень

CAUCASIAN ENTOMOLOGICAL BULLETIN

Том 17. Вып. 2

Vol. 17. No. 2



Ростов-на-Дону
2021

**Critical remarks on “Catalogue of Palaearctic Coleoptera.
Vol. 6/1. Chrysomeloidea I (Vesperidae, Disteniidae, Cerambycidae).
Updated and revised second edition”, Leiden – Boston: Brill, 2020,
with corrections and additions**

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Abstract. The second, updated edition of the Catalogue of Palaearctic cerambycids is discussed in detail. The unprecedented huge amount of gross mistakes, highly dubious and controversial data held in the first edition of the Catalogue was one of the main reasons for republication. Special attention is paid here to the sections of the first edition authored by M.L. Danilevsky, which contained the vast majority of the serious errors and unfortunate inaccuracies. These sections actually were only a careless and rough draft of a catalogue but by no means a complete and full-fledged scientific work. Very sadly, the updated catalogue is shown not only to have inherited a very large number of M.L. Danilevsky's mistakes from the first edition but it was substantially replenished with other wrong and very dubious information of this author. Significant additions and changes to the catalogue are introduced, and its numerous mistakes and misprints are corrected. The following new synonymy is established: *Paracorymbia* Miroshnikov, 1998 = *Maculileptura* Danilevsky, 2015, **syn. n.**; *Monochamus* Dejean, 1821 = *Murzinia* Lazarev, 2011, **syn. n.**; *Monochamus ruspator* (Fabricius, 1781) = *Murzinia karatauensis* Lazarev, 2011, **syn. n.** The previously established synonymy *Batesiata* Miroshnikov, 1998 = *Pyrrholeptura* Lazarev, 2016, *Neocerambyx* J. Thomson, 1861 = *Bulbocerambyx* Lazarev, 2019, *Melanoleptura scutellata scutellata* (Fabricius, 1781) = *M. scutellata ochracea* (Faust, 1878), *Anaglyptus mysticoides* Reitter, 1894 = *A. mysticoides obscurissimus* Pic, 1901, *Purpuricenus neocaucasicus* Rapuzzi et Sama, 2014 = *P. caucasicola* Danilevsky, 2015, *Purpuricenus renyvovae* Sláma, 2001 = *P. baeckmanni* Danilevsky, 2007, *Cerambyx cerdo acuminatus* Motschulsky, 1853 = *C. cerdo manderstjernae* Mulsant et Godart, 1855 is confirmed. Various cases of pure plagiarism by M.L. Danilevsky and his disrespect to scientific ethics are presented, as well as some vivid examples of this author's falsifications of diverse data and his manipulations with published scientific evidence are given. The mentioned investigator is demonstrated to have utterly disregarded a wealth of valid and reliable data by some other authors he is well aware of. Very obscene and unjust cases of substitution of some authors in the second edition are discussed. Attention is drawn to M.L. Danilevsky's various data given in the catalogue, which do not correspond, either completely or in part, to the information published in his own, relatively recent monograph on longicorn beetles of the former USSR plus Mongolia. Such an inconsistent and contradictory presentation by that author is argued to be directly related to the question of his data reliability as a whole contained in the updated catalogue. In contrast, the honest and fruitful contributions of almost all other authors to the second edition are to be emphasized.

Key words: Coleoptera, Cerambycidae, Catalogue, Palaearctic, critical remarks, corrections, additions, new synonymy.

**Критические замечания по поводу «Catalogue of Palaearctic Coleoptera. Vol. 6/1.
Chrysomeloidea I (Vesperidae, Disteniidae, Cerambycidae). Updated and revised second edition»,
Leiden – Boston: Brill, 2020 с исправлениями и дополнениями**

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Резюме. Подробно обсуждается второе, обновленное издание каталога жуков-дровосеков Палеарктики. Одним из главных оснований для этого переиздания послужило беспрецедентно огромное количество грубых ошибок, весьма сомнительных и спорных данных, содержащихся в первой версии каталога. Особое внимание обращается на разделы первого издания за авторством М.Л. Данилевского, в которых сосредоточено абсолютное большинство серьезных ошибок и досадных неточностей. Именно эти разделы фактически оказались лишь небрежным черновым наброском каталога, но отнюдь не законченным и полноценным научным трудом. Обновленный каталог не только унаследовал очень большое количество ошибок М.Л. Данилевского, допущенных им в первом издании, но и существенно пополнился его другими ошибочными и сомнительными сведениями. Представлены значительные дополнения и изменения к каталогу и исправлены его многие ошибки и опечатки. Установлена следующая новая синонимия: *Paracorymbia* Miroshnikov, 1998 = *Maculileptura* Danilevsky, 2015, **syn. n.**; *Monochamus* Dejean, 1821 = *Murzinia* Lazarev, 2011, **syn. n.**; *Monochamus ruspator* (Fabricius, 1781) = *Murzinia karatauensis* Lazarev, 2011, **syn. n.** Подтверждена ранее установленная следующая синонимия: *Batesiata* Miroshnikov, 1998 = *Pyrrholeptura* Lazarev, 2016; *Neocerambyx* J. Thomson, 1861 = *Bulbocerambyx* Lazarev, 2019; *Melanoleptura scutellata scutellata* (Fabricius, 1781) = *M. scutellata ochracea* (Faust, 1878); *Anaglyptus mysticoides* Reitter, 1894 = *A. mysticoides obscurissimus* Pic, 1901; *Purpuricenus neocaucasicus* Rapuzzi et Sama, 2014 = *P. caucasicola* Danilevsky, 2015; *Purpuricenus renyvovae* Sláma, 2001 = *P. baeckmanni* Danilevsky, 2007; *Cerambyx cerdo acuminatus* Motschulsky, 1853 = *C. cerdo manderstjernae* Mulsant et Godart, 1855. Отмечены различные случаи плагиата М.Л. Данилевского и пренебрежения им научной этикой, а также приведены яркие примеры фальсификации этим автором опубликованных сведений и его манипуляции литературными данными. Показано полное игнорирование

упомянутым исследователем большого пласта обоснованной и достоверной, хорошо известной ему информации некоторых авторов, проливающей свет на многие спорные и слабо изученные вопросы. Затронуты неэтичные и неоправданные случаи подмены некоторых авторов во втором издании. Обращается внимание на различные сведения М.А. Данилевского, приведенные в каталоге, которые полностью или во многом не соответствуют данным, опубликованным в его сравнительно недавней монографии по жукам-дровосекам бывшего СССР и Монголии. Такая непоследовательность и противоречивость информации этого автора имеет непосредственное отношение к вопросу достоверности его сведений в целом, представленных в обновленном каталоге. Напротив, дана высокая оценка добросовестному и плодотворному труду почти всех других авторов второго издания.

Ключевые слова: Coleoptera, Cerambycidae, каталог, Палеарктика, критические замечания, исправления, дополнения, новая синонимия.

Introduction

Long ago, the need to produce an updated and revised catalogue of Palaearctic cerambycids [Catalogue..., 2020], to which the present work is devoted, became certain. Some time since the publication of the first edition [Catalogue..., 2010], the individual authors, including Miroshnikov [2011a, 2013, 2016], convincingly demonstrated that the catalogue contains an unprecedentedly high number of serious errors, very dubious and controversial data. Their total quantity is so great that it has not yet been counted. About 500 (!) various corrections and comments were listed only in two works by Miroshnikov [2011a, 2013]. It was thereby also observed [Miroshnikov, 2016] that the vast majority of serious errors and unfortunate imprecisions are condensed in the catalogue sections prepared by M.L. Danilevsky or under his first authorship. After the publication of a long series of papers by this researcher [Danilevsky, 2010a, 2011a, 2012a, b, c, d, 2013a, b, 2014a] with massive corrections of mainly his own mistakes and fictions, naturally reasonable question arises. In what way was the first edition of the catalogue compiled, in particular, the sections under Danilevsky's authorship? The lack of independent and rigorous reviewers from among experienced cerambycidologists with extensive knowledge of the Palaearctic fauna of longicorn beetles had a very negative impact on the overall quality of the catalogue, but specifically on the contents of the sections prepared by the mentioned author. As a result, the first edition (namely, its part under Danilevsky's authorship) actually appears to be only a careless, rough draft of the catalogue, but by no means a finished and full-fledged scientific product. Apparently, this is the only case in the modern history of entomology when a very solid work has been published in an authoritative publishing house in such an absurd and unsuitable form in terms of content.

It is also noteworthy that Danilevsky was the only one from among the authors of the first edition of the catalogue who began to hastily correct his own countless lapses in independent papers (ignoring the particular section allocated for this purpose in subsequent editions of the catalogue; see above), demonstrating the extremely unsatisfactory results of his previous work on the catalogue. Undoubtedly, the first edition of the catalogue will go down in the history of coleopterology as a vivid example of the egregious negligence and exceptional irresponsibility of its individual authors, such as M.L. Danilevsky.

In fact, a decade has passed already since the release of the first edition, and the second, updated and

revised volume edited by M.L. Danilevsky was published [Catalogue..., 2020], whereas the problems of the first one seemingly considered things of the past. On a cursory reading of the chapeau of the second edition, I immediately noticed a number of positive aspects. In particular, I was very impressed with the appearance of some new authors, respected specialists in cerambycidology. I also noted with satisfaction that the countries located on the territory of the Transcaucasia (namely, Georgia, Armenia, and Azerbaijan) are now treated in Asia and not in Europe, as before. Earlier, when I discussed some of the geographical aspects of the first edition [Catalogue..., 2010], it was pointed out [Miroshnikov, 2011a] that an unsuccessful approach of defining the boundary of Europe and Asia in the Caucasus was accepted in the catalogue. It was thereby noted that, despite the well-known controversial provisions on this issue, nevertheless, in the vast majority of the authoritative literary sources, at least the Transcaucasia (Georgia, Armenia, and Azerbaijan), is treated as part of Asia. In the second edition, it is not reported exactly what served out for this correct change. But I will hope that also my published comments have played their role in this progress.

Some time after my first, very brief reading of the catalogue, I deemed it necessary to carefully check the information contained in some of its sections, primarily those authored by Danilevsky. This was performed not only due to the very compelling reasons mentioned above but also under other important circumstances. Quite recently, I published an extensive review [Miroshnikov, 2016] of Danilevsky's [2015a] monograph "Longicorn beetles (Coleoptera, Cerambycoidea) of Russia and adjacent countries. Part 1". After a detailed examination of this work, I came to the conclusion that the number of serious errors, frank fictions, startling contradictions, distorted data, various omissions, and just misprints in it is so huge that they all together cannot be counted even approximately. In addition, based on the very numerous and diverse examples, I presented undeniable evidence of Danilevsky's outright manipulation with the published data and his deliberate concealment of the true information released by various authors, including myself. It was also demonstrated that the monograph under discussion contains non-existent references, which Danilevsky tried to use to disguise and justify numerous serious errors made in various his own publications, including the first edition of the catalogue. It is also noteworthy that this work [Danilevsky, 2015a] was not reviewed by anyone, while M.A. Lazarev is listed as its editor-in-chief, and in a very strange way thereby, – on the very last page of the book (and not on the title page). Sure

enough, this monograph will also go down in the history of coleopterology as a particularly negative example, very similar to that described above with the first edition of the Palaearctic catalogue.

The discussed Danilevsky's work has the most direct relation to the second edition of the catalogue, since it covers a vast territory (the countries of the former USSR plus Mongolia) occupying a very significant part of the Palaearctic. The information contained in the monograph (including the numerous wrong and dubious data) was undoubtedly widely used by this author in the preparation of the corresponding sections of the updated catalogue.

The aforementioned review was published in Russian, since Danilevsky's monograph [2015a], in relation to which critical remarks were made, has been also released in Russian. To my represent work in English, discussing the original Russian text as of Danilevsky, would appear not fully correct and even counterproductive. Otherwise, there would be a question raised on the general need for a very accurate translation of Danilevsky's Russian text into English. However, taking into account the many confusing statements of this author, often with serious errors of various characters and outright fictions, it would be extremely difficult to do so and hardly possible in several cases. As a result, my constructive criticism, unfortunately, turned out to be accessible only for Russian-speaking readers. But I dare to hope that the present work will help a wider circle of readers to form a definite opinion about the monograph by Danilevsky [2015a].

As a prelude to some of the remarks made below in the special part of the present work, I consider it necessary to specifically highlight a number of the most striking examples of wrong or dubious conclusions and comments of individual authors, including the editor, available in the second edition of the catalogue. This, as I hope, shall give the reader an overview of the methods and the style of work by Danilevsky, both the editor and one of the authors of the updated catalogue.

Already in the first sections of the second edition, it was hard not to notice the clearly pseudoscientific approaches by Danilevsky to the presentation of various references, where he openly operates double standards. Exactly the same approaches of this author were recently discussed in detail in my review [Miroshnikov, 2016] on his monograph [Danilevsky, 2015a]. Since then, absolutely nothing has turned for the better in this aspect. And this was quite expected. Thus, the "Taxonomy and Geography Remarks" section (pages 32–100) of the catalogue contains more than 500 comments, in the main part devoted to the diverse publications, including geographic and other data missing from the first edition. Thereby, even if a widely or very widely distributed taxon was newly recorded from just one country (for example, somewhere in Europe), despite the fact that it inhabits the vast majority of other European countries (including the adjacent countries), then such information (with reference to the original publication) necessarily is provided. In addition, the section contains references to those publications with records of taxa new to a particular part of a country that has already been listed in the first edition. The information that is meaningless at all for the catalogue is also presented. In particular,

Danilevsky considered it "extremely important" to provide the references even to works containing simply some previously unknown records from the territory of one or another, for instance, a European country, which was already specified in the previous version of the catalogue.

At the same time, despite the huge number of references to all possible publications, sometimes obviously useless ones for the catalogue, in the mentioned section ("Taxonomy and Geography Remarks") there was a more than modest space found for references to an only few of my original data. The references to the rest of my extensive original geographical information, often highlighting the most important aspects of the distribution of various taxa, and to many other valuable achievements in the knowledge of Palaearctic cerambycids, were deliberately and unceremoniously omitted by Danilevsky. Though, my original, published information was very widely used by this author without any explanations, citations and notes in his sections and therefore appeared in the updated catalogue as the results of his own research. In addition, it should be noted that in the section "Bibliographic information" of the catalogue (page XXIII) Danilevsky assures the reader as follows: "References are given for all primary sources of genus-group names, species-group names, tribal and subtribal names, as well as for most publications of new taxonomic and geographical information appeared after 2010 or for several older taxonomy and geography publications not sourced by the previous edition". In fact, his assurances in many cases turned out to be very far from the truth.

In the present review, I restored justice and introduced the required clarity by linking appropriate references to the original publications and demonstrating which facts were listed in the catalogue for this or that Danilevsky's information. Thereby the quite obvious and outrageous cases of plagiarism committed by this author are highlighted.

It is impossible not to pay attention to some of the data specified in the updated catalogue, which are difficult to adequately assess, especially from the point of view of the norms of ethics and morale. For instance, on page 63, there is the following comment: "#224 According to Lin Mei-Ying (personal message, 2018), the record of *Nanostrangalia torui* Holzschuh, 1989a for Hubei by N. Ohbayashi et al. (2004) was a misidentification".

The authorship of the cited comments in this context, as in other similar cases, undoubtedly belongs to Danilevsky, especially since Meiyang Lin is the sole author of the catalogue's China section, excluding Taiwan. Amazing what Danilevsky, particularly as the editor of the catalogue, in such an absurd form presents the quoted information which is directly concerning the three other authors of the same edition, namely, Drs M. Lin, N. Ohbayashi, and T. Niisato. It is hardly possible to understand the motives of these actions by Danilevsky, when he points to the wrong data of the other authors of the same catalogue and thereby refers to some personal messages of another author of the same work. And this is done instead of prompting the listed authors of the catalogue to make appropriate comments based on their own publications or data considered in the edition. In my opinion, this is exactly what the editor

was obliged to do, following the most elementary science ethics. However, this was the lack of control (first of all, by independent peer review of the manuscript) over his “creation” gave Danilevsky a sense of absolute freedom in his “editorial” activities. Until now, I have not come across in the scientific publications the similar cases of such an outrageous disrespect for the co-authors of the edition by its editor. Obviously, Danilevsky has once again created a peculiar negative precedent in the scientific entomological literature.

A number of comments presented in the catalogue may discourage the reader. Thus, for example, in the comments by Lazarev on page 16 the following is noted: “*Murzinia karatauensis* Lazarev, 2011 was described from Kazakhstan (Kzyl-Orda Region, Chiili District, North Karatau Ridge, Daut Mountain) after a single female with a raw (*sic!* = row? – A.M.) of semierect strong setae along ventral side of 3rd-4th antennal joints. The specimen is very similar to African *Monochamus (Ethiopiochamus) ruspator* (Fabricius, 1781). So, the real origin of the holotype is doubtful”. These comments, in my opinion, should not be taken as scientific information. If their author, Lazarev, does not understand the essence of his own formulations, then why did the editor, Danilevsky allowed these comments in the original format, resulting in complete bewilderment?! In fact, Lazarev and Danilevsky jointly propose the reader at least to decide himself on what to do with these taxa (i.e., the genus *Murzinia* and the species *M. karatauensis*) by simply hanging them in the air. The same two researchers highlight a number of other problems in the updated catalogue, which certainly require the reader to make some independent decisions, as, for example, did Tavakilian and Chevillotte [2021] in relation to the two mentioned taxa.

In this regard, the natural question arises. Should an updated catalogue (namely, Danilevsky’s and Lazarev’s information in it) be considered a serious scientific edition and an authoritative taxonomic handbook, or is it just a collection of crosswords and rebuses? I do not have an unambiguous answer to this question! Further, in the special part, I undoubtedly made attempts to free the unprepared reader from marking independent decisions in relation to the above problems and made the appropriate proposals.

In a number of cases, some data on the distribution of taxa given by Danilevsky and Lazarev cannot be recognized as anything other than falsification. One of the striking examples of this approach is their allegations about the distribution of *Miniprionus pavlovskii* and some other taxa in Afghanistan, debunked by me below. The circumstances under which Danilevsky has “reported” *Anoploclera sexguttata* from Georgia and Transcaucasia as a whole both in the first and in the second edition of the catalogue turned out to be just amazing. These circumstances are also related to a falsification of bibliography data by him. Many other data on the distribution of various taxa listed by Danilevsky are, at the very least, causes for bewilderment. These issues are discussed in detail in a special part below, and the corresponding conclusions are given.

The irresistible desire of Danilevsky to recognize the validity of some of the taxa described by him, but rightly synonymized by various authors and firmly entered in the

scientific literature as synonyms, in the catalogue looks understandable but completely incorrect. For example, it was quite convincingly demonstrated [Sama, 2010] that *Purpuricenus caucasicus baeckmanni* Danilevsky, 2007, described from the Southern coast of Crimea, is a junior synonym for *P. renyvoniae renyvoniae* Sláma, 2001 (now *P. renyvoniae*). This synonymy was widely accepted in several subsequent publications with some explanations [Rapuzzi, Sama, 2014; Prokopov, Turbanov, 2016; Miroshnikov, 2018a; and others]. Even before the publication of the original description of *Purpuricenus caucasicola* Danilevsky, 2015 [Danilevsky, 2015b], the artificiality of this taxon, which I consequently synonymized with *P. neocaucasicus* Rapuzzi et Sama, 2014 [Miroshnikov, 2017a], was quite obvious. A very curious story preceding the description of *P. caucasicola* was subsequently explained [Miroshnikov, 2018a] since the type series of it consists mainly of specimens collected by me in the Northwest Caucasus.

However, if Danilevsky only listed the aforementioned *Purpuricenus* species in the updated catalogue as valid taxa without any further explanations and required references, then his remarks regarding to some of the taxa described by himself look amazing. In particular, this author provides the bookmark #45 to the following text (on page 268) prepared by Dr Meiyang Lin (within the framework of the section on China, in which she acts as the sole author, as mentioned above):

“[*Molorchus (Molorchus)*] *liui* Gressitt, 1948a: 51 [Yunnan] A: GAN HUB HUN SCH SHA YUN ZHE #45 *smetanai* Danilevsky, 2011f: 105 [Zhejiang] #45”.

In the comments to this reference on page 38, Danilevsky notes that “According to Holzschuh (2013a), *Molorchus liui* Gressitt, 1948a [Yunnan] = *Molorchus (Nathrioglaphyra) smetanai* Danilevsky, 2011f [Zhejiang]. But the distance between type localities makes such synonymy doubtful”.

Danilevsky’s attempts to convince the reader on the erroneous nature of the synonymy, referring only to some kind of “distance between the type localities” of these taxa, seem completely inconvenient even to an inexperienced reader. This author, awkwardly rescuing the species he described, does not mention any morphological differences between the taxa in discussion and does not at all take into account the indisputable facts of wide to very wide distribution of a huge number of coleopterous insects in various regions, including China. Thereby Holzschuh [2013] clearly indicated that *M. liui* (= *M. smetanai* Danilevsky) inhabits not only provinces of Yunnan and Zhejiang, but also Sichuan, Gansu, Shaanxi, and Hunan. Furthermore, this species has also been recorded from Hubei Province [Hua, 2002].

The absurdity of this case, however, is located elsewhere. It is my firm belief that any such catalogue, being a purely scientific, taxonomic, and geographical handbook for one or another systematic group, should contain information verified and agreed with all the authors of the edition. The catalogue is not a place for discussions between co-authors of the same work and is not an arena for clarifying possible disagreements between members of the team. These problems must, by all means, be levelled out before publication (which is, by the way, the direct duty

of the editor) and not appear on the pages of the edition to the strict judgment of the reader. In addition, the dispute started by Danilevsky exclusively unilaterally, in particular as an editor of the catalogue, appears once again very unworthy in relation to other authors of the same book and their conscientious and fruitful work. Again, I am forced to state, as above on other reasons, that until now, I have not come across such negative precedents in modern scientific literature, especially in catalogues and reference books.

Instead of directly editing the text of the edition and reconciling the relevant data with the authors of sections, Danilevsky, being uncontrolled editor of the publication, on every occasion only convenient to him, shamelessly crawls into the original text of other authors and adds his own comments, entering into contradictions with them. It seems doubtful that the authors of the catalogue had a real opportunity to discuss in detail with Danilevsky, the draft of all his comments and express their own opinions on the topics in question. I do not presume to argue, but perhaps these authors were able to get acquainted with these or those editor's comments after the publication of the book only.

With regard to the above remarks, the following should also be noted. The introduction to the catalogue (page XIII) states that "A special effort has been made to achieve agreements of the co-authors in problematic taxonomic cases", as well as "Personal opinions of the authors are given, if necessary, in the chapter "New Acts and Comments". But the edition does not have a chapter "New Acts and Comments", as indicated above, but there is a chapter "New Nomenclatural, Taxonomic and Geographical Acts, and Comments". However, a whole series of comments by Danilevsky, unilaterally challenging the data by other authors of the catalogue, is given in a fully different chapter, namely in "Taxonomy and Geography Remarks". Thus, the cited information presented in the introduction does not correspond to reality, and Danilevsky, primarily as an editor, himself violates these assurances.

The numerous intraspecific forms of various polymorphic species listed in the catalogue and presented by Danilevsky and Lazarev as their authored "subspecies" cause deep disappointment. Their number in one or another species in such genera as, for instance, *Cortodera* and *Dorcadion*, is just amazing. In particular, only for *D. cinerarium* 22 "subspecies" are listed by these authors, and for *D. scabricolle* – even 39! *Cortodera colchica* has 17 "subspecies". The listing of similar examples could easily be continued. In general, the vast majority of "subspecies" of one or another species differ only in features that can be diagnosed exclusively by Danilevsky and Lazarev, while the distribution of these "subspecies" is very misty and often scientifically inexplicable. A whole list of examples in this aspect have been discussed in the above review on Danilevsky's monograph [2015a] relatively recently [Miroshnikov, 2016].

In my opinion, such a practice of taxonomic research, which carries a large layer of dubious or completely inconclusive results, as a whole, causes significant harm to the systematics of Palaearctic longicorn beetles. The quality of the discussed updated catalogue, filled with dubious information, suffers greatly, even not taking into account all the other negative facts considered here.

Sometimes, Danilevsky is so keen on splitting some species into "subspecies" that it not only becomes a subject of true bewilderment but also looks absurdly. Thus, for instance, on page 3 in the subsection "Resurrections (from synonymy)" he states that "*Cerambyx cerdo manderstjernaе* Mulsant & Godart, 1855b is a valid name for a subspecies from Crimea and Black Sea coast (Sochi environs); antennae relatively shorter than in the nominative subspecies or in *C. c. acuminatus*". Further, on page 215 of the special part, Danilevsky provides the following information:

"*Cerambyx* (*Cerambyx*)

cerdo acuminatus Motschulsky, 1853: 79 ["De la Georgie et des pays limitrophes de la mer Caspienne"] E: ST UK A: AB AR GG IN IQ IS JO LE SY TR #279

klinzigi Podany, 1964c: 88 #279

cerdo cerdo Linnaeus, 1758: 392 ["Italya"] E: AL AU BE BH BU BY CR CT CZ FR GBi GE GR HU IR IT LA LU MA MC MD ME NL PL RO SB SK SL ST SV SZ TR UK N: MO A: TR

heros Scopoli, 1763: 51

cerdo iranicus Heyrovský, 1951: 156 A: IN #279

cerdo manderstjernaе Mulsant & Godart, 1855b: 280 [= 1855a: 180] ["la Crimée"] E: ST UK

Exactly how should the reader perceive this information, when Danilevsky completely omitted all of the necessary explanations about the peculiarities of the distribution area of most subspecies of *Cerambyx cerdo*, including the "subspecies *C. cerdo manderstjernaе*"? In what way the distribution area of this taxon, according to Danilevsky, from Crimea and the Black Sea coast of Krasnodar Region near Sochi, fits into the wide distribution area of *C. cerdo acuminatus*, covering, according to his own data, "E: ST UK A: AB AR GG IN IQ IS JO LE SY TR", including the same Crimea, the whole Caucasus and all of their Black Sea coast? In order to somehow delineate the distribution area of "*C. cerdo manderstjernaе*" in this situation, the reader needs to have an extremely rich imagination! I couldn't do so, no matter how hard I tried. It should also be noted that Plavilstshikov [1940: 95, 636] already a long time ago clearly pointed on the distribution area of *C. cerdo acuminatus* as covering "Crimea, Caucasus, Transcaucasia, northern Iran, Turkish Armenia (now eastern Anatolia. – A.M.), Asia Minor, Syria". In addition, I have at my disposal a rich *Cerambyx cerdo* material (cAM), especially from the Black Sea coast of Krasnodar Region and Crimea, on the base of which it is in no way possible to justify the validity of "*C. cerdo manderstjernaе*" in order to support the conclusions by Danilevsky. The matter is expected to be discussed in more detail in the near future.

At the same time, Danilevsky is so overwhelmed with the desire to synonymize the taxa described by other researchers, including his longtime opponents, that he is ready to implement double standards again here, as in the other cases as pointed on above. Literally on the previous page (page 2) (i.e., next to his notes on *Cerambyx cerdo* subspecies on page 3), Danilevsky synonymized *Xylosteus bartoni migliacciosi* Rapuzzi et Sama, 2018 with *Xylosteus bartoni* Obenberger et Mañan, 1933, noting that "...the type locality (Rila Mt.) of *Xylosteus bartoni* is just inside the area of *X. b. migliacciosi* Rapuzzi & Sama, 2018". As a result, it turns out that in some cases, Danilevsky directly appeals

to existence of independent but very limited distribution area of one subspecies (*Cerambyx cerdo manderstjerna*) within the wide distribution area of another (*C. cerdo acuminatus*), while in other cases (as with *Xylosteus bartoni* and *X. b. migliaccioi*) similar ideas about the geography of taxa seem categorically unacceptable for him. Without discussing the correctness of the above indicated synonymy itself, here one should consider a fundamentally opposite side of this particular case and state the completely obvious inconsistency and bias of Danilevsky's in his conclusions and opinions.

In addition, Danilevsky hastily establishes the synonymy not merely based on the original descriptions of certain taxa only, without studying the corresponding type specimens, but, most remarkably, through performing very dubious actions. Thus, for example, directly on the first pages of the catalogue, he stated the following: “*Agapanthia detrita* Kraatz, 1882c = *A. paki* Rapuzzi, 2012, syn. nov. based on the original description of *A. paki* and series of *A. detrita* from Tadjikistan, Uzbekistan, Kazakhstan and Kyrgyzstan” (page 1). It is striking that Danilevsky compares the species (*Agapanthia paki*), described from central Afghanistan, with specimens of a similar species (*Agapanthia detrita*), derived from other countries. This practice is unlikely to be credible. But here, different circumstances are the main ones. The type locality of *Agapanthia detrita* (Samarkand, Uzbekistan) is so remote from the type locality of *A. paki* (Chagcharan, central Afghanistan) that this fact if strictly following the original theory of Danilevsky himself described above (the cautionary tale with *Molorchus smetanai*), makes this synonymy unacceptable. Again, the double standards of Danilevsky are quite clearly traced! Furthermore, it is important to note one even more very significant fact of a different character. The species described by Pierpaolo Rapuzzi was published in an unreviewed journal “Humanity space. International almanac”, of where Danilevsky is one of the editors (responsible for the entomology section). He, as an editor, accepting the corresponding manuscript for consideration and publication, was obliged to express the critical remarks to the author, observing the principles of scientific ethics. But, almost without a doubt, this was not done, as can be seen from the text of Rapuzzi's publication. Once more, this situation, as in the previously mentioned cases, is difficult to consider outside the plane of elementary moral and ethical norms.

Sometimes, Danilevsky's passion for proposing a new synonymy is so great that he even repeatedly synonymize some taxa which have already been considered in this quality in his own recent publications. For example, literally on the first and second pages of the catalogue, he reports that “*Diboma bhutana* Breuning, 1975a (March) = *Diboma bhutanensis* Breuning, 1975d (October), syn. nov. (now in *Zotalemimon* Pic, 1925a)” (page 1), “*Dorcadion theophilei* Pic, 1898h = *D. kadleci* Bernhauer & Peks, 2016, syn. nov. based on the original description” (page 2), while a few years earlier this synonymy had already been proposed [Danilevsky, 2014a, 2017].

At the same time, Danilevsky is not in a hurry to recognize the correct synonymy proposed by other authors, openly demonstrating the distrust of their research results.

Thus, for instance, on page 445, he presents the following information: “[*Pilemia (Pseudopilemia) buglanica* D. Marklund & S. Marklund, 2014: 276 A: TR #241” (i.e. this taxon is given as valid), and on page 65 completely aimlessly notices that “New synonyms were proposed by Kasatkin (2018): *Phytoecia (Pseudopilemia) hirsutula* (Frolich, 1793) = *Ph. (P.) buglanica* D. Marklund & S. Marklund, 2014”, without discussing this synonymy at all. As a result, this situation looks exclusively as the following: everything that Danilevsky does, the reader is invited to accept *a priori*, and everything that other scientists (thereby the well-known, recognized cerambycidologists) create, appears in Danilevsky's opinion as highly dubious and unconvincing. This consistently attitude of arrogance towards colleagues and their work by Danilevsky can be noticed in many other cases in the catalogue, including those mentioned afore.

It is worth particularly mentioning that many, often serious mistakes and inaccuracies made by Danilevsky in the first edition of the catalogue were not corrected or at least discussed in the updated edition. The main reasons for this are perfectly understandable and easy to explain. If this author dared to correct some of his own mistakes and comment on his highly dubious data, then he would have to constantly refer to my, in fact very inconvenient to him, conclusions and critical remarks [Miroshnikov, 2011a, 2013, 2016]. In this case, Danilevsky would have to admit not only the erroneous information presented by himself but also his absolute myths and numerous manipulations with the data published by various authors. In addition, he would also have to admit his frank concealment of the most important results published by various researchers directly related to the problematic and controversial issues considered in the catalogue. Alas, not every researcher is capable of such decisive actions. Danilevsky, naturally, avoided solving these acute problems, leaving the reader of the updated catalogue a legacy of a large series of previous mistakes.

At the same time, it is not surprising that as soon as the updated Catalogue was published, Danilevsky [2021] immediately began to hastily correct his own numerous mistakes and omissions made in it. Therefore, it is very likely that a similar story should be expected, as in the case of the first edition [Catalogue..., 2010], and this author will again involve the reader in the long and exhausting process of correcting his own endless mistakes and inventions.

Concerning some of the organizational questions of preparing an updated catalogue, I should return to the composition of its team of authors. A very strange and unexpected fact is revealed here. Instead of well-known, respectful cerambycidologist Andreas Weigel (Wernburg, Germany), the author of the section on Afghanistan, Pakistan, Nepal, Bhutan, and the Indian part of the Himalayas in the first edition of the catalogue, in a strange way Lazarev has appeared, who is an apprentice of the editor, Danilevsky. In a personal message (dated January 31, 2021) I asked Andreas Weigel if he received an invitation from the publisher or the editor of the second edition to prepare an updated section for the same geographical region. This researcher, in his personal message (dated February 1, 2021), informed me that he had not received

any appropriate invitation from anyone, not even just information about the upcoming second edition. Moreover, Andreas Weigel was not familiar with his exclusion from the authors' team before the publication of the second edition. In my opinion, this is just an outrageous act with substitution of the author. Naturally, since this particular geographical section was "prepared" by Lazarev, it appears as almost a pure compilation, and not an independent original work, thereby with many mistakes. This issue is discussed in more detail below.

I know Andreas Weigel for a long time as a humble and decent person. I believe that these were qualities that prevented him from public protest action about this manipulation with the substitution of the author and attracting the attention of the scientific community to this nasty case.

Summarizing the introductory part, I would like to clearly outline some of the objectives of the work in question. The main ones are to draw the attention of the reader of the updated catalogue to a lot of wrong and controversial data it contains and to assist him to take these mistakes and inconveniences into account when working with the publication, as well as to provide the reader with the opportunity to familiarize himself in detail with the primary sources hidden from his eyes, the most important information of which was very widely used in the catalogue but the references to these original publications themselves were deliberately "omitted" by Danilevsky, contrary to elementary scientific ethics.

The material treated in this work belongs to the following institutional and private collections:

BMNH – Natural History Museum (London, United Kingdom);

ZIN – Zoological Institute of the Russian Academy of Sciences (St. Petersburg, Russia);

ZMMU – Zoological Museum of the Moscow State University (Moscow, Russia);

cAM – collection of Alexandr Miroshnikov (Krasnodar, Russia).

Results and discussion

First of all, it must be acknowledged that the approaches to the authorship in the new catalogue ("Editor and Authors" section, page XVI) had unfortunately remained the same as in the first edition [Catalogue..., 2010]. The distribution of authorship given by countries and regions and not by taxonomic groups, in a large number of cases makes it almost impossible to identify a particular author. Earlier, Miroshnikov [2011a] drew attention to this fact in the monograph "The longicorn beetles (Cerambycidae) in "Catalogue of Palaearctic Coleoptera. Stenstrup, 2010". Remarks and additions". Many species have a very extensive distribution area in the Palaearctic from Western Europe to various territories in Asia. A significant number of Asian species inhabit, for instance, Russia, China, and Korea, or, in addition, Japan and/or Mongolia. Various species are known, for example, from Iran, Afghanistan, and Pakistan, or besides it from some adjacent countries. Examples like these could easily be continued. Who should be considered the author(s) of the information about these taxa, based

on the relevant data of the new catalogue (page XXVI)? Who is (are) the author(s) of the text on supraspecific taxa when they include species (or subspecies) from the most diverse regions? I did not find the proper explanations in the catalogue.

The commitment of the editor, Danilevsky, to the allocation of longicorn beetles into a separate superfamily Cerambycoidea [Danilevsky, 2015a] is reflected in the same section "Editor and Authors" (page XXVI), where this name was erroneously given by him instead of the superfamily Chrysomeloidea presented in the original name of the edition and the "Contents" section.

Coming up next, following mainly to the original pagination of the catalogue, my remarks in relation to the various quoted information are presented. Some comments regarding catalogue data contained on a wide variety of pages, but related to each other by the similar problems and aspects, are given in the separate joint remarks. It should be thereby noted that in many cases, it is not possible to identify the authorship of comments or some other data (see above). Therefore, in these cases, they are considered here as information edited by Danilevsky.

– **Page 1.** "New Nomenclatural, Taxonomic and Geographical Acts, and Comments".

Remarks. This section of the catalogue unambiguously stipulates that each of its authors presents in its exclusively own original information and not already published results of other authors. However, the data provided by Danilevsky in the "Unavailable Names" section on pages 7, 8 and 9 and in the "Spelling" subsection on page 10 clearly indicates the opposite.

– **Page 7.** M.L. Danilevsky: "*Leptura (Pachytodes) cerambyciformis* var. *fauconneti* Pic, 1916 («Saône-et-Loire») and *Leptura (Pachytodes) cerambyciformis* var. *martialis* Pic, 1916 («Saône-et-Loire») were proposed for one population and so unavailable".

Remarks. Miroshnikov's paper [2013] published as a supplement to the above-mentioned monograph [Miroshnikov, 2011a] with corrections and refinements to the first edition [Catalogue..., 2010], also contains a lot of diverse comments. These both works are well known to Danilevsky. However, he only once substantively refers to one of them (page 59), mentioning these publications in the introduction on page XIII and the list of references on page 598 and nothing more.

At the same time, Miroshnikov [2013: 13] points out the following: "– p. 108. *Pachytodes cerambyciformis* Schrank, 1781a: 154 (*Leptura*)...

fauconneti Pic, 1916: 4 (*Leptura*)

martialis Pic, 1916: 4 (*Leptura*).

These names are published in the same work as follows: *Leptura (Pachytodes) cerambyciformis* var. *fauconneti* Pic, 1916: 4 ("Saône-et-Loire").

Leptura (Pachytodes) cerambyciformis var. *martialis* Pic, 1916: 4 ("Saône-et-Loire").

Given that the author of these two epithets definitely gave them an infrasubspecific rank and described variations of *P. cerambyciformis* from the same population... both names should be considered as an unavailable".

Therefore, Danilevsky's information quoted above is pure plagiarism.

– **Page 8.** M.L. Danilevsky: “*Phytoecia nigripennis* Jakobson, 1924c was originally published as *Phytoecia erivanica* ab. *nigripennis* Jakobson, 1924c, and so unavailable (a replacement name for *Phytoecia nigritarsis* Pic, 1895b)”.

Remarks. In the same paper by Miroshnikov [2013: 19] there is the following note: “– p. 302–304. *Phytoecia (Kalashania) erivanica* Reitter, 1899: 161...

nigripennis Jakobson, 1924c: 239.

This name was originally published by Jacobson as a replacement one and as an aberration: *Phytoecia erivanica* ab. *nigripennis* Jakobson, 1924: 239”.

Therefore, Danilevsky’s data quoted above are pure plagiarism.

– **Page 9.** M.L. Danilevsky: “*Stenopterus ater* var. *biskrensis* Dayrem, 1922 («Biskra») was described together with *Stenopterus ater* var. *atrurufus* Dayrem, 1922 («Biskra») and two more variations were mentioned in same population, so the author expressly gave to both names infrasubspecific rank (Art. 45.6.4.)”.

Remarks. In the same Miroshnikov’s [2013: 19] work there are the following comments: “– p. 205. *Stenopterus ater* Linnaeus, 1767: 642 (*Necydalis*)...

biskrensis Dayrem, 1922: 28.

In establishing the suitability of this name, it must be borne in mind that it was published in conjunction with another epithet (omitted in the “Catalogue” (2010. – *A.M.*)) as follows:

Stenopterus ater var. *biskrensis* Dayrem, 1922: 28 (“Biskra”)

Stenopterus ater var. *atrurufus* Dayrem, 1922: 28 (“Biskra”).

Given that the author of these two epithets definitely gave them an infrasubspecific rank and described variations of *S. ater* from the same population... both names should be considered as an unavailable”.

Therefore, Danilevsky’s information quoted above is pure plagiarism.

– **Page 10.** M.L. Danilevsky: “*Leptura vcranica* Laxmann, 1770 described from “*Russiae australis*” [Ukraine] was an incorrect original spelling – “lapsus calami” (Art. 32.5.), and must be corrected as *Leptura ucranica* Laxmann, 1770”.

Remarks. The above-mentioned monograph by Miroshnikov [2011a: 38] contains the following notes: “– p. 116 (“Lepturinae”). “*Strangalia attenuata* Linnaeus, 1758: 398 (*Leptura*)”.

Notes. Name *Leptura attenuata* var. *brunnescens* Balbi, 1892: 49 (“dintorni di Helen dorf, Swanetien nel Caucaso”), which is absent from the “Catalogue” (2010. – *A.M.*), should be considered as a synonym of *Strangalia attenuata*. The following should also be noted concerning the synonymy of *S. attenuata*. Studying the original description of *Leptura ucranica* [Laxmann, 1770: 596, tab. 24, fig. 6 (“*vcranica*”) (“*Russiae australis*”)], I found a clear similarity between this taxon and *S. attenuata*. In this regard, the following synonymy seems to me very likely: *Strangalia attenuata* Linnaeus, 1758 = *Leptura ucranica* Laxmann, 1770, as well as *Leptura ucranica* Laxmann, 1770 = *Leptura attenuata* var. *maculicollis* Gerhardt, 1910. The name *ucranica* Laxmann, 1770 has

not been used in the literature for a long time and its last mention is only in the “Index Animalium” (Sherborn, 1902: 1009, 1134)”.

Therefore, Danilevsky’s data quoted above are pure plagiarism, and on page 150, which listed the epithet “*ucranica* Laxmann, 1770: 596”, he deliberately omitted the reference to Miroshnikov’s monograph.

– **Page 3.** M.L. Danilevsky: “*Phytoecia (Musaria) puncticollis persica* Ganglbauer, 1884... was collected by S. Dementiev (Moscow) in South Iran (Lorestan, about 20km S Ezna, 16–17.5.2017, 33°16’46”N, 49°29’57”E, 2220m). About 1/3 of all specimens were totally black, others have red or partly red prothorax and red 1st antennal joint... (and so on. – *A.M.*)”;

– **Page 11.** M.L. Danilevsky: “*Phytoecia (Musaria) puncticollis persica* Ganglbauer, 1884... was collected by S. Dementiev (Moscow) in South Iran (Lorestan, about 20km S Ezna, 16–17.5.2017, 33°16’46”N, 49°29’57”E, 2220m). About 1/3 of all specimens are totally black, others have red or partly red prothorax and red 1st antennal joint... (and so on. – *A.M.*)”.

Remarks. It is very strange that Danilevsky provides the same information in detail regarding this taxon on pages 3 and 11.

In addition, given the entirely black colouration of many specimens of *Ph. puncticollis persica* from Luristan, Iran, and exactly the same colouration of some specimens from various Iranian localities (see above), the validity of *Ph. (Musaria) krupitskyi* Danilevsky, 2014 (in Catalogue [2020: 4] as “*Ph. (M.) puncticollis krupitskyi* Danilevsky, 2014, stat. nov.”), described from a single completely black male collected in the Turkish province of Hakkâri [Danilevsky, 2014b: Ağaçdibi, 37°30’02”N, 43°46’49”E], seems very doubtful to me. So, *Phytoecia (Musaria) puncticollis persica* Ganglbauer, 1884 = = ? *Ph. (M.) puncticollis krupitskyi* Danilevsky, 2014.

– **Page 16.** M.A. Lazarev: “*Murzinia karatauensis* Lazarev, 2011 was described from Kazakhstan (Kzyl-Orda Region, Chiili District, North Karatau Ridge, Daut Mountain) after a single female with a raw (*sic!* = row? – *A.M.*) of semierect strong setae along ventral side of 3rd-4th antennal joints. The specimen is very similar to African *Monochamus (Ethiophochamus) ruspator* (Fabricius, 1781). So, the real origin of the holotype is doubtful”.

Remarks. These comments have already been briefly discussed above. The original description of Lazarev’s [2011] has long appeared dubious to me. In addition, when studying the holotype of *M. karatauensis* stored at ZMMU, I did not manage to find any clear distinguishing features from various representatives of the genus *Monochamus* Dejean, 1821. Lazarev himself only modestly notes that “*Murzinia karatauensis*, gen. nov., sp. nov. ... is not similar to any other taxon”, while the holotype bears the additional label “*Monochamus* sp. det. S. Murzin”.

I sent an image of the holotype to Dr Karl Adlbauer (Graz, Austria) and asked his opinion on the specific attribution of the taxon in question. He kindly informed me (his personal message from February 3, 2021) that this is *Monochamus (Ethiophochamus) ruspator* (Fabricius, 1781). I have also compared the holotype with high resolution

images of some well-identified specimens of this species and therefore fully support Dr Adlbauer's authoritative opinion.

On this basis, *Monochamus* Dejean, 1821 (subgenus *Ethiopicochamus* L.S. Dillon et E.S. Dillon, 1961) = *Murzinia* Lazarev, 2011, **syn. n.**; *Monochamus* (*Ethiopicochamus*) *ruspator* (Fabricius, 1781) = *Murzinia karatauensis* Lazarev, 2011, **syn. n.**

– **Page 32.** M.L. Danilevsky: "#1 Danilevsky (2014i): ...*Brachyta* (s. str. *interrogationis mannerheimii* Motschulsky, 1860b..."

Remarks. Danilevsky besides page 32 uses the name *mannerheimii* twice more, in particular, on pages 46 and 155.

Miroshnikov's publication [2013: 14] there is the following note: "– p. 120. *Brachyta interrogationis* Linnaeus, 1758: 398 (*Leptura*)

mannerheimii Motschulsky, 1860b: 148 (*Evodinus*).

This taxon is described as *Evodinus mannerheimii*. Therefore, according to article 33.4 of the ICZN, the name *mannerheimii* should be considered as an incorrect subsequent spelling".

Danilevsky deliberately omitted the above-mentioned reference on page 155 bearing the taxon in question, but thereby he had provided a link to his publication ("#1 Danilevsky (2014i): ..."), posing the results of Miroshnikov's research as his own.

In addition, Miroshnikov [2016: 183] convincingly demonstrated that Danilevsky's monograph was actually published in 2015, while the author himself presents this work as allegedly published in 2014. Therefore, the date of publication of all taxa described in this monograph (1 genus, 5 subgenera, 1 species and 27 subspecies) should be considered 2015 [Miroshnikov, 2016: 183].

– **Page 46.** M.L. Danilevsky: "#98 *Purpuricenus graecus* Sláma, 1993, *P. renyvona* Sláma, 2001... and *P. neocaucasicus* Rapuzzi & Sama, 2013 were accepted as valid species names by Danilevsky (2015e)".

Remarks. Must be: *P. neocaucasicus* Rapuzzi & Sama, 2014... [Rapuzzi, Sama, 2014].

– **Page 55.** M.L. Danilevsky: "#169... According to Lin Mei-Ying (personal message, 2018), Sichuan was recorded before for *G. (Gnathostrangalia. – A.M.) simianshana* because Chongqing was not separated from Sichuan before 1997. The species is only known from the type locality up to now, though Sichuan is very possible for this species";

– **Pages 125–126.** "genus *Gnathostrangalia... simianshana* Chiang & L. Chen, 1993: 54 A: CHQ ?SCH #169";

– **Page 97.** M.L. Danilevsky: "#489 According to Lin Meiying (personal message dated 11.2.2020), several records for Zhejiang were published in Chinese books: *Leptura ambulatrix* Gressitt, 1951; *Sinostrangalis ikedai* (Tamanuki et Mitono, 1939)... (further 11 more taxa are recorded from various Chinese provinces. – *A.M.*)".

Remarks. It is very strange that Danilevsky, contrary to scientific ethics, on his own behalf, presents these references and comments regarding Chinese taxa, despite the fact that Dr Meiying Lin is the only author of the section on China and not an outside researcher.

On the whole, such Danilevsky's references to the personal messages of the authors of the catalogue seems absurd.

– **Page 64.** M.A. Lazarev: "#233 *Miniprionus pavlovskii* (Semenov, 1935b) undoubtedly penetrates to Afghanistan that was supposed in the description of the genus (Danilevsky, 2000d)";

– **Page 78.** M.A. Lazarev: "#345 Lazarev (2019a) recorded 11 taxa for Afghanistan, which were not mentioned for Afghanistan by Weigel (2010): *Miniprionus pavlovskii* (Semenov, 1935b)...";

– **Page 114.** M.A. Lazarev (data for Afghanistan), M.L. Danilevsky (data for Tajikistan): "genus *Miniprionus... pavlovskii* Semenov, 1935b: 239 (*Prionus*) A: AF TD (Kulyab env.) #233 #345".

Remarks. In these notes of mine, the information is discussed not only for *Miniprionus pavlovskii*, but also for some other species given for Afghanistan one way or another. In almost all cases considered below, very similar Danilevsky's and Lazarev's approaches to the presentation of the relevant data are clearly manifested.

In the cited Lazarev's publication ("2019a"), *M. pavlovskii* is noted as follows: "*Miniprionus... pavlovskii* Semenov, 1935: 239 (*Prionus*)

Danilevsky, 2000: 190 – North Afghanistan".

However, Danilevsky [2000: 190] indicates Afghanistan for this species without providing any evidence for this record: "*Miniprionus pavlovskii... distributed in South Tadzhikistan and surely in North Afghanistan.*" Therefore, Weigel, Drumont, and Komiya [Catalogue..., 2010: 92] were entirely correct to not include this country in the distribution area of *M. pavlovskii*. In addition, in a relatively recent monograph by Danilevsky [2015a: 64], the distribution of *M. pavlovskii* is given as follows: "Известен только из окрестностей Куляба в южном Таджикистане" (is known only from the vicinities of Kulyab in southern Tajikistan). It is very strange that Lazarev does not quote this Danilevsky's work for *M. pavlovskii* either in his above paper [Lazarev, 2019a] or in the catalogue, unlike other species. It is no less strange that Danilevsky himself also did not provide a reference to his own recent work [Danilevsky, 2015a], in which he indicated the distribution of this species pretty clear.

A similar situation is observed with respect to Lazarev's data edited by Danilevsky on the distribution of *Osphranteria coerulea* L. Redtenbacher, 1850. This species was noted by Lazarev also in comment #345 (page 78), and on page 200 by the same author (data for Afghanistan and Pakistan) and by Danilevsky (data for Iran, Iraq, and Turkey) as follow: "genus *Osphranteria... coerulea* L. Redtenbacher, 1850: 50 A: AF IN IQ PA TR #30 #345". In cited Lazarev's [2019a] paper, which he presents as a primary source, the following information is given: "*Osphranteria... coerulea* L. Redtenbacher, 1850: 50... Note. The species must occur in Afghanistan as it is distributed in Pakistan and in Iran" and nothing more.

Thus, the statement about the distribution of *Miniprionus pavlovskii* and *Osphranteria coerulea* in Afghanistan presented by Lazarev edited by Danilevsky, is undoubtedly a falsification, while the opposite data above by Weigel, Drumont, and Komiya [Catalogue..., 2010: 92],

Weigel, Sama, and Löbl [Catalogue..., 2010: 149] are reliable. Therefore, Afghanistan (AF) should be excluded from the distribution area of these two species.

It is hardly possible to justify the methods described above for constructing the distribution area of taxa used by Danilevsky and Lazarev. Most regrettably, as practice shows, such falsified data will be replicated in the future without discussing the original publications, and after a certain time they will be accepted at all in the literature as facts.

Attention should also be drawn to other highly doubtful data on the distribution of some species in Afghanistan presented by Lazarev and Danilevsky. All in the same comment by Lazarev #345 (page 78), another species is noted, namely *Pedostrangalia imberbis* (Ménétriés, 1832), which requires discussion, and on page 140 by the same author (data for Afghanistan) and by Danilevsky (data for Azerbaijan, Iran, and Turkmenistan) stating the following: “genus *Pedostrangalia... imberbis* Ménétriés, 1832: 231 (*Leptura*) A: AB AF IN TM #345”. Thereby the cited Lazarev’s paper [2019a] noted the following data: “*Pedostrangalia... imberbis* Ménétriés, 1832: 231 (*Leptura*)... Özdikmen, 2004: 24, 28 – Afghanistan”.

Various authors [Plavilstshikov, 1936; Danilevsky, Miroshnikov, 1985; and others] pointed on *P. imberbis* as a species with a specific distribution area (characteristic for a number of different taxa as well), covering only the Talysh Mountains in Azerbaijan, the Alborz and Southwest Kopet Dag in Iran, and the Western Kopet Dag in Turkmenistan. The record of this species from Afghanistan [Özdikmen, 2004], the boundaries of which are far from the delineated distribution area, least needs further confirmation. Thereby Danilevsky, both in the first edition [Catalogue..., 2010: 110] and in his recent monograph [Danilevsky, 2015a: 296], ignored Özdikmen’s data, describing in detail only the previously known distribution area indicated above. Under these circumstances, it is extremely strange that Afghanistan was recorded for *P. imberbis* even without a question mark, while Danilevsky did not comment on this data in any way.

A fairly similar situation is observed for another species, namely *Rutpela inermis* (J. Daniel et K. Daniel, 1898), which has approximately the same delimited distribution as *P. imberbis*. Lazarev [2019a] listed this species for Afghanistan based on the only Heyrovský’s record [1971] from the north-west of Afghanistan (Herat). In fact, this record, as in the case with the previous species, requires reliable confirmation. Danilevsky in the first edition [Catalogue..., 2010: 112] completely ignored Heyrovský’s data, and in a recent work [Danilevsky, 2015a: 339] he quoted them without any explanation. At the same time, however, he argued that *R. inermis* is “Эндемик южного Прикаспия” (endemic to the southern near-Caspian)! Again, it seems very strange that Danilevsky once more refused any comments about the distribution of *R. inermis* in Afghanistan.

In my opinion, if this country is to be added to the distribution area of *Pedostrangalia imberbis* and *Rutpela inermis*, then this should only be done using a question mark. Thus, the distribution of these two species can be noted as: A: AB (Talysh) ?AF IN TM (Kopetdag).

Even more striking than in the cases examined above is Lazarev’s information edited by Danilevsky, with a discussion of the distribution of some other taxa in Afghanistan. Thus, Lazarev on page 93 notes: “#454 A very peculiar female collected by O. Kabakov in North-East Afghanistan (Nuristan Province, upper reaches of Waygal River, 2800 m, 8.07.1972) was initially attributed (Miroshnikov, 2019a) to a single known specimen (male) of *Paktoxotus pallidus* Holzschuh, 1974 described from Pakistan”, and on page 168 indicates: “*Paktoxotus pallidus* Holzschuh, 1974a: 86 A: ?AF PA #454”.

In fact, in Miroshnikov’s [2019a] paper quoted by Lazarev, not only the Afghan female is listed and illustrated, but also the male of *P. pallidus* from the same locality identified by the author and compared with the holotype. Here is the material that is presented in this work [Miroshnikov, 2019a: 2]: “*Paktoxotus pallidus* Holzschuh, 1974 Figs 1–8... MATERIAL EXAMINED. Afghanistan: 1♂ (ZIN) (Fig. 1), “Afghan. Nurestan. Upp. Waygal riv., 2800 m, 8.7.1972, Kabakov” [NE Afghanistan, Nuristan Province, upper reaches of Waygal River, 2800 m, 8.07.1972, leg. O.N. Kabakov] / *Paktoxotus pallidus* Holzschuh, 1974 ♂ det. A. Miroshnikov; 1♀ (cAM) (Fig. 3), same label, but taken on 6.07.1972 / *Paktoxotus pallidus* Holzschuh, 1974 ♀ det. A. Miroshnikov 2017 (preliminary identification!); Pakistan: holotype ♂ (cCH) (photograph; Fig. 2)”.

It is extremely hard to miss the note on the Afghan male on the pages of the publication under discussion. It will remain a deep secret how Lazarev managed to omit this record.

Thus, the distribution area of *Paktoxotus pallidus* includes Afghanistan without a question mark: A: AF PA.

However, Lazarev not only did not notice the above information. He omitted a species recently described from Afghanistan [Miroshnikov, 2018b].

Therefore, the following species must be included in the catalogue: *Diorthus kabakovi* Miroshnikov, 2018 A: AF.

– **Page 64.** M.A. Lazarev: “#232 According to Danilevsky (2011e), *Protapatophysis vartianae* Heyrovský, 1971 absent in Afghanistan (known from north Pakistan only)”;

– **Page 82.** M.A. Lazarev: “#375 Kariyanna et al. (2017): ... *Protapatophysis vartianae* (Heyrovský, 1971) was recorded for Kashmir and (?) Himachal Pradesh”;

– **Page 190.** “genus *Protapatophysis... vartianae* Heyrovský, 1971: 81 A: ?HP KA PA #232 #375”.

Remarks. Lazarev has confused these data. In fact, the information by Kariyanna et al. [2017] is borrowed from Danilevsky’s cited work and does not contain any new records. In Danilevsky’s [2011b] publication, *P. vartianae* was recorded not only from northern Pakistan (including the disputed territories), thereby based on the male, stored at the BMNH collection (bearing the label “Chamba”), with the following interpretation: “Chamba [?North India, Himachal Pradesh – 32°33’N, 76°07’E]”. Given the characteristics of the distribution of this species in general and the place of storage of the noted specimen, the locality in question, almost without a doubt corresponds to reality.

– **Page 70.** M.A. Lazarev: “#277 *Margites decipiens* Holzschuh, 1989c was moved to *Plavichydissus* Pic, 1946b

and *Lamellocerambyx* Pic, 1923e was accepted as a valid genus name by Miroshnikov (2018c);

– **Page 218.** M.A. Lazarev: "genus *Margites*... subgenus *Margites*..."

decipiens Holzschuh, 1989c: 393 A: BT";

– **Page 220.** M.A. Lazarev: "genus *Plavichydissus* Pic, 1946b: 107... #277 *decipiens* Holzschuh, 1989c: 393 (*Margites*) A: BT".

Remarks. Lazarev got confused and included the same species in different genera. In fact, this species was rightly transferred from the genus *Margites* to *Plavichydissus* [Miroshnikov, 2018b] within the revision of the latter genus. Therefore, the record on page 220 should be considered correct, and on page 218 – erroneous. In Lazarev's [2019b: 146] publication, the species in question is also mistakenly attributed to the genus *Margites*.

– **Page 94.** "#458 According to Lazarev (2019e), *Bulbocerambyx* Lazarev, 2019e includes at least 4 species: *B. grandis* (Gahan 1891), *B. gigas* (Thomson, 1878), *B. katarinae* (Holzschuh, 2009) and *B. vitalisi* (Pic, 1923).

Neocerambyx J. Thomson, 1861 = *Massicus* Pascoe, 1867.

So, *Neocerambyx* includes 9 Palaearctic species; 5 of them were transferred here from former *Massicus*: *N. atratulus* (Holzschuh, 2018a), *N. pascoei* (J. Thomson, 1857b), *N. taiwanus* (Makihara & Niisato, 2014), *N. trilineatus* (Pic, 1933a) and *N. venustus* (Pascoe, 1859).

Taxonomic positions of *Falsomassicus theresae* Pic, 1946 and *Massicus dierli* Heyrovský, 1976 were not identified; here both species are preliminary regarded as *Neocerambyx*";

– **Page 99.** "#497 According to Miroshnikov (2020), *Neocerambyx* J. Thomson, 1861 = *Bulbocerambyx* Lazarev, 2019e and *Massicus* Pascoe, 1867 is a valid name. New synonyms were supposed: *Neocerambyx vitalisi* Pic, 1923e = *N. elenae* Lazarev, 2019e.

According to Lazarev (2020c), *Bulbocerambyx* Lazarev, 2019e must be resurrected as a valid name as well as *Massicus* Pascoe, 1867. The supposition by Miroshnikov (2020) on the synonymisation of *N. elenae* Lazarev, 2019e and *N. vitalisi* Pic, 1923e must be canceled".

Remarks. The comments #458 and #497, when viewed in a single context and taking into account the data on the specified taxa on pages 216, 218 and 219, not only greatly confuse the reader, but they also clearly show the manipulation of published data.

Firstly, it is utterly puzzling for what purpose the wrong synonymy (*Neocerambyx* = *Massicus*) and wrong combinations (*Neocerambyx pascoe*, *N. taiwanus*, *N. trilineatus*, and *N. venustus*) established by Lazarev are mentioned in comment #458, despite the fact that on pages 218–219 the genus *Massicus* is given as a valid name, and the listed species are attributed to this genus.

Secondly, the statement "Taxonomic positions of *Falsomassicus theresae* Pic, 1946 and *Massicus dierli* Heyrovský, 1976 were not identified" does not correspond to reality. In fact, both species were just skipped by Lazarev [2019c], which Miroshnikov [2020a: 76] already mentioned.

Thirdly, the following wording seems to be very strange: "here [in the Catalogue] both species [*F. theresae* and *M. dierli*] are preliminary regarded as *Neocerambyx*".

In fact, these species were already transferred to *Neocerambyx* by Miroshnikov [2020a: 79] with the establishment of new combinations.

Fourth, the comments "According to Miroshnikov (2020), *Neocerambyx* J. Thomson, 1861 = *Bulbocerambyx* Lazarev, 2019e and *Massicus* Pascoe, 1867 is a valid name" and "According to Lazarev (2020c), *Bulbocerambyx* Lazarev, 2019e must be resurrected as a valid name as well as *Massicus* Pascoe, 1867" are very vague in regards to the genus *Massicus*. In fact, Miroshnikov [2020a] not only convincingly proved the fallacy of the synonymy established by Lazarev [2019c] but also formally restored the genus *Massicus* from the synonyms of *Neocerambyx*. Lazarev [2020: 123], after this publication, was forced only to admit the incorrectness of his synonymy and nothing more: "After all I am ready to accept preliminary the high taxonomy value of the structure of anterior coxae and accept the restoration of the validity of *Massicus*".

Fifth, Miroshnikov [2020a, b] convincingly demonstrated the artificiality of the genus *Bulbocerambyx* Lazarev, 2019. This conclusion has been supported by a number of other researchers [Li et al., 2020; Holzschuh, 2020; Tavakilian, Chevillotte, 2021; and others]. Thus, the above synonymy *Neocerambyx* J. Thomson, 1861 = *Bulbocerambyx* Lazarev, 2019 should be accepted.

Sixth, Lazarev [2019c] described *Neocerambyx elenae*, mistakenly comparing it only with a morphologically wholly different species, *N. atratulus*, claiming that his new species "is very close to *N. atratulus*". Until now, he has not proved the reality of the species described by him and has not shown its difference from truly similar taxa. In response to Miroshnikov's [2020a] remarks, Lazarev could only note that "*N. elenae* is a very good species not close to any other" and nothing more. These are amazing arguments! Therefore, I still consider the following presumptive synonymy to be relevant: *Neocerambyx vitalisi* Pic, 1923 = ?*N. elenae* Lazarev, 2019.

– **Page 120.** M.L. Danilevsky: "genus *Anastrangalia*... *dubia dubia*..."

planeti Pic, 1945b: 5".

Remarks. Must be: *planeti* Pic, 1945b: 5 (*Leptura*) [Miroshnikov, 2011: 9, 32].

– **Page 122.** M.L. Danilevsky: "genus *Anoplodera*... *rufipes astrabadensis* Pic, 1900n: 82 A: AB IN".

Remarks. Must be: A: AB (Talysh) IN.

– **Page 122.** M.L. Danilevsky: "genus *Anoplodera*... *sexguttata* Fabricius, 1775: 198 (*Leptura*) E: ... ST... A: GG TR".

Remarks. The fictional data on the distribution of this species in Georgia (GG) mentioned by Danilevsky in the updated catalogue are the legacy of the first edition [Catalogue..., 2010: 98]. However, the circumstances under which this author originally included Georgia in the distribution area of *A. sexguttata* require additional discussion here, as was done by Miroshnikov [2016: 197–198] earlier in more detail. These circumstances are one of the most striking examples of Danilevsky's falsification of published data.

Danilevsky explains the information in question in his monograph [2015a: 254] as follows: "...многочисленные

упоминания *Anoploclera sexguttata* для Северного Кавказа, как и указания для Закавказья, и конкретно для Грузии (Danilevsky & Smetana, 2010), вероятно, основаны на указании Плавильщикова (1936) для Черноморского побережья Кавказа” (the numerous mentions of *Anoploclera sexguttata* for the North Caucasus, as well as the indications for the Transcaucasia, and specifically for Georgia (Danilevsky & Smetana, 2010), are probably based on Plavilstshikov’s (1936) indication for the Black Sea coast of the Caucasus). However, in Plavilstshikov’s [1936] work, to which Danilevsky is referring, there is no mention of the “Black Sea coast of the Caucasus” regarding the distribution area of *A. sexguttata* either in the Russian part text (page 330) or in the German part (page 553). In fact, this monograph by Plavilstshikov contains the following absolutely clear indication: “Distribution: in the European part of the USSR, it is distributed from the Black Sea to Leningrad and Perm; the eastern border is, as it seems, the Ural Mountain Range” [Plavilstshikov, 1936: 330]. As is known, apart from the Caucasus, another territory of the former USSR (for example, Odessa, Nikolaev, Kherson regions, now parts of the Ukraine) is situated on the Black Sea, and this species is recorded exactly from this territory. It is thereby important to note that none of Plavilstshikov’s other works, neither previous nor in subsequent ones [Plavilstshikov, 1932, 1948, 1955, 1965], mentions the data from “Black Sea coast of the Caucasus” for *A. sexguttata*. But would Plavilstshikov [1936] really had such a report, then in this case also it would be difficult to understand Danilevsky’s “interpretation” given the fact that the length of the Russian “Black Sea coast of the Caucasus” is even greater than it is in Georgia (even including Abkhazia). At the same time, it is completely unclear to me exactly what “indications for the Transcaucasia” Danilevsky had in mind, except for his own “specifically for Georgia”. It is highly unlikely that he, knowing at least a single record from the Transcaucasia, would refuse this hypothetical corresponding reference under the given circumstances. But I felt it necessary to check all of the publications on *A. sexguttata* cited by Danilevsky [2015a: 254]. None of these works contains any records of this species from the Transcaucasia!

However, I did not confine myself only to the bibliography indicated in Danilevsky’s [2015a] book, and for greater confidence in this matter, I studied a number of other important works, mainly of catalogues [Heyden et al., 1883, 1891, 1906; Pic, 1900; Boppe, 1921; Plavilstshikov, 1955, and some others]. But in these publications there is also not a single record of the species in question from the area of Transcaucasia. Therefore, all of Danilevsky’s reasoning on this score are devoid of any sense, and his “report for Georgia” and “indications for the Transcaucasia” regarding *A. sexguttata* are pure invention. It is thereby quite obvious that this author, attempting to justify his fictional data published previously, deliberately gives references to information that does not exist in the literature.

The distribution of this species in the Caucasus has not yet been studied in detail [Miroshnikov, 2011b]. From there, only the single, probably old record from the surroundings of Dolzhanskaya Village in the north of Krasnodar Region

[Arzanov et al., 1993] is known and two other, about a century old, specimens from Teberda, which I discovered in the ZMMU collection [Miroshnikov, 2011b]. Danilevsky deliberately missed the reference to the latter paper, which contains the paramount data shedding light on the distribution of *A. sexguttata* in the Caucasus.

Taking into account all of the above information, Georgia (GG) should be excluded from the distribution area of *Anoploclera (Anoploclera) sexguttata*. At the same time, the probability of the distribution of this species in the Transcaucasia cannot be ruled out completely, as noted before [Miroshnikov, 2016].

– **Pages 126–127.** M.L. Danilevsky: “genus *Grammoptera...* *ustulata ustulata* Schaller, 1783: 298 (*Leptura*)...”

Remarks. As in the first edition [Catalogue..., 2010: 101], the following synonym for this taxon is missing: *Grammoptera ustulata* var. *semirufescens* Pic, 1947: 4 (“Guerreaux”) [Miroshnikov, 2013: 13].

– **Page 134.** M.L. Danilevsky: “genus *Lepturobosca* Reitter, 1913a: 17...

virens Linnaeus, 1758: 397 (*Leptura*) E... ST...”

Remarks. The southernmost locality of this species in the European part of Russia is in the north of Rostov Region and was first reported by Kasatkin [2005: 56, Veshenskaya Village]. However, Danilevsky did not provide a reference to these important data either in the first edition of the catalogue or in the second one. In addition, he completely ignored Kasatkin’s information in his monograph [Danilevsky, 2015a]. Earlier, Miroshnikov [2016] drew attention to this fact.

– **Page 137.** M.L. Danilevsky: “genus *Pachytodes...* *cerambyciformis* Schrank, 1781a: 154 (*Leptura*) E: ...” (the Asian regions, including the countries of the Transcaucasia, are completely absent in the distribution area of *P. cerambyciformis*. – A.M.).

Remarks. In the first edition [Catalogue..., 2010: 108], the distribution area of this species in the Caucasus was presented as follows: “E: AB... AR... GG... ST...” Miroshnikov [2011a: 18, 35] already noted that the records of *P. cerambyciformis* from Azerbaijan and Armenia require reliable confirmation, and in Georgia it is known from the only old record from Abastumani.

Danilevsky deliberately did not provide a reference to this work, and presented some of the published results of Miroshnikov’s research as his own. At the same time, this author also removed Georgia from the distribution area of species in question.

Pachytodes cerambyciformis was recorded from Georgia for the first time by Plavilstshikov [1925: 294, 301, “Transcaucasia: Abas-Tuman, V (Zhicharev! Coll. mea)”. For a long time, I repeatedly tried to find the corresponding material in Plavilstshikov’s collection stored at the ZMMU, but to no avail [Miroshnikov, 2009a: 790]. Only in 2010, I finally was able to locate one female with a label “Transcauc., Abas-Tuman, V, Zhicharev leg.” in this collection [Miroshnikov, 2011b: 557–558, insert, fig. 2] that fully corresponds to Plavilstshikov’s [1925] publication. In addition, in the ZIN collection, I found another Caucasian specimen (male) of this species with the label “Caucasus,

Teberda?". Consequently, I discussed in detail both these records and, in general, the distribution of the species in question [Miroshnikov, 2016: 197–198].

Danilevsky's complete disregard for all the above publications containing the evidence of the most important factual material, including illustrated one, does not surprise me for obvious reasons. Moreover, against the background of his mythical data on the distribution of *Anoplodera sexguttata* in the same Georgia, it even looks just absurdly.

Considering the above, Georgia (GG) should be included in the distribution area of *Pachytodes cerambyciformis*, at least with a question mark.

– **Page 137.** M.L. Danilevsky: "genus *Pachytodes*... *cerambyciformis* Schrank, 1781a: 154..."

bisquadristigmatus Pic, 1915a: 29 (*Leptura*)".

Remarks. Must be: *bisquadristigmus* [Miroshnikov, 2013: 13].

In addition, the name *sexmaculatus* Panzer, 1795: 272 (*Leptura*) (HN) given in the first edition [Catalogue..., 2010: 108], but not marked in it as a homonym [Miroshnikov, 2011a: 9, 35], has been omitted in the updated catalogue.

– **Page 140.** M.L. Danilevsky: "genus *Pedostrangalia*... subgenus *Neosphenalia*... *kurda* Sama, 1996c: 104 A: AR GG IN IQ TR #181";

– **Page 57.** M.L. Danilevsky: "#181 *Pedostrangalia kurda* Sama, 1996c was recorded for Iran by Villiers (1967: 351, as "emnipoda")".

Remarks. In the first edition [Catalogue..., 2010: 110], the distribution area of this species was indicated as follows: "A: TR". Miroshnikov [2011a: 18, 35; 2011b: 558] discussed the distribution of *P. kurda* in Georgia and Armenia for the first time.

Danilevsky deliberately did not provide a reference to these works, and presented the results of Miroshnikov's research as his own.

In addition, it should be noted that, according to Sama et al. [2008], *P. kurda* is absent in Iran, but Danilevsky also did not provide a reference to this work.

Given the above, Iran (IN) should be included in the distribution area of *Pedostrangalia kurda* only under a question mark.

– **Page 140.** M.L. Danilevsky: "genus *Pedostrangalia*... subgenus *Neosphenalia*..."

verticenigra Pic, 1892v: 416 (*Leptura*) E: GR (Samos) A: GG TR".

Remarks. This species was recorded from Georgia without any comments both in the first edition [Catalogue..., 2010: 111], and in the second one. Miroshnikov [2011a: 35–36] discussed in detail the distribution of *P. verticenigra* and noted that there are still no specific records from Transcaucasia, including Georgia. There is only one old record from the "southwestern Transcaucasia" [Panin, Săvulescu, 1961: 201, as *Strangalia (Pedostrangalia) verticalis*]. But this report, almost without a doubt, is based on Plavilstshikov's [1936: 456, as *Strangalia (Pedostrangalia) verticalis*] assumption about the distribution of this species in the mentioned area, taking into account its record from Artvin [Nesterov, 1912: 0153, as *Leptura verticalis*]. In addition, *P. verticenigra* is recorded from Artvin in some contemporary publications [Tozlu et al., 2002].

It is very likely that the record from Georgia in the first edition [Catalogue..., 2010] was made on the basis of data by Panin and Săvulescu [1961] and/or Plavilstshikov [1936].

Danilevsky deliberately did not provide a reference to Miroshnikov's [2011a] monograph, in which the data of the first edition on the distribution of *P. verticenigra* in Georgia and the Asian part of Turkey, repeated in the updated catalogue, are clearly commented.

– **Pages 140–141.** M.L. Danilevsky: "genus *Pedostrangalia*... subgenus *Pedostrangalia*... *tokatensis* Sama, 1996c: 103 A: GG TR".

Remarks. In the first edition [Catalogue..., 2010: 110], the distribution area of this species was given as follows: "A: TR". Miroshnikov [2011a: 18, 35; 2011b: 557–558] discussed the distribution of *P. tokatensis* in Georgia for the first time.

Danilevsky deliberately did not provide a reference to these works, and presented the results of Miroshnikov's research as his own.

– **Pages 146–147.** M.L. Danilevsky: "genus *Stictoleptura* Casey, 1924: 280..."

subgenus *Aredolpona* Nakane & K. Ohbayashi, 1957: 50... *rubra rubra* Linnaeus, 1758: 397 (*Leptura*) E: ... ST..."

Remarks. The southernmost locality of this taxon in the European part of Russia is in Rostov Region and was first reported by Kasatkin [1999: 37: Myasnikovskiy District, Nedvigovka Village]. However, Danilevsky did not provide a reference to these important data either in the first edition of the catalogue or in the second one. In addition, he completely ignored Kasatkin's information in his monograph [Danilevsky, 2015a], thereby very strangely claiming that *S. rubra* is not recorded from Rostov Region. Earlier, Miroshnikov [2016] drew attention to this paradoxical allegation.

– **Pages 146, 148.** M.L. Danilevsky: "genus *Stictoleptura* Casey, 1924: 280..."

subgenus *Paracorymbia* Miroshnikov, 1998: 587 type species: *Leptura fulva* DeGeer, 1775".

Remarks. Danilevsky [2015a] has failed to substantiate convincingly the supraspecific classification of some groups of the tribe Lepturini, in particular *Stictoleptura* – *Paracorymbia*, but he repeated it in the updated catalogue without any comments. However, Miroshnikov [2016: 184–191, colour pls 7–8, figs 1–14] discussed in great detail the relevant statements and conclusions of this author and demonstrated perfectly clear that they are mainly based on fabrications, scientifically unproven arguments, deep contradictions, and rather dubious characteristics. Very mysterious and inexplicable circumstances thereby are noted under which Danilevsky abandoned *Paracorymbia* as a separate genus.

In fact, the generic status of *Paracorymbia* is accepted without any doubt by wide circle of researchers [Rejzek, Rébl, 1999; Vives, 2000, 2001; Filimonov, Udalov, 2002; Sama, 2002, 2005; Tezcan, Rejzek, 2002; Tozlu et al., 2002; Devesa, Bahillo, 2003; Doychev, Georgiev, 2004; Telnov, 2004; Pesarini, Sabbadini, 2004, 2007; Bartenev, 2004, 2009; Serafim, 2004, 2006, 2008; Vitali, 2004, 2005, 2009, 2014a, 2014b, 2018; Dedyukhin, 2005; Kasatkin, 2005; Konvička, 2005; Brelih et al., 2006; Özdikmen,

Okutaner, 2006; Rapuzzi, Sama, 2006; González Peña et al., 2007; Kovács, 2007; Rapuzzi, Georgiev, 2007; Rozner, 2007; Özdikmen, 2007, 2008a, b; Hovorka, 2008; Peris-Felipo et al., 2008; Sama et al., 2008, 2010a, b, 2012; Malmusi, Saltini, 2009; Doychev et al., 2009; Terekhova, Bartenev, 2009; Adlbauer, 2010; Gurău, 2010; Hellrigl, 2010; Sláma, 2010; Bartenev, Terekhova, 2011; Gnjatović, Žikić, 2011; Tamutis et al., 2011; Sama, Rapuzzi, 2011; Sama et al., 2011; Týr, 2011; Wetton, 2011; Wright, 2011; Zamoroka, Panin, 2011; Alziar, Lemaire, 2012; Ceccolini et al., 2012; Gobbi et al., 2012; Nash, 2012; Peris-Felipo, Jiménez-Peydró, 2012; Rejzek, 2012; Abdurakhmanov, 2012, 2013; Labatut et al., 2013; Skrylnik, 2013; Papi, Ceccolini, 2014; Topalov et al., 2014; Zhukov, 2015; Pesarini C., Pesarini E., 2016; Alexander, 2019; Brock, 2021; Georgiev et al., 2021; Toriti et al., 2021; and many others].

The establishing of the subgenus *Maculileptura* Danilevsky, 2015 based on the *maculicornis*-group that I have proposed earlier [Miroshnikov, 1998a, b] cannot be considered expedient, especially as part of the genus *Stictoleptura*. In addition, Danilevsky erroneously included *Paracorymbia picticornis* (*excisipes*-group) in the named subgenus.

Thus, *Paracorymbia* (*Paracorymbia*) Miroshnikov, 1998 = *Maculileptura* Danilevsky, 2015 (2014 sensu Danilevsky, see above), **syn. n.**

Given the above, as well as some of the comments presented below, in the updated catalogue, *Paracorymbia* should be considered as an independent genus with the following taxa:

genus *Paracorymbia* Miroshnikov, 1998: 587...

subgenus *Batesiata* Miroshnikov, 1998: 594...
(= *Pyrrholeptura* Lazarev, 2016, see below)...

pyrrha Bates, 1884: 216 (*Leptura*)...

tesserula orientalis Vartanis, 2019: 14...

tesserula tesserula Charpentier, 1825: 227 (*Leptura*)...

subgenus *Paracorymbia* Miroshnikov, 1998: 587...
(= *Maculileptura* Danilevsky, 2015)

[the *fulva*-group]

fulva DeGeer, 1775: 137 (*Leptura*)...

hybrida Rey, 1885b: 277 (*Leptura*)...

nadezhdae Plavilstshikov, 1932: 182 (*Leptura*)...

pallidipennis Tournier, 1872: 346 (*Leptura*)...

sambucicola Holzschuh, 1982a: 65 (*Brachyleptura*)...

tonsa K. Daniel et J. Daniel, 1891: 31 (*Leptura*)...

[the *excisipes*-group]

benjamini benjamini Sama, 1993: 470 (*Corymbia*)...

benjamini ehdenensis Sama et Rapuzzi, 2000: 10...

excisipes K. Daniel et J. Daniel, 1891: 6 (*Leptura*)...

picticornis Reitter, 1885: 390 (*Leptura*)

[the *maculicornis*-group]

maculicornis DeGeer, 1775: 139 (*Leptura*)...

ondreji Sláma, 1993: 59 (*Brachyleptura*)...

pallens Brulle, 1832: 264 (*Leptura*)...

simponica Fairmaire, 1885: 317 (*Leptura*)...

– **Pages 146, 148.** “genus *Stictoleptura* Casey, 1924: 280...

subgenus *Pyrrholeptura* Lazarev, 2016d: 15 type species:
Leptura pyrrha Bates, 1884

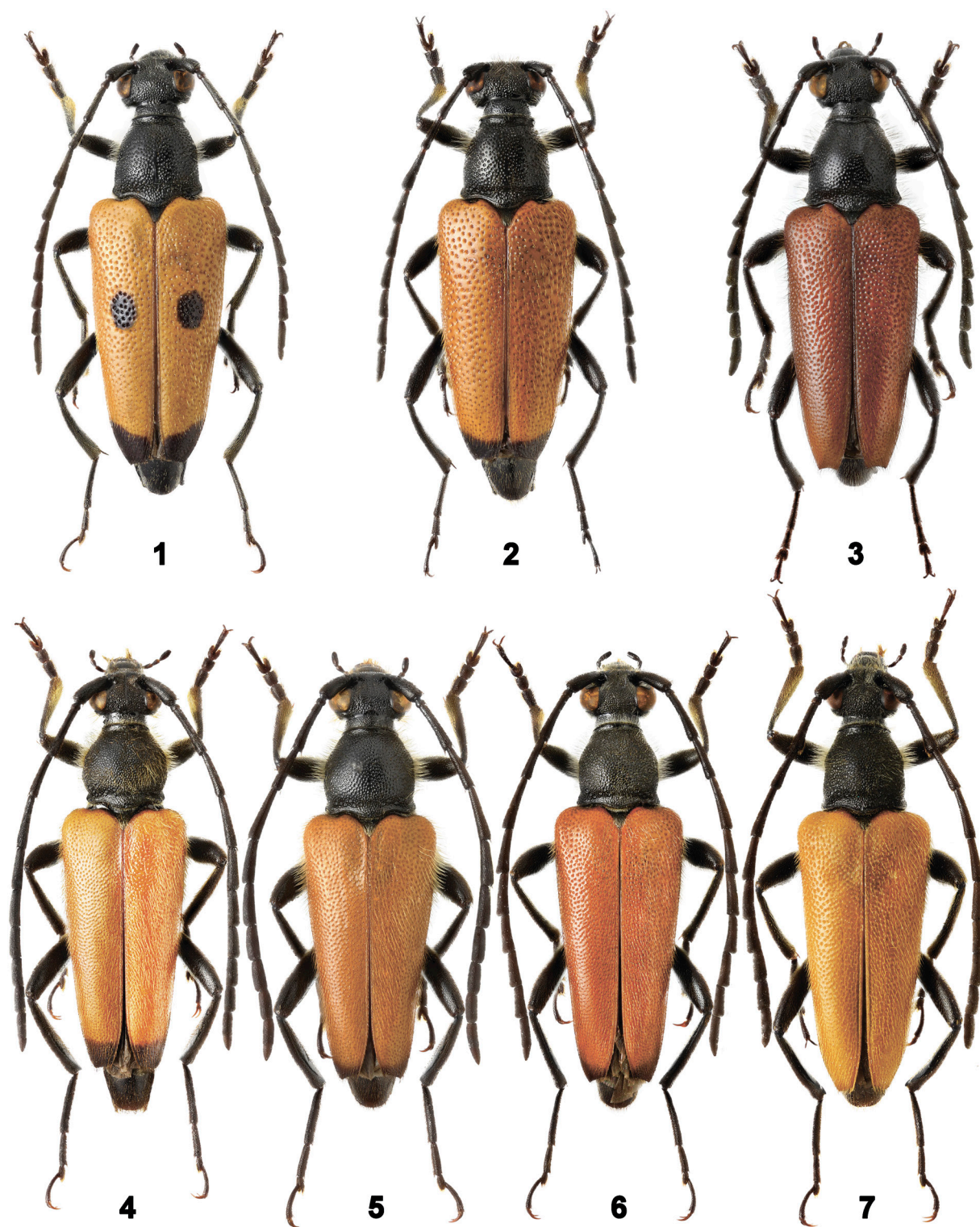
pyrrha Bates, 1884a: 216 (*Leptura*) A: JA”

Remarks. *Leptura pyrrha* Bates, 1884 and *Leptura tesserula* Charpentier, 1825 belong to the subgenus *Batesiata* Miroshnikov, 1998 of *Paracorymbia* Miroshnikov, 1998 [Miroshnikov, 1998a, b].

Danilevsky [2015a: 276], trying to demonstrate the allegedly strong morphological differences between these species, states the following: “Эти виды не имеют между собой ничего общего. *Stictoleptura pyrrha* (Bates, 1884) имеет небольшую вырезку последнего стернита брюшка самца, очень своеобразную форму парамер, расширенных у вершины и очень густую пунктировку красных (!) надкрылий” (these species have nothing in common. *Stictoleptura pyrrha* (Bates, 1884) has a shallow emargination of the last sternite of the male abdomen, a very peculiar shape of parameres widened at the apex, and a very dense punctation of red (!) elytra).

Miroshnikov [2016: 186–188, colour pls 7–8, figs 1–3, 8–10] convincingly proved the profound fallacy of these Danilevsky’s allegations and demonstrated in detail, on the contrary, a very evident morphological similarity of the species under consideration in a variety of important features.

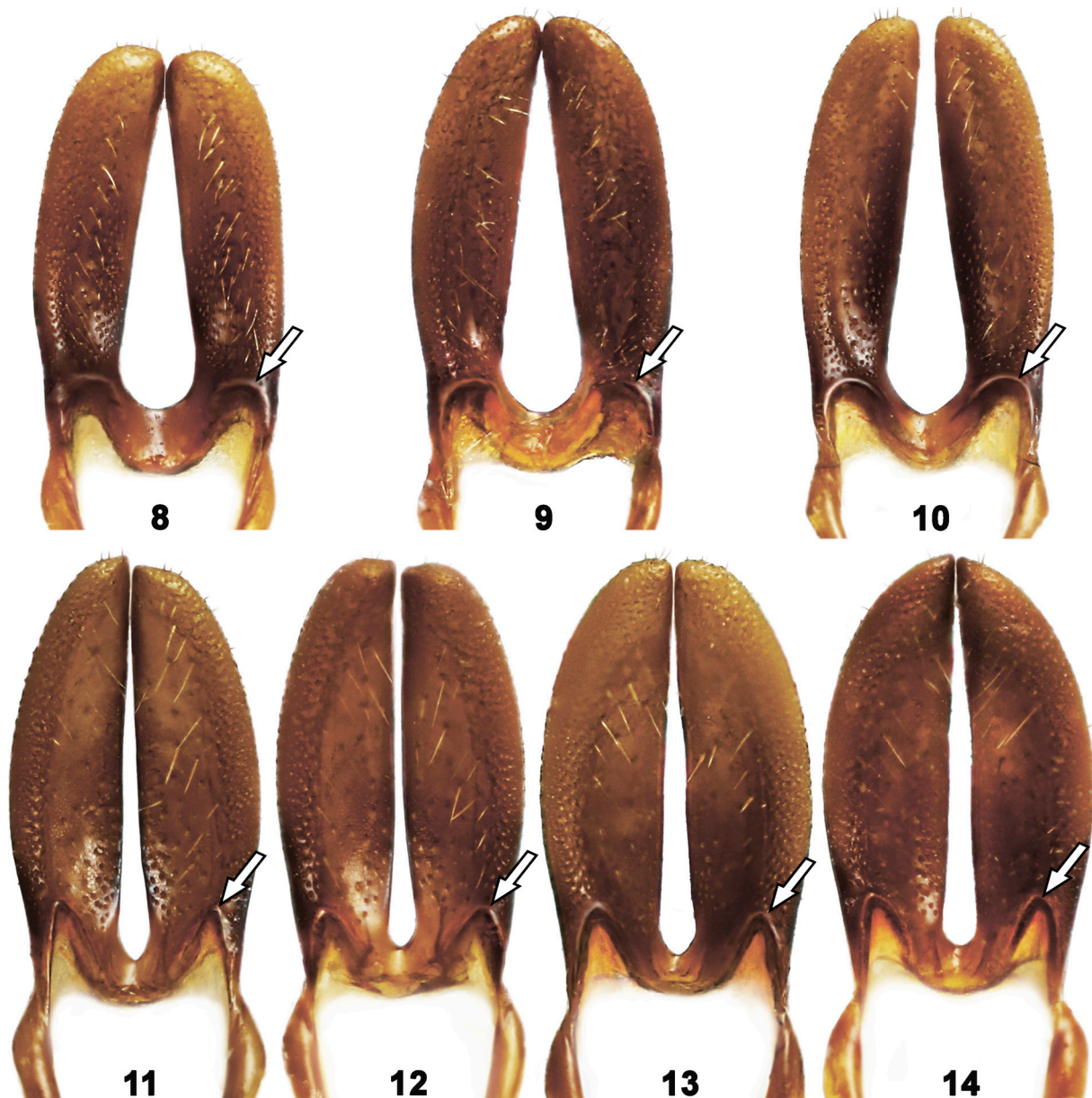
This similarity is observed, in particular, in the structure of the male last (visible) abdominal sternite (including the shape of the apical emargination, as in Figs 15–18), the male genitalia (including the shape of the parameres, as in Figs 8–10, which in fact may even be barely distinguishable in the discussed species, as in Figs 9, 10, as well as the shape of the protrusion at the base of each paramere, as indicated by arrows in Figs 8–10), the head (especially posteriorly, including the temples), the antennae (including the characteristic shape of the truncated apical external angle of antennomeres 5–9 or 6–9, as in Figs 1–3), the pronotum, the elytral apices, the metatibiae, and some other details. A noticeable similarity in habitus was also acknowledged (Figs 1–3). It is indicated that the elytra of *Paracorymbia* (*Batesiata*) *pyrrha*, although with a denser punctation than those in *P. (B.) tesserula*, are nevertheless coarse and noticeably similar in size to *P. (B.) tesserula* (Figs 23, 24). Convincing examples of different elytral colouration (similar to the two species under consideration) are shown in some closely related species in similar genera *Stictoleptura*, *Brachyleptura*, and some others. In addition, attention was drawn to the original description of the Japanese species under consideration [Bates, 1884]. Its author, a well-known English naturalist and explorer, a great connoisseur of longicorn beetles, Henry Walter Bates, also emphasized the colouration of the elytra in the original description, but only as a species trait. But first of all, he very clearly pointed out the close relation between *Leptura pyrrha* and *L. tesserula*: “LEPTURA PYRRHA, n. sp. *L. tesserula* proxime affinis, differt solum elytris rubris immaculatis...” [Bates, 1884: 216]. It is extremely doubtful that Bates accidentally compared his new Japanese species only with a single West-European species distributed also in Asia Minor and the Caucasus, and particularly selected it from a large number of other, externally more or less strongly similar taxa (including those with red elytra) already known to science at that time from various areas of Holarctic. In fact, he was the first to unmistakably predetermined a natural group for these two species.



Figs 1–7. Species of *Paracorymbia*, males, habitus, dorsal view.

Рис. 1–7. Виды рода *Paracorymbia*, самцы, общий вид сверху.

1–2 – *P. (Batesiata) tessera*; 3 – *P. (B.) pyrria*; 4 – *P. (Paracorymbia) sambucicola*; 5 – *P. (P.) fulva*; 6 – *P. (P.) tonsa*; 7 – *P. (P.) pallidipennis*.



Figs 8–14. Species of *Paracorymbia*, apical part of tegmen, ventral view.

Рис. 8–14. Виды рода *Paracorymbia*, верхинная часть тегмена снизу.

8–9 – *P. (Batesiata) pyrrrha*; 10 – *P. (B.) tessarula*; 11 – *P. (Paracorymbia) sambucicola*; 12 – *P. (P.) fulva*; 13 – *P. (P.) tonsa*; 14 – *P. (P.) pallidipennis*.

To confirm the above similarity between *Paracorymbia (B.) tessarula* and *P. (B.) pyrrrha*, the same similarity was demonstrated between very closely related species of the nominative subgenus [Miroshnikov, 2016], in particular, *Paracorymbia (P.) sambucicola*, *P. (P.) fulva*, *P. (P.) tonsa*, and *P. (P.) pallidipennis* (Figs 4–7, 11–14, 19–22).

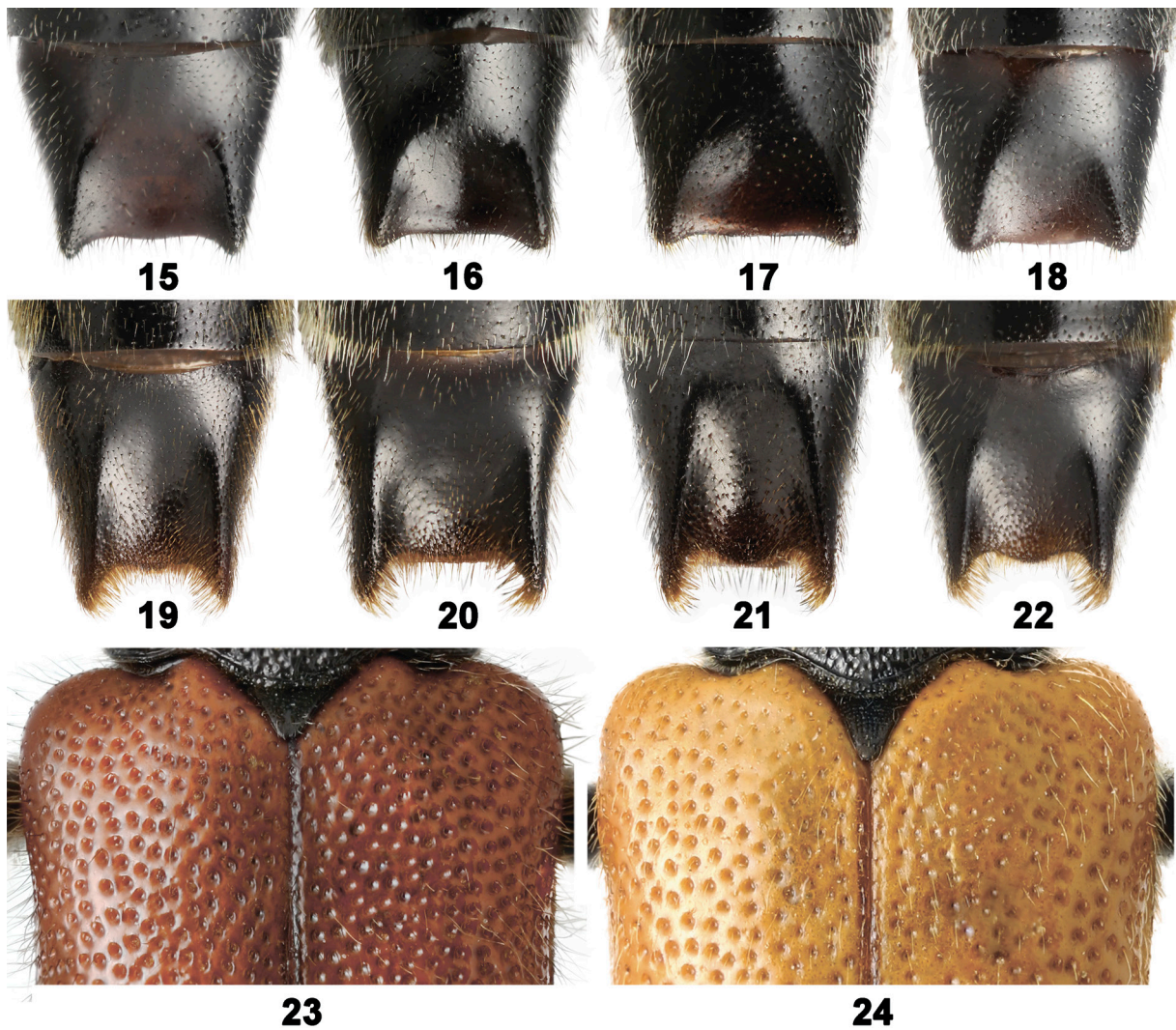
Lazarev [2016: 14–15], based on Danilevsky's deeply erroneous opinions on the "morphological isolation" of *Leptura pyrrrha* [Danilevsky, 2015a], established a separate subgenus *Pyrrholeptura* Lazarev, 2016 of *Stictoleptura* for this taxon. He fully repeated the "differences" listed by Danilevsky for *Paracorymbia (Batesiata) tessarula* and *P. (B.) pyrrrha*, which do not correspond to reality.

Therefore, in the updated catalogue, the previously established synonymy should be considered: *Batesiata* Miroshnikov, 1998 (subgen. pro *Paracorymbia* Miroshnikov, 1998) = *Pyrrholeptura* Lazarev, 2016 (subgen. pro *Stictoleptura* Casey, 1924) [Miroshnikov, 2016].

– Page 147. M.L. Danilevsky: "subgenus *Batesiata* Miroshnikov, 1998: 594...

tessarula tessarula Charpentier, 1825: 227 (*Leptura*)...
bisignata Faldermann, 1837: 313 (*Leptura*) [HN]
dejeani Ganglbauer, 1889c: 469 (*Leptura*) [RN]"

Remarks. Miroshnikov [2013: 14] pointed out that the last two names (the former as a junior homonym and the latter as a replacement name) were omitted in the first edition [Catalogue..., 2010].



Figs 15–24. Species of *Paracorymbia*, males, details of structure.
 15–16, 24 – *P. (Batesiata) tesseraula*; 17–18, 23 – *P. (B.) pyrriha*; 19 – *P. (Paracorymbia) sambucicola*; 20 – *P. (P.) fulva*; 21 – *P. (P.) tonsa*; 22 – *P. (P.) pallidipennis*; 15–22 – last (visible) sternite; 23–24 – base of elytra.
 Рис. 15–24. Виды рода *Paracorymbia*, самцы, детали строения.
 15–16, 24 – *P. (Batesiata) tesseraula*; 17–18, 23 – *P. (B.) pyrriha*; 19 – *P. (Paracorymbia) sambucicola*; 20 – *P. (P.) fulva*; 21 – *P. (P.) tonsa*; 22 – *P. (P.) pallidipennis*; 15–22 – последний (видимый) стернит; 23–24 – основание надкрылий.

Danilevsky deliberately did not provide a reference to this paper, and presented the results of Miroshnikov's research as his own.

– Page 148. M.L. Danilevsky: "...*Melanoleptura* Miroshnikov, 1998: 594...
scutellata miroshnikovi Danilevsky, 2012m: 915 A: AB IN
scutellata ochracea Faust, 1878: 135 (*Leptura*) E: ST A: AB
 AR GG TR #78
scutellata scutellata Fabricius, 1781: 247 (*Leptura*) E: AL
 AU BE BH BU BY CR CT CZ DE FR GB GE GR HU IR IT
 LA LU MC MD ME PL PT RO SB SK SL SP ST SV SZ TR
 UK";

– Page 43. M.L. Danilevsky: "#78 According to Sláma (2015a): *Stictileptura scutellata ochracea* (Faust, 1878) is a valid name for a subspecies from Caucasus and Transcaucasia (populations from the neighbouring areas of Turkey could be included)".

Remarks. Miroshnikov [1998a, b] was the first who pointed on the morphological peculiarity of the Hyrcanian form of *Melanoleptura scutellata* (based on material from the Talysh Mountains, Azerbaijan) and its distinguish features from the nominative form, including inhabiting the rest of the Caucasus and Transcaucasia.

In the first edition [Catalogue..., 2010: 55, "New act and comments"], Sama indicated the following: "*Stictoleptura scutellata ochracea* Faust, 1879 (*sic*) is raised from variety of *Stictoleptura scutellata* Fabricius, 1781 to subspecies. I have examined a long series of specimens from northern Iran (chiefly Gilan and Mazandaran prov.) and Azerbaijan. All specimens constantly differ from those of *S. scutellata* s. str. by..."

Miroshnikov [2011a: 29–30] subsequently noted that *Leptura scutellata* var. *ochracea* Faust, 1878: 135 (*sic*) was described from the vicinities of Baku and with no doubt is

identical with the nominative subspecies. A new synonymy thereby was established: “*Stictoleptura scutellata scutellata* (Fabricius, 1781) = “*S. scutellata ochracea* (Faust, 1878).

Danilevsky [2012d: 915] indicated the following: “The type locality of *Leptura scutellata* var. *ochracea* Faust, 1878 is “Baku” – according to the original description, so it is very far from Talysh – the northern most area, where the Iranian subspecies described in details (but not named!) by Miroshnikov (1998: 595–596) is also distributed. I do not know *S. scutellata scutellata* from Baku environs, but the nominative subspecies is very numerous in North Azerbaijan (specimens from Ismailly and Zeyva are available), and represented here by usual Caucasian form without erect setae on lateral sides of prothorax – the unique character of Iranian subspecies. In general the fauna of Baku region is much closer to North Azerbaijan, than to Talysh. So, *S. s. scutellata* (Fabricius, 1781) = *Leptura scutellata* var. *ochracea* Faust, 1878, and the subspecies from Talysh and North Iran is described here as new: *Stictoleptura scutellata miroshnikovii*, ssp. n. ...” Danilevsky thereby ignored the reference to the aforementioned monograph by Miroshnikov and the data given in it concerning *Leptura scutellata* var. *ochracea* Faust, 1878, interpreting them as his own.

In subsequent work, Danilevsky [2015a] once again recognized the absence of distinguishing features between the nominative form and various Caucasian populations (excluding the Talysh Mountains), including those from the northern part of Azerbaijan. He once again thereby emphasized that “*S. s. scutellata* (Fabricius, 1781) = *Leptura scutellata* var. *ochracea* Faust, 1878” (again, without reference to Miroshnikov’s [2011a] monograph).

Later on, contrary to his recent views and beliefs [Danilevsky, 2012d, 2015a], Danilevsky (comments #78) noted that “According to Sláma (2015a): *Stictoleptura scutellata ochracea* (Faust, 1878) is a valid name for a subspecies from Caucasus and Transcaucasia...” and listed this taxon as valid.

But it is no less paradoxical that afore quoted Danilevsky’s comments do not correspond to reality. In fact, Sláma [2015: 40–41] highlighted this form with a question mark and noted the following: “I have seen several specimens of *S. scutellata* from Caucasus that were slightly different than the nominal form (see below), but completely different than the imagoes from Transcaucasia. Although differences from the nominal form are not clearly significant enough, possibly it could be named ssp. *ochracea* Faust, 1879). Spread: Caucasus (Abchazia)... Here is a short determination key of *Stictoleptura scutellata* geographical subspecies based on antennal segments. In the key I am not listing the possible ssp. *ochracea* from Caucasus, which is just slightly different”. Thus, this author made only some assumptions about the isolation of this form and did not even include it in the key to subspecies of *S. scutellata*.

Based on the above, the data for taxa under discussion should be reported as follows:

scutellata miroshnikovii Danilevsky, 2012m: 915 A: AB (Talysh) IN

scutellata scutellata Fabricius, 1781: 247 (*Leptura*) E: AL AU BE BH BU BY CR CT CZ DE FR GB GE GR HU IR IT

LA LU MC MD ME PL PT RO SB SK SL SP ST SV SZ TR UK A: AB (except for Talysh) AR GG TR *ochracea* Faust, 1878: 135 (*Leptura*).

– Page 148. M.L. Danilevsky: “subgenus *Stictoleptura* Casey, 1924: 280...

cordigera cordigera Fuessly, 1775: 14 (*Leptura*) E: ... ST... UK...”

Remarks. In the first edition [Catalogue..., 2010: 114], the distribution area of this taxon did not include the south of the European part of Russia (ST). Miroshnikov [2011a: 19, 37; 2011b: 556] recorded *S. cordigera* from Derbent, Dagestan (ZIN), and also noted the doubtfulness of data on its distribution in Crimea.

Danilevsky deliberately did not provide a reference to these works, and presented the results of Miroshnikov’s research as his own.

At the same time, the discussed part of the distribution area of *Stictoleptura cordigera cordigera* must be recorded as follows: E: ...ST (Derbent, Dagestan)... ?UK (Crimea)...

– Page 150. M.L. Danilevsky: “genus *Strangalia* Dejean, 1835: 355...

attenuata Linnaeus, 1758: 398...

brunnescens Balbi, 1892: 49”.

Remarks. Must be: *brunnescens* Balbi, 1892: 49 (*Leptura*) [Miroshnikov, 2011a: 10, 38].

– Pages 153–154. M.L. Danilevsky: “genus *Oxymirus* Mulsant, 1862: 464...

cursor Linnaeus, 1758: 393...

vittatus Gmelin, 1790: 1865 (*Stenocorus*)”.

Remarks. Must be: *vittatus* Gmelin, 1790: 1865 (*Cerambyx*) [Miroshnikov, 2011a: 10, 39].

– Page 154. M.L. Danilevsky: “genus *Brachyta* Fairmaire, 1865: 185...

interrogationis ebenina Mulsant, 1839: 240...

flavonotata Mulsant, 1839: 239 (*Pachyta*)”.

Remarks. In the first edition [Catalogue..., 2010: 120], this name was given as follow: “*flavolineata* Mulsant, 1839: 240 (*Pachyta*)”. Miroshnikov [2011a: 10, 40] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not provide a reference to this work, and presented the results of Miroshnikov’s research as his own.

– Page 166. M.L. Danilevsky: “genus *Gnathacmaeops* Linsley & Chemsak, 1972: 135...

pratensis Laicharting, 1784: 172 (*Leptura*) E: ... ST... A: AB... AR... GG...”

Remarks. Both in the first [Catalogue..., 2010] and the second edition of the catalogue there are no comments on the distribution of this species in the Caucasus (ST, AB, AR, GG). At the same time, some researchers, in particular Sama [2002], considered Plavilstshikov’s [1936] records of *G. pratensis* from the Caucasus erroneous.

Miroshnikov [2011b: 555–556] discussed in detail the distribution of this species in the Caucasus, supplemented with reliable records and images of Caucasian specimens. In addition, it was noted [Miroshnikov, 2011a: 19, 39] that the record of *G. pratensis* from Azerbaijan requires reliable confirmation.

Danilevsky deliberately did not give a reference to the mentioned works, which contained the most important data on the distribution of this species in the North Caucasus, Georgia, and Armenia.

Given the above, Azerbaijan (AB) should be excluded from the distribution area of *Gnathacmaeops pratensis*.

– **Page 174.** M.L. Danilevsky: "genus *Rhagium* Fabricius, 1775: 182... subgenus *Hagrium* Villiers, 1978: 85... *bifasciatum* Fabricius, 1775: 183 E: ... A: AB AR GG IN TR #104".

Remarks. As in the first edition [Catalogue..., 2010: 132], Danilevsky recorded this species from Azerbaijan, Armenia, and Iran without any details. However, Miroshnikov [2011a: 19–20, 41–42] noted that the records of *Rh. bifasciatum* from these countries require reliable confirmation, and the information about its occurrence in the Caspian coastal forests of Iran [Adeli, 1972: "Kaspische Walder"] undoubtedly belongs to some other species.

Continuing with the same work by Miroshnikov [2011a], Danilevsky [2015a: 81], completely disregarding the reference to it, have thus noted that this species "может встречаться в Армении и Азербайджане, хотя автору неизвестны ни соответствующие публикации, ни экземпляры... указание для Ирана (Abai, 1969) сомнительно" (may occur in Armenia and Azerbaijan, although the author knows neither the corresponding publications, nor the specimens... the record from Iran (Abai, 1969) is doubtful). In this work, thereby he did not explain in any way his own data on the distribution of *Rh. bifasciatum* in Armenia, Azerbaijan, and Iran as presented in the first edition [Catalogue..., 2010], but only provided a brief reference "Danilevsky & Smetana, 2010: 132" and nothing more.

Such inconsistency and duplicity of Danilevsky's data seem to be at least strange, and this was already highlighted before [Miroshnikov, 2016: 206]. As usual, he deliberately did not provide a reference to Miroshnikov's works and repeatedly gave incorrect data on the distribution of this species in the regions under discussion.

Given the above, Azerbaijan (AB), Armenia (AR), and Iran (IN) should be excluded from the distribution area of *Rhagium (Hagrium) bifasciatum*.

– **Pages 174–175.** M.L. Danilevsky: "genus *Rhagium* Fabricius, 1775: 182... subgenus *Megarhagium* Reitter, 1913a: 6...

causicum causicum Reitter, 1889c: 287 ?E: ?ST A: AB AR GG TR".

Remarks. In the first edition [Catalogue..., 2010: 132], the distribution area of this taxon was indicated as follows: "E: AB AR GG ST". Miroshnikov [2011b: 554–555] discussed in detail the distribution of this subspecies in the Caucasus and the adjacent part of Turkey, providing reliable locality information and discussing the erroneous nature of some records from Abkhazia. Danilevsky [2015a], ignoring this paper, very confusingly and largely erroneously described the distribution area of *Rh. causicum causicum*. He noted, in particular, that this subspecies is distributed "from the Northwest Caucasus (i.e. Krasnodar Region and the Republic of Adygea. – A.M.) throughout Transcaucasia (including Abkhazia. – A.M.)". Miroshnikov [2016]

critically examined Danilevsky's data and convincingly demonstrated their inconsistency.

In the updated edition of the catalogue, as noted above, Danilevsky only placed a question mark near the "ST" symbol (in this case, referring to the territory of the Northwest Caucasus) and added Turkey without any details and compulsory references to Miroshnikov's [2011b; 2016] papers that contain the most important reliable information on the distribution of this taxon.

Considering the above, the south of the European part of Russia (ST) (meaning the Northwest Caucasus) should be excluded from the distribution area of *Rhagium causicum causicum* and Gombori in Kakheti and Tbilisi should be accepted as the northernmost localities of this taxon in the Caucasus [Miroshnikov, 2016: 196, fig. 46].

– **Pages 174–175.** M.L. Danilevsky: "genus *Rhagium* Fabricius, 1775: 182... subgenus *Megarhagium* Reitter, 1913a: 6...

causicum semicorne Holzschuh, 1974b: 118 A: AB IN".

Remarks. Must be: A: AB (Talysh) IN [Miroshnikov, 2016].

– **Pages 174–176.** M.L. Danilevsky: "genus *Rhagium* Fabricius, 1775: 182... subgenus *Rhagium* Fabricius, 1775: 182...

inquisitor inquisitor Linnaeus, 1758: 393...

...

nubecula Bergstrasser, 1778: 26 (*Cerambyx*)".

Remarks. Must be:

nubecula Bergstrasser, 1778: 26 (*Cerambyx*)

sudeticum Plavilstshikov, 1915: 46.

The latter name is omitted in the updated catalogue, and in the first edition it was indicated with a wrong page (35) [Miroshnikov, 2011a: 11, 42].

– **Pages 174–176.** M.L. Danilevsky: "genus *Rhagium* Fabricius, 1775: 182... subgenus *Rhagium* Fabricius, 1775: 182...

inquisitor schtschukini Semenov, 1898: 601 E: ST A: AB AR GG TR".

Remarks. As in the first edition [Catalogue..., 2010: 133], Danilevsky recorded this subspecies from Azerbaijan and Armenia without any comments. However, Miroshnikov [2011a: 20, 43] noted that the record from Azerbaijan requires reliable confirmation, and the accurate data from Armenia are still not known.

Following up on work done by Miroshnikov [2011a], Danilevsky [2015a: 81] ignoring the reference to it, did not mention *Rh. inquisitor schtschukini* neither for Azerbaijan nor for Armenia, but only indicated the possibility of records from the territory of the last country.

Once again, there has been an inconsistency of the data of this author. In addition, he, as usual, deliberately did not provide a reference to Miroshnikov's work in the updated catalogue.

Given the above, Azerbaijan (AB) should be excluded from the distribution area of this taxon, and Armenia (AR) must be left under the question mark since there are some reliable records from the neighboring areas of northeastern Turkey. Thus, the following entry should be made: E: ST (NW & N Caucasus) A: ?AR GG TR.

– **Page 179.** “genus *Neorhamnusium* Hayashi, 1976: 1...
rugosipenne Pic, 1939b: 2 (*Rhamnusium*) A: SHA SHX #225”.

Remarks. Comment #225 (page 63), supposedly for this species, has no relation to it, as well as to any other species of this genus.

– **Page 179.** M.L. Danilevsky: “genus *Rhamnusium* Latreille, 1829: 130...”

bicolor graecum Schaufuss, 1862: 311...

limbatum Pic, 1897c 30”.

Remarks. In the first edition [Catalogue..., 2010: 135], this name was given as “*limbatum* Pic, 1901h: 31”. Miroshnikov [2013: 15] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not provide a reference to this paper, and presented the results of Miroshnikov’s research as his own. In addition, there must be: *limbatum* Pic, 1897c: 30.

– **Page 180.** “tribe Teledapini Pascoe, 1871
genus *Teledapalpus* Miroshnikov, 2000a: 38 type species:
Teledapalpus murzini Miroshnikov, 2000
cremarius Holzschuh, 1999: 6 (*Teledapus*) A: SHA
hospes Holzschuh, 1999: 6 (*Teledapus*) A: GAN
murzini Miroshnikov, 2000a: 41 A: SCH
zamotajlovi Miroshnikov, 2000a: 41 A: SCH
zlotichini Miroshnikov, 2000a: 41 A: SHA
genus *Teledapus* Pascoe, 1871: 268 type species *Teledapus dorcadioides* Pascoe, 1871
celsicola Holzschuh, 1999: 5 A: XIZ
dorcadioides Pascoe, 1871: 269 A: UP
ocularis Holzschuh, 1981: 94 A: PA
picatus Holzschuh, 2003a: 149 A: SCH
pilosellus Holzschuh, 2007: 181 A: SCH
querceti Holzschuh, 2007: 180 A: UP”.

Remarks. Based on the revision of the tribe Teledapini [Miroshnikov, 2021a], taking into account Ohbayashi’s and Chou’s [2021] paper, the following changes and additions should be made:

genus *Teledapalpus* Miroshnikov, 2000a: 38...
cremarius Holzschuh, 1999: 6 (*Teledapus*) A: SHA
daliensis Miroshnikov, 2021: 242 A: YUN
hospes Holzschuh, 1999: 6 (*Teledapus*) A: GAN
linyeyiei Huang, Li et Zhang, 2021: 441 (*Teledapus*) A: YUN
lobanovi Miroshnikov, 2021: 242 A: SCH
murzini Miroshnikov, 2000a: 41 A: SCH
picatus Holzschuh, 2003a: 149 (*Teledapus*) A: SCH
pilosellus Holzschuh, 2007: 181 (*Teledapus*) A: SCH
transitivus Miroshnikov, 2021: 247 A: YUN
uenoi N. Ohbayashi et Chou, 2021: 200 A: YUN
zamotajlovi Miroshnikov, 2000a: 41 A: SCH
zlotichini Miroshnikov, 2000a: 41 A: SHA
genus *Teledapus* Pascoe, 1871: 268...
celsicola Holzschuh, 1999: 5 A: XIZ
dorcadioides Pascoe, 1871: 269 A: HP UP
ocularis Holzschuh, 1981: 94 A: PA
querceti Holzschuh, 2007: 180 A: UP.

– **Pages 180–181.** M.L. Danilevsky: “genus *Leptorhabdium* Kraatz, 1879d: 118...
causicum Kraatz, 1879d: 118 (*Psilorhabdium*) E: ST
A: AB AR GG TR”.

Remarks. Danilevsky did not provide any comments on the distribution of this species. However, Miroshnikov [2011a: 43–44] noted that the record of *L. causicum* from Azerbaijan requires confirmation, although some of its localities are known in the north-east of Armenia.

Following up on the work done by Miroshnikov [2011a], Danilevsky [2015a: 68], ignoring the reference to it, stated that “экземпляры из Азербайджана до сих пор не известны” (the specimens from Azerbaijan are still not known). Once again, there has been an inconsistency of the data of this author.

Considering the above, Azerbaijan (AB) should be excluded from the distribution area of *Leptorhabdium causicum* for now.

– **Page 189.** M.L. Danilevsky: “genus *Apatophysis* Chevrolat, 1860a: 95... subgenus *Angustophysis* Pic, 1956a: 2...”

margiana Semenov & Shchegoleva-Barovskaya, 1936: 77...
plavilstshikovi Miroshnikov, 1992: 392”.

Remarks. Miroshnikov [2014a: 16–18, 39–41, figs 12–34] convincingly showed the fallacy of this synonymy established by Danilevsky [2008].

Danilevsky deliberately did not provide a reference to the former work.

Given the above, *Apatophysis plavilstshikovi* Miroshnikov, 1992 described from Turkmenistan should be accepted as a valid species. In addition, there must be: Stshegoleva-Barovskaja.

Thus, the following corrections need to be made:
margiana Semenov & Stshegoleva-Barovskaja, 1936: 77...
plavilstshikovi Miroshnikov, 1992: 392 A: TM.

– **Page 190.** “genus *Formosotoxotus* Hayashi, 1960a: 1...
gressitty Miroshnikov & Lin, 2014: 117...”

Remarks. Must be: *gressitty*.

– **Page 190.** “genus *Protapatophysis* Semenov & Stshegoleva-Barovskaia, 1936: 26...”

Remarks. The updated catalogue should include *Protapatophysis hindukushensis* Miroshnikov, 2020 [Miroshnikov, 2020c: Pakistan] published on June 26, 2020. The catalogue itself was published only on December 16, 2020 and, most likely, this species was just missed, as in the case of *Diorthus kabakovi* Miroshnikov, 2018 (see above).

In addition, there must be: Stshegoleva-Barovskaja.

– **Page 190.** “genus *Trypogeus* Lacordaire, 1869: 236...
guangxiensis Miroshniko & Liu, 2016a: 255...”

Remarks. Must be: Miroshnikov.

– **Page 191.** M.L. Danilevsky: “genus *Icosium* P.H. Lucas, 1854: viii...
tomentosum atticum Ganglbauer, 1882: 743 E: ... CY... A: ...
CY...”

Remarks. Must be: A: ... CY...

– **Pages 191–192.** M.L. Danilevsky: “genus *Anaglyptus* Mulsant, 1839: 91... subgenus *Anaglyptus* Mulsant, 1839: 91...
arabicus Kuster, 1847b: 95 (*Clytus*) E: ST A: ?AB AR
GG TR”.

Remarks. This species is reliably known from the northern part of Azerbaijan [Miroshnikov, 2000: 64, 74, Kazakh, Evlakh]. However, Danilevsky deliberately did not

provide a reference to this paper and did not comment on the indicated localities in any way.

Considering the above, Azerbaijan (AB) should be included in the distribution area of *Anaglyptus arabicus* without a question mark.

– **Pages 191–192.** M.L. Danilevsky: "genus *Anaglyptus* Mulsant, 1839: 91... subgenus *Anaglyptus* Mulsant, 1839: 91... *danilevskii* Miroshnikov, 2000b: 77 A: AB AR GG IN TR".

Remarks. The first records of this species from eastern Anatolia (Erzincan Prov., 4 km E Gemecik vill.) were published by Miroshnikov [2011a: 20, 45] based on the material kindly provided by Dr Denis G. Kasatkin (Rostov-on-Don, Russia).

Danilevsky deliberately did not provide a reference to this work, and presented the results of Miroshnikov's research as his own.

In addition, in this monograph [Miroshnikov, 2011a: 11, 45], attention is drawn to the correct spelling of the name (in the first edition as *danilevskiy*). But Danilevsky also kept silent about this.

– **Pages 191–192.** M.L. Danilevsky: "genus *Anaglyptus* Mulsant, 1839: 91... subgenus *Anaglyptus* Mulsant, 1839: 91... *mysticoides mysticoides* Reitter, 1894c: 128 ["Centralen Kaukasus"] A: AB AR GG TR".

Remarks. Danilevsky did not explained the area [Reitter, 1894: 128, "Centralen Kaukasus"] in any further way, leaving the reader with another riddle. In fact, in the revision of the Caucasian species of the genus *Anaglyptus* [Miroshnikov, 2000: 67], the data from the type specimens of this species are given with the corresponding labels (including their images): "1♂ (HNHM), "Caucasus. Meskisch. Gb. Leder. Reitter", "A. *mysticoides* m. 1994", "Coll. Reitter", "Lectotypus, design. G. Sama, 1999"; "1♀ (HNHM), "Kaukas Leder", "Coll. Reitter", "Paralectotypus, design. G. Sama, 1999". It should be thereby noted that the lectotype designation for this taxon remains unpublished.

Already Leder [1881: 501] more accurately described the area given on the first label (see above): "Mein erstes Standquartier war das aus meinen älteren Berichten her bekannte Michailowo in der Nähe von Suram, von wo aus ich leicht zu Fuss oder mittelst der Eisenbahn in das so ergiebige "Meskische Gebirge" (Meskisches Gebirge. – A.M.) gelangen konnte. Anfangs Juni fuhr ich über Borshom und Achalzich nach dem von Alters her bekannten Badeorte Abastuman". This Leder's locality (Meskheta = Meskhetian Mountain Range near Abastumani, Georgia) is mentioned in various other papers [Jäch, 1992; Savitsky, 1997; Germann, 2020; and others]. It is located just in the central part of the Caucasian Isthmus. Without any doubt, Reitter's [1894] "Centralen Kaukasus" was based exactly on this locality.

The Meskhetian Mountain Range is also clearly given in the list of reliable Caucasian localities for *A. mysticoides* in the above revision [Miroshnikov, 2000: 67]. Danilevsky deliberately did not provide a reference to this paper, which contains a number of important data on the distribution, morphology, and bibliography of the Caucasian *Anaglyptus*.

– **Pages 191–192.** M.L. Danilevsky: "genus *Anaglyptus* Mulsant, 1839: 91... subgenus *Anaglyptus* Mulsant, 1839: 91...

mysticoides obscurissimus Pic, 1901n: 59 ["Tokat"] A: TR #155";

– **Page 54.** M.L. Danilevsky: "#155 *Anaglyptus mysticoides obscurissimus* Pic, 1901 was accepted by Özdikmen, Atak & Uckan (2017a)".

Remarks. In fact, this taxon is only an extreme melanistic form, differing from the typical colour form in the entirely black elytra and nothing else. It should be thereby noted that both colour forms are observed in the same populations in Anatolia. In particular, I studied several males from Sivas (collection of Stanislav Kadlec's, now at the Národní Museum, Prague), one of which has entirely black elytra, and the rest of the specimens demonstrate a typical colouration [Miroshnikov, 2000: 67, 82, figs 11–12].

It should be noted that the forms with entirely or almost completely black elytra have also been observed in the other *Anaglyptus* species [Plavilstshikov, 1940; Heyrovský, 1955; Panin, Sävulescu, 1961; Villiers, 1978; Miroshnikov, 2000; Sama, 2002; and many others], thereby they are not geographically isolated in any way and appear intermixed with a typical form in the same populations.

Given the above, *Anaglyptus mysticoides* Reitter, 1894 = *Anaglyptus mysticoides obscurissimus* Pic, 1901.

– **Page 193–194.** M.L. Danilevsky: "genus *Paraclytus* Bates, 1884a: 234...

raddei Ganglbauer, 1882: 737... A: AB IN...

reitteri Ganglbauer, 1882: 737... A: AB IN".

Remarks. For both species there must be: A: AB (Talysh) IN [Miroshnikov, 2014b].

– **Page 196.** M.L. Danilevsky: "genus *Aromia* Audinet-Serville, 1834a: 559...

moschata ambrosiaca Steven, 1809: 40...

thoracica Fischer von Waldheim, 1823: tab. 48, figs 3, 4. [1824: 236]...;

– **Page 280.** M.L. Danilevsky: "genus *Purpuricenus* Dejean, 1821: 105...

budensis Gotz, 1783: 72...

wredii Fischer von Waldheim, 1823: tab. 49, fig. 2 [1824: 238]".

Remarks. In the first edition [Catalogue..., 2010: 147, 198], these two synonyms were given as follows: "*thoracicus* Fischer von Waldheim, 1824: 236" and "*wredii* Fischer von Waldheim, 1824: 238". Miroshnikov [2013: 15–16] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not provide a reference to this paper, and presented the results of Miroshnikov's research as his own.

– **Page 198.** "genus *Chloridolum*...

nadieri Skale, 2018a: 234...

jeanvoinei Pic, 1937d: 108 (*Chelidonium*) [HM]..."

Remarks. Must be: [HN].

– **Page 204.** M.L. Danilevsky: "genus *Callidium* Fabricius, 1775: 187... subgenus *Callidostola* Reitter, 1913a: 37...

aeneum longipenne Plavilstshikov, 1940: 300 E: ST A: AB GG TR".

Remarks. Miroshnikov [2011a: 20, 47] noted that the record of this taxon from Azerbaijan requires reliable confirmation. The locality of *C. aeneum longipenne* closest to the territory of this country is only the vicinities of Tbilisi.

Danilevsky did not comment on these remarks in any way and did not provide evidence of the records from Azerbaijan. He deliberately did not give a reference to the mentioned work.

Considering the above, Azerbaijan (AB) should be excluded from the distribution area of *Callidium aeneum longipenne*.

– **Page 205.** M.L. Danilevsky: “genus *Leioderes* L. Redtenbacher, 1845: 110... *kollari kollari* L. Redtenbacher, 1849: 482 E: ... A: GG *tuerki* Ganglbauer, 1886c: 517 (*Callidium*) A: LE SY TR”.

Remarks. In the first edition [Catalogue..., 2010: 151], the following was noted: “genus *Leioderes*... *kollari* L. Redtenbacher, 1849: 482 E: ... GG... A: TR *tuerki* Ganglbauer, 1886c: 517 (*Callidium*) A: LE SY TR”.

Miroshnikov [2011a: 20, 47] noted that the only indication of *L. kollari* for the Caucasus, based on the material derives from the vicinities of Tbilisi [Eichler, 1930], requires reliable confirmation and most likely belongs to a different species.

According to Sama [2002], *L. kollari* is distributed exclusively in Europe, and its old records from Syria and Asia Minor should be attributed to *L. tuerki*. On this basis, Miroshnikov [2011b: 560] noted that the report on the discovery of *L. kollari* in Tbilisi for the time being should also be attributed to *L. tuerki*, but the distribution in the Caucasus of the *Leioderes* representative generally needs reliable confirmation.

Danilevsky excluded Turkey (TR) from the distribution area of *L. kollari* and left only Georgia (GG) without commenting on these data. He deliberately did not refer to either Sama [2002] or both of Miroshnikov’s works.

Given the above, Georgia (GG) should be excluded from the distribution area of *Leioderes kollari* and this country should be included in the distribution area of *Leioderes tuerki* under a question mark. These corrections are most appropriately consistent with the distribution peculiarities of both species.

– **Pages 206–207.** M.L. Danilevsky: “genus *Phymatodes* Mulsant, 1839: 47... subgenus *Phymatoderus* Reitter, 1913a: 39... *glabratus* Charpentier, 1825: 225 (*Callidium*) E: ... ST... A: GG LE #115 #464”.

Remarks. Miroshnikov [2011a: 20–21, 48–49; 2011b: 560] described in detail the history of the study of this species in the Caucasus and noted that until now, its reliable records from the region and also from Crimea are missing. On this basis, *Phymatodes glabratus* was excluded from the Caucasian fauna [Miroshnikov, 2011b: 560].

Danilevsky does not comment on this information in any way and deliberately did not provide a reference to the mentioned works.

Considering the above, Georgia (GG) and the south of the European part of Russia (ST) (meaning the Russian Caucasus) should be excluded from the distribution area of *Phymatodes glabratus*.

– **Pages 206–207.** M.L. Danilevsky: “genus *Phymatodes* Mulsant, 1839: 47... subgenus *Phymatoderus* Reitter, 1913a: 39...

lividus Rossi, 1794: 98 (*Callidium*) E: ... ST... A: AB GG IS LE SY TR”.

Remarks. Miroshnikov [2011a: 20–21, 48–49; 2011b: 560] described in detail the history of the study of this species in the Caucasus and noted that until now its reliable records from the region are not known, except for the only Plavilstshikov’s [1915] very old record from Burgun-Madjary, Stavropol Region.

Danilevsky does not comment on this information in any way and deliberately did not provide a reference to Miroshnikov’s works.

Given the above, Azerbaijan (AB) and Georgia (GG) should be excluded from the distribution area of *Phymatodes lividus*.

– **Pages 206–207.** M.L. Danilevsky: “genus *Phymatodes* Mulsant, 1839: 47... subgenus *Phymatoderus* Reitter, 1913a: 39...

puncticollis Mulsant, 1862: 99 E: ... ST UK... #180”.

Remarks. The distribution of this species in the Caucasus, including the Russian territory, has practically not been studied. Miroshnikov [2011b: 561] confirmed the indication of this species from the Ciscaucasia [Plavilstshikov, 1940, 1955, 1965; Arzanov et al., 1993] based on the specific material from Maykop, Republic of Adygea.

Danilevsky deliberately did not provide a reference to Miroshnikov’s paper, which contains the most important reliable data on the records of *Phymatodes puncticollis* from the Russian Caucasus (ST).

– **Pages 206, 208.** M.L. Danilevsky: “genus *Phymatodes* Mulsant, 1839: 47... subgenus *Phymatodes* Mulsant, 1839: 47...

testaceus Linnaeus, 1758: 396...

luridus Paykull, 1800: 87 (*Callidium*)”.

Remarks. Must be: *luridus* Paykull, 1800: 87 (*Callidium*) [HN] [Miroshnikov, 2011a: 11, 48].

– **Pages 209–210.** M.L. Danilevsky: “genus *Ropalopus* Mulsant, 1839: 40... subgenus *Ropalopus* Mulsant, 1839: 40...

lederi Ganglbauer, 1882: 747 (*Rhopalopus*) E: ST... A: AB AR GG TR”.

Remarks. Miroshnikov [2011a: 20, 47] noted that the record of this species from Azerbaijan requires reliable confirmation. The localities of *R. lederi* closest to the territory of this country are only the vicinities of Tbilisi, Georgia, and Dilizhan, Armenia. In addition, the distribution of this species in the north-east of Anatolia was noted for the first time.

Danilevsky deliberately did not give a reference to this work but included the Asian part of Turkey, noted by Miroshnikov, under the guise of the results of his own research.

Given the above, Azerbaijan (AB) should be excluded from the distribution area of *Ropalopus lederi*.

– **Page 215.** M. Lin: “genus *Carinolesthes* Vitali, Gouverneur & Chemin, 2017: 53...

ningshanensis Chiang, 1981: 79 (*Aeolesthes*) A: SHA”.

Remarks. Miroshnikov [2019b] noted that this species is significantly different from other members of the genus. Probably it is a transitional form between *Carinolesthes* and *Pseudaeolesthes*, seemingly closer to the latter genus. But its generic attribution needs to be clarified. This should be kept in mind when considering the species.

– **Page 215.** M.L. Danilevsky: "genus *Cerambyx* Linnaeus, 1758: 388... subgenus *Cerambyx* Linnaeus, 1758: 388..."

dux Faldermann, 1837: 264 (*Hammaticherus*)...

intricatus Fairmaire, 1848: 167 (*Hammaticherus*)".

Remarks. Miroshnikov [2009b: 50–51; 2011a: 12, 49–50] demonstrated in great detail that this synonymy is highly controversial, and the distribution of *C. dux* in Western Europe is not entirely clear.

Danilevsky deliberately did not provide a reference to these works containing data important for the catalogue, and did not comment on them in any way.

Given the above, the synonym under consideration must be indicated as follows: ? *intricatus* Fairmaire, 1848: 167...

– **Page 215.** M.L. Danilevsky: "genus *Cerambyx* Linnaeus, 1758: 388... subgenus *Cerambyx* Linnaeus, 1758: 388..."

heinzianus Demelt, 1976: 65 A: TR".

Remarks. In the first edition [Catalogue..., 2010: 159], the distribution area of this species was given as follows: "E: GG". Miroshnikov [2011a: 21, 50–51] noted that *C. heinzianus* was described from eastern Anatolia [Demelt, 1976], while it was recorded erroneously from Georgia.

Danilevsky deliberately did not give a reference to Miroshnikov's work, and presented the research results indicated in it as his own.

– **Pages 215–216.** M.L. Danilevsky: "genus *Cerambyx* Linnaeus, 1758: 388... subgenus *Cerambyx* Linnaeus, 1758: 388..."

miles Bonelli, 1812: 178 E: ... ST... UK..."

Remarks. Miroshnikov [2009b: 52; 2011a: 21, 51] discussed in detail the data on the distribution of this species in the Russian part of the Caucasus and Crimea and noted that they need reliable confirmation.

Danilevsky deliberately did not give a reference to these works and did not comment in any way on the data important for the catalogue indicated in them.

Given the above, it is necessary to present the considered fragments of the distribution area of *C. miles* as follows: E: ... ?ST... ?UK (Crimea)...

In the first edition of the catalogue [2010: 57], in the section "New acts and comments", the correct publication date of Bonelli's work describing *C. miles* was justified. However, Miroshnikov [2004a: 110] had shown even earlier that this date is 1812, and not 1823, as indicated in many literary sources.

Danilevsky deliberately ignored this paper well known to him, in which the aforementioned important data were noted for the first time.

– **Pages 215–216.** M.L. Danilevsky: "genus *Cerambyx* Linnaeus, 1758: 388... subgenus *Cerambyx* Linnaeus, 1758: 388..."

nodulosus nodulosus Germar, 1817: 220 E: ... ST... A: AB AR... GG..."

Remarks. Miroshnikov [2009b: 51; 2011a: 21, 51] discussed the distribution of *C. nodulosus* in the Caucasus and noted that this species is still not known in Azerbaijan, while in the Russian part of the Caucasus it is distributed only on the Black Sea coast of Krasnodar Region.

Danilevsky deliberately did not give a reference to these works and did not provide any evidence about the records of this species from Azerbaijan.

Given the above, it is necessary to exclude Azerbaijan (AB) from the distribution of *Cerambyx nodulosus*, while the above Russian fragment of the distribution area should be presented as follows: E: ...ST (NW Caucasus)...

– **Page 216.** "genus *Derolus* Gahan, 1891a: 26..."

Mimoderolus Pic, 1933a: 11 type species *Aeolesthes (Mimoderolus) uniformis* Pic, 1933 #207".

Remarks. Miroshnikov [2018b] showed that *Mimoderolus* Pic, 1933 is in fact a synonym of the genus *Tapinolachnus* J. Thomson, 1865.

Considering the above, *Mimoderolus* should be excluded from the synonyms of *Derolus*.

– **Page 217.** M.L. Danilevsky: "genus *Dissopachys* Reitter, 1886: 68..."

pulvinatus Reitter, 1886: 68 A: AB IN TM".

Remarks. Must be: A: AB (Talysh) IN TM (Kopetdag).

– **Page 217.** "genus *Dymasius* J. Thomson, 1864: 234 type species *Dymasius strigosus* J. Thomson, 1864 (= *Cerambyx macilentus* Pascoe, 1859) #400".

Remarks. *Dymasius strigosus* J. Thomson, 1864 has been resurrected from the synonymy with *Dymasius macilentus* (Pascoe, 1859) [Miroshnikov, 2018b].

– **Page 217.** "Genus (genus. – A.M.) *Elydnus* Pascoe, 1869a: 509 type species *Elydnus amictus* Pascoe, 1869 #400 *hirayamai* Matsushita, 1941: 153 A: JA (Ryukyus) TAI *kisanus* Matsushita, 1935: 540 A: TAI".

Remarks. Both species should be attributed to the genus *Dymasius* J. Thomson, 1864 [Miroshnikov, 2017b: 183]. The members of the genus *Elydnus* Pascoe, 1869 are still unknown from the territory covered by the catalogue [Miroshnikov, 2017b, 2019b; Miroshnikov, Tichý, 2018].

The synonymy "*Dymasius* (s. str.) = *Dymasius (Elydnus)* Pascoe, 1869a) were accepted", indicated in comment #400 (page 86) requires clarification from the authors or editor of the catalogue.

– **Page 218.** "genus *Margites* Gahan, 1891a: 26... subgenus *Margites* Gahan, 1891a: 26..."

Remarks. The generic status of *Laomargites* was restored by Miroshnikov [2018b]. Previously [Gressitt, Rondon, 1970], this taxon was considered as a subgenus of the genus *Margites*.

Considering the above, the genus *Margites* should be accepted without an intrageneric classification.

– **Page 218.** "genus *Margites* Gahan, 1891a: 26... *lajoyei* Pic, 1926f: 76 A: YUN ORR".

Remarks. Miroshnikov [2018b] noted that the record of this species from Yunnan [Catalogue..., 2010: 161] requires confirmation since it was described from southern Vietnam and is still reliably known only from the holotype.

Therefore, if to include Yunnan in the distribution area of *Margites lajoi*, then this should be made no more than under a question mark: A: ?YUN **ORR**.

– **Page 218**. “genus *Massicus* Pascoe, 1867a: 319 [RN]... *Falsomassicus* Pic, 1946a: 7 type species *Falsomassicus theresae* Pic, 1946”;

– **Page 219**. “genus *Neocerambyx* J. Thomson, 1861: 194...

theresae Pic, 1946a: 7 (*Falsomassicus*) A: CH”.

Remarks. It is entirely wrong to regard *Falsomassicus* as a synonym for the genus *Massicus*, while its type species *F. theresae* is attributed to the genus *Neocerambyx*.

As noted above, *Falsomassicus theresae* was transferred to the genus *Neocerambyx* with the establishment of a new combination [Miroshnikov, 2020a: 79].

Falsomassicus should be considered as a synonym for the genus *Neocerambyx*.

– **Pages 218–219**. “genus *Massicus* Pascoe, 1867a: 319 [RN]...

taiwanus Makihara & Niisato, 2014: 24 (*Massicus*) A: TAI”.

Remarks. Must be: *taiwanus* Makihara & Niisato, 2014: 24 A: TAI.

– **Page 220**. “genus *Parolesthes* Vitali, Gouverneur & Chemin, 2017: 58...

laosensis Gressitt & Rondon, 1970: 64 A: YUN **ORR**”.

Remarks. Must be: *laosensis* Gressitt & Rondon, 1970: 64 (*Aeolesthes*)...

– **Page 220**. “genus *Trachylophus* Gahan, 1888b: 59... *rugicollis* Gressitt, 1948a: 48 A: FUJ GUA GUI HAI HKG HUB HUN JIX SCH #400”.

Remarks. This species was transferred to the genus *Neocerambyx* by Miroshnikov [2020a: 80] on the basis of a detailed study of the holotype.

The indication of the listed provinces (excluding Sichuan, from where the species was described), both in the considered catalogue and in the previous Chinese catalogue [Chen et al., 2019: 135], in my opinion [Miroshnikov, 2021b], requires confirmation. By the way, Niisato (page 23) and Oh (page 24) have already excluded Taiwan and Korea, which were mentioned for this species in the same Chinese catalogue [Chen et al., 2019].

Jacquot [2020] recorded *Neocerambyx rugicollis* from Yunnan and provided a picture of the corresponding specimen.

– **Pages 226, 229**. M.L. Danilevsky: “genus *Chlorophorus* Chevrolat, 1863: 290...

herbstii Brahm, 1790: 148 (*Leptura*) E: ... ST...”

Remarks. Miroshnikov [Miroshnikov, 2009a: 792] discussed in detail the history of the study of this species in the Caucasus and demonstrated that it is actually absent in the region, including its Russian territory.

Danilevsky deliberately did not provide a reference to this paper, which contains the most important data on the distribution of *Ch. herbstii* in the south of the European part of Russia (ST).

– **Pages 226, 232**. M.L. Danilevsky: “genus *Chlorophorus* Chevrolat, 1863: 290

varius varius O.F. Muller, 1766: 188...

c-duplex Scopoli, 1786: 46 (*Stenocorus*)”.

Remarks. The list of synonyms omits *Clytus* (*Clytanthus*) *aegyptiacus* Ganglbauer, 1882 733 (HN) given in the first edition [Catalogue..., 2010], but not mentioned as a homonym [Miroshnikov, 2011a: 13, 53]. In the same work, the attention was drawn to the correct publication date of *Stenocorus c-duplex* Scopoli, 1786.

Danilevsky deliberately did not provide a reference to this work.

– **Pages 233, 235**. M.L. Danilevsky: “genus *Clytus* Laicharting, 1784... subgenus *Clytus* Laicharting, 1784: 88... *schneideri schneideri* Kiesenwetter, 1879a: 313 [1879b: 57] A: AR AB GG IN TR”.

Remarks. Miroshnikov [2011a: 21, 53] noted that the indications of this species from Azerbaijan and Iran require reliable confirmation. Until now, there are no credible records of *C. schneideri* from these regions. Previously, Sama et al. [2008] excluded this species from the fauna of Iran.

Danilevsky did not provide any evidence of the distribution of *C. schneideri* in these countries and deliberately did not refer either to Miroshnikov’s monograph or to the work of Sama et al.

Given the above, Azerbaijan (AB) and Iran (IN) should be excluded from the distribution area of *Clytus schneideri*.

– **Page 245**. M.L. Danilevsky: “genus *Pseudosphegesthes* Reitter, 1913a: 50...

brunnescens Pic, 1897g: 262 (*Clytus*) E: ST A: AB AR GG ?IN TR”.

Remarks. In the first edition [Catalogue..., 2010: 177], the distribution area of this species was given as follows: “E: GG ST”. Miroshnikov [2011a: 21, 59], based on the reliable data, recorded *P. brunnescens* from Azerbaijan, Armenia, and Turkey (Artvin).

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Pages 249, 251**. M.L. Danilevsky: “genus *Xylotrechus* Chevrolat, 1860d: 456... subgenus *Xylotrechus* Chevrolat, 1860d: 456...

arvicola arvicola Olivier, 1800: 64...

kraatzii Lederer, 1864: 485 (*Clytus*)”.

Remarks. Must be: *kraatzii*... [Miroshnikov, 2011a: 14, 60].

– **Pages 249, 251**. M.L. Danilevsky: “genus *Xylotrechus* Chevrolat, 1860d: 456... subgenus *Xylotrechus* Chevrolat, 1860d: 456...

arvicola iranica Rapuzzi & Sama, 2014a: 7 A: AB IN #106”.

Remarks. Must be: A: AB (Talysh) IN...

– **Page 257**. M.L. Danilevsky: “genus *Deilus* Audinet-Serville, 1834b: 73...

fugax Olivier, 1790: 253 (*Callidium*) E: ... A: AB AR... GG... SY TR #11”.

Remarks. Miroshnikov [2011a: 22, 61; 2011b: 559] discussed the distribution of this species in the Caucasus and gave only a few old records from Borzhomi, Georgia, and Dilizhan, Armenia, and also noted that the data from Azerbaijan requires confirmation.

Danilevsky did not comment on these publications in any way and deliberately did not give a reference to them.

Given the above, Azerbaijan (AB) should be excluded from the distribution area of *Deilus fugax*.

– **Page 265.** M.L. Danilevsky: “genus *Molorchus* Fabricius, 1793: 356... subgenus *Caenoptera* C.G. Thomson, 1859: 150...

minor minor Linnaeus, 1758: 421 (*Necydalis*) E: ... ST... A: ... GG...

monticola Plavilstshikov, 1931: 38 A: AB AR GG IN TM”.

Remarks. Miroshnikov [2011a: 22, 62] discussed in detail the distribution of *M. minor minor* in Georgia, which was not noted in the first edition [Catalogue..., 2010: 191]. It was thereby given that the specimens of this taxon from the Northwest Caucasus and central Georgia (Tsagveri) clearly differ from specimens of *M. monticola* from the Talysh Mountains, Azerbaijan (the type locality of the latter species). In addition, it was noted that the distribution of both species in the Caucasus had been very poorly studied and, so far, it has not been possible to outline even approximate boundaries of the distribution area of these forms in the region.

Danilevsky did not comment on this publication in any way and deliberately did not give a reference to it. However, he used Miroshnikov’s data and included Georgia (GG) in the distribution area of *M. minor minor* under the guise of a result of his own research.

– **Page 279.** “genus *Euryphagus* J. Thomson, 1864: 196...

miniatus Fairmaire, 1904a: 145 (*Purpuricenus*)...

caputorubeus S.T. Yu, 1935: 10 (*Purpuricenus*)”.

Remarks. In the first edition of [Catalogue..., 2010: 198], the latter name was given as follow: “...*caputorubens* P.-Y. Yu, 1935: 1”.

Through the courtesy of Dr Hiroshi Makihara (Isumi, Chiba, Japan), I received a hard-to-find original publication [Yu, 1935] and made the appropriate corrections, – *caputorubeus* S.T. Yu, 1935: 10 [Miroshnikov, 2013: 16]. This name is now correctly listed in the updated catalogue.

– **Pages 280, 282.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...

caucasicola Danilevsky, 2015e: 386 E: ST A: AB ?AR GG

...

neocaucasicus Rapuzzi & Sama, 2014b: 156 A: TR”.

Remarks. *Purpuricenus caucasicola*, described by Danilevsky [2015b] based on the material collected mainly by me [Miroshnikov, 2012], in fact, does not have any reliable morphological differences from *P. neocaucasicus*. The type series of the latter species includes specimens from various regions of both Anatolia and the Caucasus (in particular, Lisi Lake, Tbilisi, Georgia) [Rapuzzi, Sama, 2014: 156–157]. I repeatedly drew the attention of Danilevsky to this fact prior his publication [Danilevsky, 2015b], discussing this issue in detail in our personal correspondence (December 2014 – April 2015). However, this researcher stayed true to the specific status of the Caucasian form, but even in the original description, he failed to provide any clear evidence of this. Danilevsky [2015b: 387] indicated only the following extremely dubious differences between *P. neocaucasicus* and *P. caucasicola*: “in general *P. caucasicola* sp. n. is distinctly narrower, with more red pronotum; anterior margin of black elytral area is rounded, very rare emarginated”. I studied in detail some material from various regions of Anatolia and the Caucasus and again was unable to find

any stable and reliable differences between the Anatolian and Caucasian populations. On this basis, *P. caucasicola* was synonymized with *P. neocaucasicus* [Miroshnikov, 2017a: 284–285].

Thus, the previously established synonymy should be accepted: *Purpuricenus neocaucasicus* Rapuzzi et Sama, 2014 = *Purpuricenus caucasicola* Danilevsky, 2015 (see also below), as well as the following corrections need to be made:

neocaucasicus Rapuzzi & Sama, 2014b: 156 E: ST A: AB ?AR GG TR

caucasicola Danilevsky, 2015: 386.

– **Pages 280, 282.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...

baeckmanni Danilevsky, 2007c: 38 E: UK (Crimea)

...

renyvonae renyvonae Sláma, 2001: 225 E: BU CR MC TR SB”.

Remarks. This species, as noted above, should be considered a synonym for *Purpuricenus renyvonae* Sláma, 2001 [Sama, 2010; Rapuzzi, Sama, 2014; Prokopov, Turbanov, 2016; Miroshnikov, 2018c; and others].

On this basis, the following corrections need to be made:

renyvonae Sláma, 2001: 225 E: BU CR MC TR SB UK (Crimea)

baeckmanni Danilevsky, 2007c: 38.

– **Page 280.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...

dalmatinus Sturm, 1843: 353 E: ... A: ...”

Remarks. Must be: E: ... N: EG A: ... [Alfieri, 1916; Katheh-Bader, 1996; Sama, Rapuzzi, 2000; Sama et al., 2002; Sama et al., 2010b].

– **Pages 280–281.** M.A. Lazarev: “genus *Purpuricenus* Dejean, 1821: 105...

indus Semenov, 1908: 261 [RN] A: AF KA PA “Punjab”.

Remarks. Miroshnikov [2011a: 22, 63] noted that this species is known from Kashmir what was omitted in the first edition [Catalogue..., 2010: 198].

Lazarev, driven by Danilevsky, deliberately did not provide a reference to the former publication, and the results of Miroshnikov’s research is therefore presented as his own.

In addition, Lazarev simply copied the data for Pakistan (PA “Punjab”) out of the first edition [Catalogue..., 2010: 198] and did not take into account Plavilstshikov’s information [1934: 4, “Peshawar; Murree; Waziristan”], also mentioned by Miroshnikov [2011a: 63], about the wide distribution of *P. indus* in northern Pakistan. Plavilstshikov additionally confirmed this distribution in the subsequent work [1935: 190, “Kashmir, Waziristan; Murree, Peshawar, Cambelpore (now Attock. – A.M.)”].

– **Pages 280–281.** M.A. Lazarev: “genus *Purpuricenus* Dejean, 1821: 105...

kabakovi Miroshnikov & Lobanov, 1990: 15 A: AF KA PA”.

Remarks. Miroshnikov [2011a: 22, 63] noted that this species is known from Pakistan and Kashmir that were omitted in the first edition [Catalogue..., 2010: 198].

Lazarev, driven by Danilevsky, deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Pages 280–281.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...
kaehleri kaehleri Linnaeus, 1758: 393...
bilineatus Mulsant, 1839: 34...”

Remarks. Must be:

kaehleri kaehleri Linnaeus, 1758: 393...
aetnensis Bassi, 1834: 471 [Miroshnikov, 2011a: 15, 64]
bilineatus Mulsant, 1839: 34...

– **Pages 280–281.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...
kaehleri kaehleri Linnaeus, 1758: 393... E: BE FR GR IT MC PT SB SP SZ

kaehleri menetriesi Motschulsky, 1845a: 87 E: ST A: AB AR GG IN TR”

Remarks. In the first edition [Catalogue..., 2010: 198], the part of the distribution area of the former subspecies was given as follows: “E: AB... AR... GG... A: TR”, while the distribution area of the latter one – “E: AB AR GG ST A: IN TR”. Miroshnikov [2013: 17] noted that Azerbaijan, Armenia, Georgia, the Asian part of Turkey, and the Russian part of the Caucasus, where *P. kaehleri menetriesi* is distributed, were mistakenly included in the distribution area of the nominative subspecies.

Danilevsky deliberately did not give a reference to this paper, and presented the results of Miroshnikov’s research as his own.

– **Pages 280–281.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...
kaehleri litoralis Depoli, 1913: 22 [“Liburnischen Karst”]...”

Remarks. In the first edition [Catalogue..., 2010: 198], this taxon was listed as “...*litoralis* Pic, 1914c: 7”. Miroshnikov [2013: 17] made the following corrections: “...*litoralis* Depoli, 1913: 22 [“Liburnischen Karst”]”

Danilevsky deliberately did not give a reference to this paper, and presented the results of Miroshnikov’s research as his own.

– **Pages 280, 283.** M.L. Danilevsky: “genus *Purpuricenus* Dejean, 1821: 105...
wachanrui Levrat, 1858: 261 [Turquie] A: AB CY IN IQ SY TR

...

bilunatus Schaufuss, 1871: 210 [Cypern]”

Remarks. Miroshnikov [2011a: 22, 64] noted that *P. wachanrui* is known from Syria and the Asian part of Turkey that were omitted in the first edition [Catalogue..., 2010: 199] and then mentioned [Miroshnikov, 2013: 17] a synonym “*bilunatus* Schaufuss, 1871” missing in the catalogue.

Danilevsky deliberately did not give a reference to these works, and presented the results of Miroshnikov’s research as his own.

– **Page 285.** M.L. Danilevsky: “genus *Callimus* Mulsant, 1846: [5]...
angulatus angulatus Schrank, 1789: 77 (*Saperda*) E: ... A: AB AR GG...”

Remarks. Miroshnikov [2011a: 22, 64; 2011b: 559] noted that records of this taxon from Armenia and Georgia need reliable confirmation. In the Caucasus, *C. angulatus* is still known only from the Talysh Mountains, Azerbaijan, where it is very common.

Danilevsky did not provide any evidence of the distribution of the taxon in indicated countries and deliberately did not give a reference to these works.

Considering the above, Armenia (AR) and Georgia (GG) should be excluded from the distribution area of *Callimus angulatus angulatus*.

– **Pages 285–286.** M.L. Danilevsky: “genus *Callimus* Mulsant, 1846: [5]... subgenus *Procallimus* Pic, 1907a: 7...
egregius Mulsant & Rey, 1863: 146 E: ST UK A: GG LE SY TR #182”

Remarks. Miroshnikov [1991: 494; 2007: 231; 2011a: 22, 64] repeatedly noted that the records of this species from the Caucasus require reliable confirmation. Information on the distribution of *C. egregius* in Crimea is also not confirmed [Zagaikevich, 1991: 153–154; Bartenev, 2009: 199; Miroshnikov, 2011a: 22, 65].

Danilevsky deliberately did not give a reference to any of these publications and did not prove the presence of this species either in the Caucasus or in Crimea.

Based on the above, Georgia (GG), the Russian part of the Caucasus (ST) and Crimea (UK) should be excluded from the distribution area of *Callimus egregius* and presented as follows: A: LE SY TR.

– **Page 291.** M.L. Danilevsky: “genus *Acanthocinus* Dejean, 1821: 106...

aedilis Linnaeus, 1758: 392 (*Cerambyx*)...
montanus Audinet-Serville, 1835a: 33”

Remarks. Must be: *montanus* Audinet-Serville, 1835a: 33 (*Aedilis*) [Miroshnikov, 2011a: 15, 65].

– **Pages 291–292.** M.L. Danilevsky: “genus *Acanthocinus* Dejean, 1821: 106...

elegans Ganglbauer, 1884: 534 A: AB IN”

Remarks. This species was recorded from southern Dagestan (Samur River delta, 30 km south of Derbent), which confirmed the non-randomness of its record from the northern part of Azerbaijan (Nabran) [Miroshnikov, 2009a: 793]. Danilevsky ignored this paper.

Based on the above, it is necessary to include the south of the European part of Russia in the distribution area of *Acanthocinus elegans* as follows: ST (Derbent env., Dagestan).

– **Page 299.** M.L. Danilevsky: “genus *Aegomorphus* Haldeman, 1847: 45...

clavipes Schrank, 1781a: 135...
lucidus Starck, 1890: 71...”

Remarks. In the first edition [Catalogue..., 2010: 213], this name was noted as “*lucidus* Plavilstshikov, 1927a: 59”. Miroshnikov [2011a: 65–66] presented the correct data about the author and the publication date of this epithet, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Page 299.** M.L. Danilevsky: “genus *Oplisia* Mulsant, 1862: 300...

cinerea Mulsant, 1839: 152...”

Remarks. In the first edition [Catalogue..., 2010: 213], Georgia (GG) is noted for this species. Miroshnikov [2011a: 65–66] pointed out that these data need reliable confirmation. In addition, there are still no reliable

records of the species from the Russian territory of the Caucasus.

Danilevsky deliberately did not give a reference to this work and used Miroshnikov's data to exclude Georgia (GG) from the distribution area of *Oplasia cinerea*.

– **Page 300.** M.L. Danilevsky: "genus *Agapanthia* Audinet-Serville, 1835a: 35... *cardui* Linnaeus, 1767: 632..."

Remarks. In the first edition [Catalogue..., 2010: 214], this species was recorded from Armenia. Miroshnikov [2011a: 22–23, 66] indicated that these data are erroneous.

Danilevsky deliberately did not give a reference to this work and used Miroshnikov's data to exclude Armenia (AR) from the distribution area of *Agapanthia cardui*.

– **Page 300.** M.L. Danilevsky: "genus *Agapanthia* Audinet-Serville, 1835a: 35... *cardui* Linnaeus, 1767: 632..."

peragalli Mulsant, 1862: 364".

Remarks. Must be: *peragalloi*... [Miroshnikov, 2011a: 15, 66].

– **Page 300.** M.L. Danilevsky: "genus *Agapanthia* Audinet-Serville, 1835a: 35..."

suturalis Fabricius, 1787: 149 (*Saperda*) E: FR GR (Dodecanissos) IT MA PT SP N: ... A: AB AR GG CY IN IS IQ JO LE SY TR".

Remarks. Miroshnikov [2011a: 22–23, 66–67] indicated that this species was recorded from Derbent (southern Dagestan).

Danilevsky deliberately did not provide a reference to the mentioned work without commenting on this important data in any way.

Based on the above, it is necessary to include the south of the European part of Russia in the distribution area of *Agapanthia suturalis* as follows: ST (Derbent, Dagestan). It is thereby important to note that Miroshnikov [2016] showed in detail the features of the distribution of a number of Transcaucasian species that penetrate into southern Dagestan.

– **Pages 300–301.** M.L. Danilevsky: "genus *Agapanthia* Audinet-Serville, 1835a: 35... subgenus *Eoptes* Gistel, 1857a: 93..."

asphodeli asphodeli Latreille, 1804: 282..."

spencii Gyllenhal, 1817: 187".

Remarks. Must be: *spencei* Gyllenhal, 1817: 187 (*Saperda*) [Miroshnikov, 2011a: 15, 67].

– **Pages 300, 302.** M.L. Danilevsky: "genus *Agapanthia* Audinet-Serville, 1835a: 35..."

lateralis Ganglbauer, 1884: 541 E: TR

bilateralis Pic, 1927e: 1 ["Turkestan" – wrong record]".

Remarks. In the first edition [Catalogue..., 2010: 213], this name was given as "*bilateralis* Pic, 1927e: 1". Miroshnikov [2011a: 15, 68] noted that *Agapanthia lateralis bilateralis*, described from Turkestan [Pic, 1927], probably derives from a different locality. Otherwise, the synonymy in question cannot correspond to reality since *Agapanthia lateralis* is described from "Constantinopel, Kleinasien" [Ganglbauer, 1884].

Danilevsky deliberately did not give a reference to Miroshnikov's monograph, and presented the research results given in it as his own.

– **Pages 300, 303.** M.L. Danilevsky: "genus *Agapanthia* Audinet-Serville, 1835a: 35... subgenus *Homoblephara* Pesarini & Sabbadini, 2004b: 128..."

maculicornis maculicornis Gyllenhal, 1817, app.: 189 (*Saperda*) E: ... ST..."

Remarks. The distribution of this taxon is extremely poorly studied in the south of the European part of Russia (ST). Miroshnikov [2009a: 793] gave only two localities in the Russian Caucasus: the Nogai steppe in Dagestan and the vicinities of Novorossisk on the Black Sea coast of Krasnodar Region.

Danilevsky deliberately did not give a reference to this paper, which contains the most important information about the distribution of *A. maculicornis* in the Russian Caucasus, although in many other cases he considered it necessary to comment on various records of taxa from the territory of a country or region, already noted in the first edition of the catalogue.

– **Page 313.** M.L. Danilevsky: "genus *Deroplia* Dejean, 1835: 348..."

geni naviauxi Villiers, 1970: 136..."

– **Page 683.** Villiers, A. 1970: Cérambycides récoltés en Iran par MM. R. Naviaux et M. Rapilly. *L'Entomologiste* 26: 133–137.

Remarks. Must be:

geni naviauxi Villiers, 1971: 136 [Miroshnikov, 2011a: 16, 69];

Villiers, A. 1971: Cérambycides récoltés en Iran par MM. R. Naviaux et M. Rapilly. *L'Entomologiste* 26 [1970]: 133–137 [Miroshnikov, 2011a: 112].

– **Pages 337, 339, 341, 343, 349, 357.** M.L. Danilevsky: "genus *Dorcadion* Dalman, 1817a: 397..."

subgenus *Acutodorcadion*...

arietinum phenax Jakovlev, 1899c: 68...

subgenus *Cribridorcadion*...

bisignatum Jakovlev, 1899c: 66...

ciscaucasicum ciscaucasicum Jakovlev, 1899c: 59...

laeve hyrcanum Jakovlev, 1899c: 64...

sokolowi Jakovlev, 1899b: 150–151...

apicipenne Jakovlev, 1899b: 61...

jacobsoni Jakovlev, 1899a: 243..."

Remarks. In the first edition [Catalogue..., 2010], these names have been indicated since 1900. Miroshnikov [2011a: 16, 69] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov's research as his own.

– **Pages 337, 339, 342.** M.L. Danilevsky: "genus *Dorcadion* Dalman, 1817a: 397... subgenus *Cribridorcadion*..."

caspiense Breuning, 1956k: 723 A: AB".

Remarks. Must be: A: AB (Talysh).

– **Pages 337, 339, 345.** M.L. Danilevsky: "genus *Dorcadion* Dalman, 1817a: 397... subgenus *Cribridorcadion*..."

equestre equestre Laxmann, 1770: 596 (*Cerambyx*) E: CT ST UK A: ?GG (?Gagry)".

Remarks. In the first edition [Catalogue..., 2010: 246], the distribution area of this taxon was given as follows:

“E: CT ST UK”. Miroshnikov [2011b: 562–563] discussed in detail the history of the study of *D. equestre* in the Caucasus and noted that the old records from Gagra and the vicinities of Krasnodar (Novotitarovskaya) [Bogdanov-Katjkov, 1917; Dobrovolsky, 1951] need reliable confirmation.

Danilevsky deliberately did not give a required reference, and presented the results of Miroshnikov’s research as his own.

– **Pages 337, 339, 345.** M.L. Danilevsky: “genus *Dorcadion* Dalman, 1817a: 397... subgenus *Cribridorcadion*...

equestre *nogelli* Fairmaire, 1866b: 270 A: TR

nogelii Thomson, 1867a: 58 [HN] “Armenia”.

Remarks. Must be: *nogelii* J. Thomson, 1867a: 58...

In addition, in the first edition [Catalogue..., 2010], the name *exclamationis* J. Thomson, 1867: 58 is given as a synonym for *D. equestre nogelii* (page 53 is mistakenly indicated in the catalogue, which Miroshnikov [2011a: 16, 69] drew attention to). The updated catalogue does not contain this name at all. If it is really a synonym for *D. equestre nogelii* (I did not specifically study this issue), then the indicated gap should be filled.

– **Pages 337, 339, 345.** M.L. Danilevsky: “genus *Dorcadion* Dalman, 1817a: 397... subgenus *Cribridorcadion*...

glaucum *glaucum* Faldermann, 1837: 277 A: ?AB ?AR IN”.

Remarks. In the first edition [Catalogue..., 2010: 247], the distribution area of this taxon was given as follows: “E: AB A: IN”. Miroshnikov [2011a: 70] noted that the record of *D. glaucum* from Azerbaijan requires reliable confirmation, although it is described from “Transcaucasia” [Faldermann, 1837].

Danilevsky deliberately did not give a required reference, and presented the results of Miroshnikov’s research as his own.

– **Pages 337, 339, 359.** M.L. Danilevsky: “genus *Dorcadion* Dalman, 1817a: 397... subgenus *Cribridorcadion*...

weyersii Fairmaire, 1866b: 271 A: TR...

weyersii Thomson, 1867a: 51 [“Armenia”].

Remarks. Must be: *weyersii* J. Thomson, 1867a: 51...

– **Pages 376, 379.** M.L. Danilevsky: “genus *Exocentrus* Dejean, 1835: 339...

pseudopunctipennis Holzschuh, 1979a: 115 E: ?ST A: AB AR GG IN TM”.

Remarks. In the first edition [Catalogue..., 2010: 246], the distribution area of this species was given as follows: “E: AB AR GG ST A: IN TM”. Miroshnikov [2011a: 25, 82] noted that the indication of *E. pseudopunctipennis* for the south of the European part of Russia (ST) is almost certainly wrong. Its records in this region are only possible from the south of Dagestan, but they are so far not known from there.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Pages 376, 379.** M.L. Danilevsky: “genus *Exocentrus* Dejean, 1835: 339...

punctipennis *punctipennis* Mulsant & Guillebeau, 1856: 103 E: ... ST...

punctipennis *signatus* Mulsant & Rey, 1863: 163 E: ST A: AB GG TR”.

Remarks. The first edition [Catalogue..., 2010: 311] contains the following data:

“*punctipennis* Mulsant & Guillebeau, 1856: 103 E: AB ... ST... *signatus* Mulsant & Rey, 1863: 163”.

Miroshnikov [2011a: 25, 82] noted that the record of *E. punctipennis* from Azerbaijan is erroneous, and its observations in Lenkoran obviously refer to another species, *E. pseudopunctipennis* described later. The distribution of both species in the Caucasus has also been discussed before [Miroshnikov, 2004b: 136–137].

Danilevsky did not give any morphological differences between *E. punctipennis signatus* and the nominative subspecies and did not indicate the peculiarities of their distribution in the south of the European part of Russia (ST). In fact, the validity of the former taxon is questionable and requires confirmation.

In addition, Danilevsky did not provide evidence of reliable records of “*E. punctipennis signatus*” from Azerbaijan. Therefore, this country (AB) should be excluded from its distribution area.

– **Page 384.** M.L. Danilevsky: “genus *Lamia* Fabricius, 1775: 170...

textor Linnaeus, 1758: 392 (*Cerambyx*) E: ... A: AB AR... GG...”.

Remarks. Miroshnikov [2011a: 23, 70] noted that the indication of this species for Azerbaijan requires confirmation. No reliable records from there are known.

Danilevsky deliberately did not give a reference to this work and did not provide any evidence of the distribution of the species in the country in question.

Considering the above, Azerbaijan (AB) should be excluded from the distribution area of *Lamia textor* for now.

– **Pages 406–407.** M.L. Danilevsky: “genus *Monochamus* Dejean, 1821: 106...

galloprovincialis cinerascens Motschulsky, 1860b: 150...

sibiricus Pic, 1908b: 5 (*Monohammus*)

galloprovincialis pistor Germar, 1818: 242...

unifasciatus Pic, 1905a: 12 (*Monohammus*)...”.

Remarks. In the first edition [Catalogue..., 2010: 282], the names *sibiricus* and *unifasciatus* were given as follows: “*sibiricus* Pic, 1908b: 5” and “*unifasciatus* Pic, 1915f: 12 (*Monochamus*)”. Miroshnikov [2011a: 16, 70] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Page 416.** “genus *Aconodes*...

breuningii Gouverneur, 2015: 88...

submontanus Breuning, 1975d: 345 [HM]”.

Remarks. Must be: [HN].

– **Pages 423–424.** M.L. Danilevsky: “genus *Mallosia* Mulsant, 1862: 399... subgenus *Semnosia* K. Daniel, 1904a: 304...

scovitzii Faldermann, 1837: 284 (*Saperda*) A: AB AR GG ?IN TR”.

Remarks. In the first edition [Catalogue..., 2010: 294], the distribution area of this species was given as follows:

"E: AB AR GG". Miroshnikov [2011a: 23, 71] noted that the record from Georgia requires reliable confirmation, while the ZIN collection contains the material from old collections deriving from the extreme east of Anatolia (Greater Ararat Mt.).

Danilevsky deliberately did not provide a reference to this work, and presented the results of Miroshnikov's research as his own. Thereby he did not give any evidence about the records of the species from Georgia.

Considering the above, Georgia (GG) should be excluded from the distribution area of *Mallosia scovitzii*.

– **Pages 423–424.** M.L. Danilevsky: "genus *Mallosia* Mulsant, 1862: 399... subgenus *Semnosia* K. Daniel, 1904a: 304... *tristis* Reitter, 1888a: 134 A: AB IN".

Remarks. In the first edition [Catalogue..., 2010: 294], the distribution area of this species was given as follows: "E: AB A: IN TR". Miroshnikov [2011a: 23, 71] discussed the peculiarities of distribution of *M. tristis* and expressed doubts about its presence in eastern Anatolia.

Danilevsky deliberately did not provide a reference to this work, and presented the results of Miroshnikov's research as his own. In addition, the distribution of *M. tristis* in Azerbaijan is limited to the Talysh Mountains only. Therefore, the distribution area of this species must be indicated as follows: A: AB (Talysh) IN.

– **Pages 426–427.** M.L. Danilevsky: "genus *Oberea* Dejean, 1835: 351... subgenus *Amaurostoma* J. Muller, 1906: 223... *euphorbiae* Germar, 1813: 131 (*Saperda*) E: ... A: AB AR...".

Remarks. In the first edition [Catalogue..., 2010: 294], the distribution area of this species (regarding Transcaucasia) was indicated as follows: "E: AR...". Miroshnikov [2011a: 23, 72] noted that the data for Armenia require reliable confirmation but thereby presented an old record of the species from Azerbaijan (Ganja), which he had paid attention to earlier [Miroshnikov, 2004b: 137].

Danilevsky deliberately did not give a reference to these works, and Miroshnikov's data for Azerbaijan was presented as his own. At the same time, he did not provide any evidence about the occurrence of the species in Armenia.

Considering the above, Armenia (AR) should be excluded from the distribution area of *Oberea euphorbiae*.

– **Pages 426, 430–431.** M.L. Danilevsky: "genus *Oberea* Dejean, 1835: 351... *oculata* Linnaeus, 1758: 394... *quadrimaculata* Donisthorpe, 1913: 158".

Remarks. In the first edition [Catalogue..., 2010: 299], this name was given as follows: "*quadrimaculata* Donisthorpe, 1898: 302". Miroshnikov [2011a: 16, 73] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov's research as his own.

– **Pages 426, 430–431.** M.L. Danilevsky: "genus *Oberea* Dejean, 1835: 351... *oculata* Linnaeus, 1758: 394... *tomensis* Kiseleva, 1927: 131".

Remarks. In the first edition [Catalogue..., 2010: 299], this name was given as follows: "*tomensis* Kiseleva, 1926: 131". Miroshnikov [2013: 19] discussed in detail the publication history of the article by Kiseleva [1926], who described *Oberea oculata tomensis*. Danilevsky deliberately did not give a reference to Miroshnikov's work, and the publication date of this name was recorded incorrectly. Thus, the following information is correct: *tomensis* Kiseleva, 1926: 131.

– **Pages 434–435.** M.L. Danilevsky: "genus *Phytoecia* Dejean, 1835: 351... subgenus *Helladia* Fairmaire, 1864a: 176... *humeralis scapulata* Mulsant, 1851b: 194 [1852: 54] A: SY TR *mersinensis* Pic, 1900p: 140".

Remarks. In the first edition [Catalogue..., 2010: 303], these names were given as follows: "*scapulata* Mulsant, 1852: 54" and "*mersinensis* Pic, 1900x: 140 (*Helladia*)". Miroshnikov [2013: 19] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this paper, and presented the results of Miroshnikov's research as his own.

– **Pages 434–436.** M.L. Danilevsky: "genus *Phytoecia* Dejean, 1835: 351... subgenus *Helladia* Fairmaire, 1864a: 176... *praetextata praetextata* Steven, 1817: 184 (*Saperda*) E: ... ?ST (N Caucasus) TR UK (Crimea) A: AB AR ?GG TR".

Remarks. In the first edition [Catalogue..., 2010: 304], the distribution area of this taxon was given as follows: "E: AB AR BU GG RO ST UK A: TR".

Miroshnikov [2011a: 24, 76] noted that the records of *Ph. p. praetextata* from Georgia (GG) and the south of the European part of Russia (ST), bearing in mind the territory of the Russian Caucasus) require confirmation since it is not known in the Caucasus north of the vicinities of Sevan City.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov's research as his own.

– **Pages 434–436.** M.L. Danilevsky: "genus *Phytoecia* Dejean, 1835: 351... subgenus *Helladia* Fairmaire, 1864a: 176... *pretiosa* Faldermann, 1837: 298 E: ?ST (Derbent) A: AB AR GG IN ?LE TR #182 #192";

– **Page 59.** M.L. Danilevsky: "#192 *Ph. (H.) pretiosa* (Faldermann, 1837) was recorded from Georgia (Borzhomi – incorrect locality?) by Sama et al. (2007) on the basis (a single old specimen?) of the collection of Geneva Museum. One old specimen of *Ph. pretiosa* is preserved (Miroshnikov, 2011b: 24) in Zoological Institute (St. Petersburg) with the label "Derbent".

Remarks. In the first edition [Catalogue..., 2010: 304], the distribution area of this species was given as follows: "E: AB AR GG A: IN IQ SY TR". Miroshnikov [2011a: 24, 76] pointed out that reliable data on the occurrence of *Ph. pretiosa* in Georgia remain unknown, while this species is recorded from Derbent, Dagestan.

At first glance, it seems quite unexpected that against the background of Danilevsky's total disregard of many

publications, especially those authored by Miroshnikov (as demonstrated in detail in the present work), in this case he bothered himself to provide the specified reference. In fact, he did it only in order to express doubt about the Derbent record. At the same time, Danilevsky, in an absolutely strange way, does not propose a question mark for Georgia, in complete contrast to his own comments on this topic.

Miroshnikov [2016], as noted above, demonstrated in detail the peculiarities of the distribution for a number of Transcaucasian species that extend to southern Dagestan. With this in mind, the record of *Ph. pretiosa* from Derbent seems quite reliable, and I have no doubts about this.

Based on the above, the Caucasian part of the distribution area of *Phytoecia pretiosa* should be presented as follows: E: ST (Derbent, Dagestan) A: AB AR ?GG.

– **Pages 434–436.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Helladia* Fairmaire, 1864a: 176...

tectaceovittata tectaceovittata Pic, 1834c: 18 (*Musaria*) [“Ins. Kojun, Lac Urmania]...”

Remarks. In the first edition [Catalogue..., 2010: 51], the type locality of this taxon was given as follows: “Kojim, Lac Urmania”. Miroshnikov [2011a: 29] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Pages 434, 436–437.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351...

subgenus *Kalashania* Danilevsky, 2010a: 43...

erivanica Reitter, 1899: 161 A: AB AR GG IN TR...

pici Reitter, 1892: 64 A: AB AR GG IN IQ TR #181...

subgenus *Musaria* J. Thomson, 1864: 120...

kurdistanica Ganglbauer, 1884: 572 A: AB AR GG IN IQ TR”.

Remarks. Miroshnikov [2011a: 24, 76–77] noted that the records of these three species from Georgia require reliable confirmation.

Danilevsky deliberately did not give a reference to this work and did not provide any evidence about the records of the mentioned species from Georgia.

Based on the above, Georgia (GG) should be excluded from the distribution area of *Phytoecia erivanica*, *Ph. pici*, and *Ph. kurdistanica*.

– **Pages 434, 436–437.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351...

subgenus *Musaria* J. Thomson, 1864: 120...

astarte perrini Pic, 1892l: clxxxvi [1892d: 44] A: LE (sic. – A.M.)”.

Remarks. Must be: ... A: LE SY [Rejzek et al., 2001; Sama et al., 2010a, b; Cocquempot et al., 2020].

– **Pages 434, 436–437.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351...

subgenus *Musaria* J. Thomson, 1864: 120...

puncticollis puncticollis Faldermann, 1837: 291 E: ST A: AB AR GG IN IQ TR”.

Remarks. This taxon was first recorded from the south of Russia by Kasatkin [1998: 60, Dagestan, Kurush]. However, Danilevsky did not provide a reference to these

important data neither in the first edition of the catalogue nor in the second one.

Based on the above, it is necessary to include the south of the European part of Russia in the distribution area of *Phytoecia (Musaria) puncticollis puncticollis* as follows: ST (Kurush, Dagestan).

– **Pages 434, 436–437.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351...

subgenus *Musaria* J. Thomson, 1864: 120...

wachanrui Mulsant, 1851a: 127 E: GR (Rodos) ST A: IN IS JO LE SY TR”.

Remarks. In the first edition [Catalogue..., 2010: 305], the distribution area of this species was given as follows: “E: GR (Rodos) A: IN IS JO LE SY TR”. Miroshnikov [2011b: 563–564] noted that the ZMHB collection contains the male of this species from old collections with labels “Daghestan” (handwritten), “Daghestan Ca.” (printed) and did not rule out the credibility of this record.

Danilevsky deliberately did not give a reference to this paper, and presented the results of Miroshnikov’s research as his own. At the same time, it is advisable to include the discussed data in the distribution area of *Phytoecia wachanrui* as follows: E: ... ?ST (Dagestan)...

In addition, it should be noted that the southwestern boundaries of the distribution area of this species also need to be clarified. In the first edition, Sama [Catalogue..., 2010: 305] recorded *Ph. wachanrui* from Rodos, Greece. This was based probably on the specimen from the Muséum d’histoire naturelle de Genève (Switzerland), listed in Gianfranco Sama’s database (Pierpaolo Rapuzzi, personal communication of November 27, 2021). During a recent visit to the mentioned museum, Mr Rapuzzi studied this specimen at my request and kindly provided me with its images, including the labels (1 male, “Rhodus”, “*Phytoecia wachanrui* Muls. Breuning dét.”; Breuning’s collection). At the same time, Dr Denis G. Kasatkin (his personal communications of November 24 and December 9, 2021) is inclined to consider that the distribution of *Ph. wachanrui* in Rodos requires confirmation by the additional material. His opinion is based on the results of an analysis of the distribution areas of *Musaria* species inhabiting Asia Minor and adjacent territories.

– **Pages 434, 439.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Opsilia* Mulsant, 1862: 387, 431...

coerulescens coerulescens Scopoli, 1763: 49...

flavicans Mulsant, 1851a: 137 [RN]”.

Remarks. In the first edition [Catalogue..., 2010: 301], this name was given as follows: “*flavicans* Mulsant, 1862: 431 (*Opsilia*)”. Miroshnikov [2011a: 16–17, 73] made the corrections accordingly, which were then used in the updated catalogue.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research as his own.

– **Pages 434, 439.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Opsilia* Mulsant, 1862: 387, 431...

molybdaena Dalman, 1817b: 186 (*Saperda*) E: ... ST... A: ?AB ?AR... ?GG IN... TM TR...#115”.

Remarks. In the first edition [Catalogue..., 2010: 302], the distribution area of this species in Transcaucasia was given as follows: “E: AB AR... GG”. Miroshnikov [2011a: 23–24, 74] noted that in the Caucasus, the only known locality of this species is Dagestan (Novy Biryuziak), and distribution in Azerbaijan, Armenia, and Georgia is nothing more than just possible.

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov’s research presented as his own.

– **Pages 434, 439.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Opsilia* Mulsant, 1862: 387, 431...”

prasina kotaika Miroshnikov, 2009: 244 A: AR TR

prasina prasina Reitter, 1911b: 270 A: AB (Talysh) IN ?TM”.

Remarks. In the first edition of [Catalogue..., 2010: 302], there are the following data: “*Opsilia prasina* Reitter, 1911b: 270 (*Phytoecia*) E: AB AR A: IN TM”. Miroshnikov [2011a: 24, 74–75] discussed the distribution of *Ph. prasina* taking into account the subspecies *Ph. prasina kotaika* from Armenia described by him [Miroshnikov, 2009c] and expressed doubts about the presence of the nominative form in Turkmenistan.

Danilevsky deliberately did not give a reference to the former work, and presented the results of Miroshnikov’s research as his own. In addition, he did not provide any specific records of *Ph. prasina kotaika* from Turkey.

– **Pages 434, 440.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351...”

subgenus *Paracoptosia* Danilevsky, 2017f: 1137...

urartica Kasatkin, 2015b: 43 A: TR”.

Remarks. Must be: A: AB (Talysh) IN TR [Kasatkin, 2015].

– **Pages 434, 442.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Phytoecia* Dejean, 1835: 351... *croceipes* Reiche & Saulcy, 1858: 17 [RN] A: AB AR... GG... *longicollis* Costa, 1878: 27”.

Remarks. Miroshnikov [2011a: 24, 77] noted that *Ph. croceipes* was recorded from Derbent, Dagestan by König [1899], while the information about its record from Surami, Georgia [Schneider, Leder, 1879] requires confirmation. In addition, Miroshnikov [2013: 20] found out that Costa’s work describing *Phytoecia longicollis* was for the first time published in a separate edition in 1875 [Costa, 1875], and only later re-published in 1878 on the pages of the “Atti della Reale accademia delle Scienze Fische e Matematiche, Napoli” [Costa, 1878].

Danilevsky deliberately did not provide a reference to Miroshnikov’s works ignoring the important data given in them, and again presented erroneous and questionable information.

Based on the above, the Caucasian part of the distribution area of *Phytoecia croceipes* should be read as follows: E: ST (Derbent, Dagestan) A: AB AR... ?GG..., while the above name must be listed as follows: *longicollis* Costa, 1875: 27 [= 1878: 27].

– **Pages 434, 442–444.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Phytoecia* Dejean, 1835: 351...”

pustulata adulta Ganglbauer, 1884: 572 A: IN (“Astrabad”) #291

pustulata murina Marseul, 1870: 384 [“Sarepta”] E: ST A: KZ KI UK UZ

pulla Ganglbauer, 1886a: 130...

pustulata pilipennis Reitter, 1895c: 161 A: AB AR IN TR #397

pustulata pustulata Schrank, 1776: 66 (*Cerambyx*) E: ... ST... A: AB AR GG...

vexans Reitter, 1895c: 162”;

– **Page 72.** M.L. Danilevsky: “#291... *Phytoecia* (s. str.) *pustulata adulta* Ganglbauer, 1884 was accepted as a valid name for a subspecies from Iran”;

– **Page 85.** M.L. Danilevsky: “#397 According to Danilevsky (2014b: 236–237), *Phytoecia* (s. str.) *pustulata pilipennis* Reitter, 1895c is a valid name of a subspecies from Transcaucasia”.

Remarks. In the first edition [Catalogue..., 2010: 307], these taxa and names were listed as follows:

“*pustulata murina* Marseul, 1870: 384 E: AB AR A: IN

...

adulta Ganglbauer, 1884: 572

...

pilipennis Reitter, 1895c: 161

pustulata pulla Ganglbauer, 1886a: 130 E: ST A: KZ KI UZ

...

vexans Reitter, 1895c: 162

pustulata pustulata Schrank, 1776: 66 (*Cerambyx*) E: AB... AR... GG... ST UK... A: KI KZ TD TR UZ”.

Miroshnikov [2013: 20] pointed out that Sarepta (now Volgograd) is the type locality for *Phytoecia murina* [Marseul, 1870: 384: “Sarepta, Russie”], and discussed in detail the problem associated with this and some other taxa mentioned above and with various data on their distribution.

Danilevsky deliberately did not provide a reference to the former paper, and Miroshnikov’s fundamental research results were used in the updated catalogue for an allegedly original interpretation of the *Phytoecia pustulata* subspecies and their distribution areas.

– **Pages 434, 442, 444.** M.L. Danilevsky: “genus *Phytoecia* Dejean, 1835: 351... subgenus *Phytoecia* Dejean, 1835: 351...”

rufipes latior Pic, 1895b: 66 A: SY TR

rufipes rufipes Olivier, 1800: 25... E: ST... A: ES GG...

ludovici Pic, 1891e: 133 [1891m: cxxxv] [“Sarepta”]”.

Remarks. In the first edition [Catalogue..., 2010: 307–308], the following information was provided:

“*rufipes latior* Pic, 1895c: 66 A: SY TR

ludovici Pic, 1891f: 60 [HN]

rufipes rufipes Olivier, 1795: 25 (*Saperda*) E: AR... GG...

ludovici Pic, 1891m: cxxxv”.

Miroshnikov [2011a: 24, 78] made corrections to the listed synonymy and presented appropriate explanations. These important corrections were subsequently used by Danilevsky in the updated catalogue but without reference to Miroshnikov [2011a].

In the same monograph [Miroshnikov, 2011a: 24, 78], it was noted that the record of *Ph. rufipes rufipes* from Georgia requires confirmation. The distribution of this taxon in the Caucasus has practically not been studied, and I still know only its record from Armenia (Garni: personal message from Danilevsky, 2009; the relevant material is kept in his collection).

Danilevsky did not provide the required reference to Miroshnikov's work. However, it is very strange that he removed Armenia from the Caucasian part of the distribution area of taxon under discussion and left only Georgia, entirely confusing the geographical information.

– **Pages 434, 442, 444.** “genus *Phytoecia* Dejean, 1835: 351... subgenus *Phytoecia* Dejean, 1835: 351... *rufiventris* Gautier des Cottés, 1870a: 104 A: ANH ES FE FUJ GUA GUI GUX HEB HEI HEN HUB HUN JA JIA JIL JIX MG NC NMO SC SHA SHN SHX TAI ZHE **ORR**”.

abdominalis Chevrolat, 1882: 62”.

Remarks. Miroshnikov [2011a: 17, 78] discussed this synonymy in detail and convincingly showed its doubtfulness. It was thereby indicated that *Phytoecia abdominalis* is described from “Hispania, Valladolid” [Chevrolat, 1882: 62], while *Ph. rufiventris* inhabits very different territories in Asia (see above). It is possible that Chevrolat's material is actually of Asian origin rather than Spanish.

Danilevsky ignored these important data and did not provide any comments on this matter.

Considering the above, the discussed synonymy should be indicated as follows: “*abdominalis* Chevrolat, 1882: 62 [?“Hispania, Valladolid”].

– **Page 445.** M.L. Danilevsky: “genus *Pilemia* Fairmaire, 1864a: 175... subgenus *Pilemia* Fairmaire, 1864a: 175...”

annulata annulata Hampe, 1852b: 315 (*Phytoecia*) A: AB AR IN TR”.

Remarks. In the first edition [Catalogue..., 2010: 308], the distribution area of this taxon was given as follows: “E: AB A: IN TR”. Miroshnikov [2011a: 25, 79] noted that the ZMMU collection contains the specimen from the Kagyzman area (the extreme east of Anatolia). Therefore, the record of *P. annulata* from the western areas of Armenia [Plavilstshikov, 1948], almost without a doubt, corresponds to reality.

Danilevsky deliberately did not give the required reference, and presented the results of Miroshnikov's research as his own.

– **Pages 445–446.** M.L. Danilevsky: “genus *Pilemia* Fairmaire, 1864a: 175...”

subgenus *Pilemia* Fairmaire, 1864a: 175...

angusterufonotata Pic, 1952a: 2 E: AL GR

inarmata Holzschuh, 1984b: 168

...

annulata wawerkana Reitter, 1905b: 240...

angorensis Pic, 1952a: 2

breverufonotata Pic, 1952a: 2 A: TR

maculifera Holzschuh, 1984b: 170

griseomaculata Pic, 1891h: 102...

laterufonotata Pic, 1952a: 2

...

samii Özdikmen & Turgut, 2010: 97 A: TR

serriventris Holzschuh, 1984b: 169 E: BU

smatanai Holzschuh, 2003: 240 A: TR

tigrina Mulsant, 1851a: 134...

anchusae Fuss, 1852b: 138

vagecarinata Pic, 1952a: 3 A: SY TR

subgenus *Pseudopilemia* Kasatkin, 2018...

buglanica D. Marklund & S. Marklund, 2014: 276 A: TR

evae D. Marklund & S. Marklund, 2014: 274 A: TR

hirsutula hirsutula Frolich, 1793: 141...

...

tournieri Pic, 1952a: 2

hirsutula homoiesthes Ganglbauer, 1888d: 197...

konyaensis Danilevsky, 2010e: 20 A: TR

kruszelnickii Szczepański & Karpiński, 2017: 142 E: GR

moreana Breuning, 1943b: 102...

hladilorum Holzschuh, 2006a: 274

holtzi Pic, 1952a: 3”

Remarks. Must be [Fuss, 1852: 138; Ganglbauer, 1888: 197; Pic, 1952: 2–3; Holzschuh, 1984: 168–170; 2003: 240; 2006: 274; Danilevsky, 2010b: 20; Özdikmen, Turgut, 2010: 97; D. Marklund, S. Marklund, 2014: 274, 276; Szczepański, Karpiński, 2017: 142; Kasatkin, 2018: 160; Miroshnikov, 2011a: 17, 81–82]:

subgenus *Pilemia* Fairmaire, 1864a: 175...

angusterufonotata Pic, 1952a: 2 (*Phytoecia*) E: AL GR

inarmata Holzschuh, 1984b: 168 (*Phytoecia*)

...

annulata wawerkana Reitter, 1905b: 240...

angorensis Pic, 1952a: 2 (*Phytoecia*)

breverufonotata Pic, 1952a: 2 (*Phytoecia*) A: TR

maculifera Holzschuh, 1984b: 170 (*Phytoecia*)

griseomaculata Pic, 1891h: 102...

laterufonotata Pic, 1952a: 2 (*Phytoecia*)

...

samii Özdikmen & Turgut, 2010: 97 (*Phytoecia*) A: TR

serriventris Holzschuh, 1984b: 169 (*Phytoecia*) E: BU

smatanai Holzschuh, 2003: 240 (*Phytoecia*) A: TR

tigrina Mulsant, 1851a: 134...

anchusae Fuss, 1852b: 138 (*Phytoecia*)

vagecarinata Pic, 1952a: 3 (*Phytoecia*) A: SY TR

subgenus *Pseudopilemia* Kasatkin, 2018...

evae D. Marklund & S. Marklund, 2014: 274 (*Phytoecia*)

A: TR

hirsutula hirsutula Frolich, 1793: 141...

...

buglanica D. Marklund & S. Marklund, 2014: 276 (*Phytoecia*)

...

tournieri Pic, 1952a: 2 (*Phytoecia*)

hirsutula homoiesthes Ganglbauer, 1888d: 197 (*Phytoecia*)...

konyaensis Danilevsky, 2010e: 20 (*Phytoecia*) A: TR

kruszelnickii Szczepański & Karpiński, 2017: 142 (*Phytoecia*) E: GR

moreana Breuning, 1943b: 102...

hladilorum Holzschuh, 2006a: 274 (*Phytoecia*)

holtzi Pic, 1952a: 3 (*Phytoecia*)

As can be seen from the above, Danilevsky often inattentively uses the original publications of many authors [Fuss, 1852; Pic, 1952; Holzschuh, 1984, 2003, 2006; Özdikmen, Turgut, 2010; D. Marklund, S. Marklund, 2014; Szczepański, Karpiński, 2017], and even his own [Danilevsky, 2010b].

– **Pages 446–447.** M.L. Danilevsky: “genus *Pogonocherus* Dejean, 1821: 107... subgenus *Pogonocherus* Dejean, 1821: 107...”

hispidulus Piller & Mitterpacher, 1783: 35... E: ... A: AB AR GG KZ (north-east) TR".

Remarks. In the first edition [Catalogue..., 2010: 312], the Asian part of the distribution area of this species was given as follows: "E: AB... AR... GG... A: TR". Miroshnikov [2008: 325] highlighted the record of *P. hispidulus* from the north-east of Kazakhstan for the first time and drew attention to it in his subsequent monograph [Miroshnikov, 2011a: 25, 83].

Danilevsky deliberately did not provide a reference to these works and presented Miroshnikov's discovery of *P. hispidulus* in the north-east of Kazakhstan as his own.

– **Pages 446–447.** M.L. Danilevsky: "genus *Pogonocherus* Dejean, 1821: 107... subgenus *Pityphilus* Mulsant, 1862: 302... *ressli* Holzschuh, 1977a: 132 A: AB IN".

Remarks. Must be: A: AB (Talysh) IN [Miroshnikov, 2008].

– **Pages 446–448.** M.L. Danilevsky: "genus *Pogonocherus* Dejean, 1821: 107... subgenus *Pogonocherus* Dejean, 1821: 107... *sieversii* Ganglbauer, 1887b: 139 (*Pogonochaerus*) ?E: ?UK A: AR GG TR

caucasicus Ganglbauer, 1891: 132 (*Pogonochaerus*)".

Remarks. In the first edition [Catalogue..., 2010: 313], this species was given as follows: "*sieversii* Ganglbauer, 1887b: 139 E: AR GG UK A: TR". Miroshnikov [2008: 326; 2011a: 25, 83] discussed in detail the distribution of *P. sieversii* and noted that its records from Ukraine are very dubious and most likely are associated with incorrect identification of the corresponding specimens from Crimea, apparently belonging to *P. perroudi*. In addition, it is noted that the species in question was described as *Pogonochaerus sieversii*, and *Pogonochaerus caucasicus* Ganglbauer, 1891 is a homonym.

Danilevsky deliberately did not give a reference to these works, and presented the results of Miroshnikov's research (except for the data on homonymy) as his own.

Considering the above, the following correction needs to be made: *caucasicus* Ganglbauer, 1891: 132 (*Pogonochaerus*) [HN].

– **Pages 446–448.** M.L. Danilevsky: "genus *Pogonocherus* Dejean, 1821: 107...

Remarks. In the first edition [Catalogue..., 2010: 312–313], some names were given as follows: "*ovalis* Gyllenhal, 1827: 65" (syn. pro *P. decoratus*); "*setifer* O. F. Müller, 1776: 92" (syn. pro *P. fasciculatus fasciculatus*); "*ovalis* Gmelin, 1790: 1863 (*Lamia*)" (syn. pro *P. ovatus*); "*vaulogeri* Pic, 1927e: 1" (syn. pro *P. perroudi perroudi*).

Miroshnikov [2011a: 17–18, 82–83] made the following corrections: *ovalis* Gyllenhal, 1827: 65 (*Lamia*); *setifer* O. F. Müller, 1776: 92 (*Cerambyx*); *ovalis* Gmelin, 1790: 1863 (*Cerambyx*); *vaulogeri* Pic, 1927e: 1 (*Pogonochaerus*).

Danilevsky deliberately did not give a reference to these works, and presented some results of Miroshnikov's research as his own. In addition, in the updated catalogue the latter name has not been corrected, and it should be listed as *vaulogeri* Pic, 1927e: 1 (*Pogonochaerus*).

– **Pages 473–474.** M.L. Danilevsky: "genus *Saperda* Fabricius, 1775: 184... subgenus *Lopezcolonia* Alonso-Zarazaga, 1998: 131...

octopunctata Scopoli, 1772: 101...

sempunctata Reitter, 1909: 57..."

Remarks. Miroshnikov [2011a: 18, 86] indicated the latter name as a synonym for *S. octopunctata*, which was omitted in the first edition [Catalogue..., 2010: 330].

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov's research as his own.

– **Pages 473–475.** M.L. Danilevsky: "genus *Saperda* Fabricius, 1775: 184..."

Remarks. In the first edition [Catalogue..., 2010: 330], some names were given as follows: "*griseus* Mulsant, 1839: 184" (syn. pro *S. carcharias*); "*deudecimpunctata* Brahm, 1790: 176 (*Leptura*)" (syn. pro *S. perforata*); "*betulina* Geoffroy, 1785: 78" (syn. pro *S. populnea*).

Miroshnikov [2011a: 18, 86] made the following corrections: *griseus* Mulsant, 1839: 184 (*Anaerea*); *deudecimpunctata* Brahm, 1790: 176 (*Cerambyx*); *betulina* Geoffroy, 1785: 78 (*Leptura*).

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov's research as his own.

– **Page 476.** M.L. Danilevsky: "genus *Stenostola* Dejean, 1835: 350..."

ferrea maculipennis Holzschuh, 1982b: 155 E: ST UK".

Remarks. In the first edition [Catalogue..., 2010: 331], the distribution area of this taxon was given as follows: "E: ST". Miroshnikov [2011a: 25, 86] noted that the ZMMU collection contains the material from Crimea (1 female from Yalta and 1 female from Simferopol).

Danilevsky deliberately did not give a reference to this work, and presented the results of Miroshnikov's research as his own. At the same time, the distribution area of *Stenostola ferrea maculipennis* should be indicated as follows: E: ST (NW & N Caucasus) UK (Crimea).

– **Page 477.** M.L. Danilevsky: "genus *Tetrops* Kirby, 1826 (in Kirby & Spence 1826: 498)..."

Remarks. Judging from the catalogue, certain rules for providing the author(s) of a taxon in a case where the author(s) of a taxon does not correspond to the author(s) of the publication in which this taxon is established (described) are followed but not strictly defined. Based on this, the following data should be here: genus *Tetrops* Kirby, 1826: 498...

Otherwise, all taxa originally described in a similar way should be listed in a similar way as for *Tetrops*, indicating the author(s) of the taxon and the author(s) of the corresponding publication, for example, *Clytus schneideri schneideri* Kiesenwetter (in Schneider & Leder, 1879...) (page 235) etc.

– **Pages 477–478.** M.L. Danilevsky: "genus *Tetrops* Kirby, 1826..."

starkii aquilus Danilevsky, 2012m: 937, 939 E: ST (Krasnodar env.)".

Remarks. This taxon is known in the Northwest Caucasus not only from the vicinities of Krasnodar but also from some other localities of the region. Therefore, it must be: E: ST (NW Caucasus).

Conclusion

Based on the above, it is quite obvious that Danilevsky's data published in the second edition of the catalogue is neither higher in quality nor more reliable than those contained in the previous edition. The updated catalogue has not only inherited a huge number of mistakes and oversights from the first edition but also added many other erroneous and highly dubious data by this author.

Moreover, some very unpleasant aspects appear so abundant in the second edition that they leave a profoundly negative impression of the entire catalogue.

First of all, Danilevsky's pure plagiarism cannot remain unnoticed. He, grossly disregarding scientific ethics altogether, widely used various original data of some authors, in particular those by Miroshnikov, presenting them as the results of his own research and giving no pertinent references. In a too large number of cases, Danilevsky did not bother to refer to the original works containing the useful data completely absent from or mistakenly contained in the first edition. The cases listed above are only a fraction of similar examples, which are simply too numerous to quote them all. In particular, very numerous corrected names, data on the authors of taxa, pagination of the original descriptions, actual publication release dates, various explanations, many corrections to bibliography, and other data were almost fully omitted. Taken as a whole, these data are very extensive and belong mainly to Miroshnikov, but Danilevsky used the fruits of this labour with neither any hesitation nor comment.

Danilevsky's falsifications of various data, only some examples of which have been vividly highlighted above, leave an indelible mark on the quality of the catalogue, while his obvious bibliographic manipulations only reinforce the generally negative impression of this edition.

Danilevsky has utterly disregarded a wealth of valid and reliable data by various investigators he is well aware of, including Kasatkin, Miroshnikov, Sama, and others. Since the omitted information shed light on many controversial and poorly studied issues, this negatively impacts the reference data contained in the catalogue. This is largely due to Danilevsky's stubborn unwillingness to acknowledge the numerous obvious mistakes he had made in the first edition and repeated in the updated catalogue without any comments. It is thereby noteworthy that many of the statements expressed by that author in his relatively recent monograph [Danilevsky, 2015a], in one way or another contradict the data of the catalogue, especially as regards distributions. Some examples above demonstrate this in full. At the same time, the 2015 monograph by Danilevsky and the updated catalogue, where Danilevsky reused in every possible way a huge amount of his erroneous and dubious data, are both worth so many reproaches that my earlier evaluation still holds [Miroshnikov, 2016].

Danilevsky's editorial activities, with a strong negative connotation described above, has, in turn, had a great impact on the content of the catalogue.

Regretfully, my expectations to see a corrected and updated version of the catalogue with reliable information devoid of previous mistakes have not been met. Once again,

sections authored by Danilevsky raised a lot of criticism in most if not all aspects, while I found his editorial style most unappealing. Only the conscientious and fruitful work of almost all other authors has made their parts of the catalogue as a wholly successful and commendable.

I will consider with satisfaction my mission justified if the present evaluation, at least to a certain extent, will allow the reader to be able to correctly navigate the catalogue and soberly judge its merits and demerits.

Acknowledgements

I am very grateful to Svetlana V. Andreeva (ZIN), Maxwell V.L. Barclay (BMNH), Alexey A. Gusakov (ZMMU) for the opportunity to study the museum material, to Karl Adlbauer (Graz, Austria), Denis G. Kasatkin (Rostov-on-Don, Russia), Sergei A. Kurbatov (Moscow, Russia), Pierpaolo Rapuzzi (Prepotto, Italy), Andreas Weigel (Wernburg, Germany) for having helpfully shared some valuable information. I give special thanks to Dmitry Telnov (Natural History Museum, London, United Kingdom), Alexander S. Konstantinov (National Museum of Natural History, Smithsonian Institution, Washington D.C., USA), Sergejs Berdnikovs (Northwestern University, Chicago, USA) who helped greatly with the final English editing of much of the draft. I am deeply indebted to Maxim V. Nabozhenko (Precaspian Institute of Biological Resources of the Daghestan Federal Research Centre of the Russian Academy of Sciences, Makhachkala, Russia), Alexey Yu. Solodovnikov (Natural History Museum of Denmark, University of Copenhagen, Denmark), again to Dmitry Telnov, and to anonymous reviewers who have kindly provided useful suggestions when reading the manuscript, to Kirill V. Makarov (Moscow Pedagogical State University, Moscow, Russia) for having rendered his great help in the preparation of photographs. Last but not least, I am most grateful to my wife Tatiana P. Miroshnikova who helped a lot in the preparation of the illustrations for publication.

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Received / Поступила: 3.11.2021

Accepted / Принята: 19.12.2021

Published online / Опубликовано онлайн: 30.12.2021