

Call it *schneiderii*, but document which identification key you used

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Preface

Boudot (2022) criticised in unusually harsh language with reference to 'Resolution 74 (26) on the Right of Reply of the Committee of Ministers of the Council of Europe (<https://rm.coe.int/CoERMPublicCommonSearchServices/DisplayDCTMContent?documentId=09000016805048e1>)' in an e-mail with attached word document dated 31st January 2022, local time Zerf: 15.45h a small and rather insignificant publication written by Nataly Snegovaya and myself, and demanded that his counter statement be published. In my function as the responsible editor of IDF Report I granted this wish. It has to be noted that all we had done was insisting on retaining the record of Barteneff (1912a) which we are considering a proven record of *Gomphus vulgatissimus*.

Motive for the publication of Schorr & Snegovaya (2022)

In a pluralistic, knowledge-based society, the discussion of scientific publications is normality. In this context, we exercised our right, covered by tradition and good practice, to objectively criticise an issue and, importantly, to make constructive suggestions on how to deal in the near future with a taxonomic problem that even Selys-Longchamps could not solve.

- We were of the opinion that, according to our knowledge, *Gomphus vulgatissimus* should still be considered part of the dragonfly fauna of Azerbaijan and consequently should also be included in a distribution map.
- We consider the currently used methods for the identification of the taxa *vulgatissimus* and *schneiderii* as not fully developed, and document on the basis of SEM images that, according to the current state of knowledge, *G. vulgatissimus* occurs in Azerbaijan.
- It will not be further explained in detail here that a critical comparison of existing illustrations does not substantiate the alleged differences between *vulgatissimus* and *schneiderii*, just as the molecular genetic study by Dumont et al. (2021) does not provide a solution to the taxonomic problem. However, IDF has in the meantime made funds available to facilitate an evidence-based discussion - at least with regard to a sub-area of the two taxa.

A conflict is apparently **triggered** by the following paragraph, the meaning of which is assessed differently by Boudot (2022) than by Schorr & Snegovaya (2022), namely as a "discussion", whereas I consider it to be an opinion that has been solidified into a statement (my phrasing: "without any discussion"): "Similarly, two records of *Gomphus vulgatissimus* made by Skvortsov & Snegovaya (2014) and Snegovaya (2020) have been rejected as, due to strong variation in both taxa, this species is difficult to separate from *G. schneiderii* on the basis of colour pattern (De Knijf et al. 2013) and intermediate individuals are known where the two taxa meet. The authors themselves have expressed some doubts ("most probably belong to true *G. vulgatissimus*"), and as these records fall within

the range of *G. schneiderii* we regard the records as doubtful and have thus omitted *G. vulgatissimus* from this atlas. Whether the records represent *G. schneiderii* or intermediate individuals is unknown." (Boudot et al. 2021: 10)

When publishing our paper, we didn't write about *G. schneiderii* in general, but two special issues: The record of Bartenef and the record published in Schorr & Snegovaya (2022). This may have caused confusion.

One can disagree about the semantic meaning of some of the above formulations. However, despite all doubts and taxonomic problems, in the case of the situation in Azerbaijan Bartenef (1912b) has provided illustrations and Nataly Snegovaya has published verifiable SEM images of an individual that is also considered by other odonatologists to be *G. vulgatissimus*. Also, this decision to consider *G. vulgatissimus* as a faunal component of Azerbaijan is based solely on structural features, and explicitly leaves "colour pattern" out of consideration. However, this at least clearly and verifiably documents something that can be falsified by other odonatologists. And one does not have to choose a spatially indeterminate formulation - "these records fall within the range of *G. schneiderii* we regard the records as doubtful and have thus omitted *G. vulgatissimus* from this atlas". However, the following is only my opinion (see below): I find no evidence-based analysis of the range of *Gomphus schneiderii* anywhere, and dare to ask whether anyone can really determine beyond doubt what is behind the taxon "*Gomphus schneiderii*".

The pitfalls of semantics - what is 'discussion', what is 'opinion', what is 'statement'?

A **discussion** in the scientific sense also allows for other opinions. It places one's own results at the centre of the considerations, explains why one's own view - according to current knowledge - is correct despite the opinion of other published views. However, many authors explicitly allow other conclusions to be reached, taking into account broader or deeper studies to be done in future.

This is contrasted with an **opinion**. An opinion or position on an issue can be a contribution so that others also communicate their opinion. However, it can also be a position of authors that is not intended to trigger an exchange of opinions, but is published without further or more in-depth discussion. It thus becomes a **statement** or can even become a dogma that excludes any discussion.

I consider the paragraph on page 10 by Boudot et al. (2021) quoted above - regardless of its psychological intention - not to be a discussion, but an opinion, since the state of knowledge, especially the increase in new knowledge, has not changed significantly. The authors do not include any new facts for their decision not to include *G. vulgatissimus* in the West Asia Atlas, but have only changed their assessment of an almost unchanged data situation - only Snegovaya has contributed new material -, and consequently held a different opinion in 2021 than in 2015 (Boudot & Kalkman 2015). The interpretation of Snegovaya's published material was accepted by us, and we even added new material of "*G. schneiderii*" following the current insight of Boudot et al. (2021) in the general distribution of this taxon (see our published map).

In 2015, *G. vulgatissimus* was still considered for Azerbaijan, but no longer in 2021 (Boudot & Kalkmann 2015, Boudot et al. 2021). The crucial question for me was: Why was *G. vulgatissimus* published in 2015, but now no longer considered to be *G. vulgatissimus*?

Furthermore, it is crucial that the assumption of Schröter et al. (2015: 327) that *G. vulgatissimus* could also occur as far as Azerbaijan has not yet been modified: „Given correct determination, also single migrants of *Gomphus vulgatissimus* may have been involved here and it appears to be possible that at least the contact zone of both species might run through the eastern part of the Transcaucasian depression.“

Thus, Boudot et al. (2021) interpreted old data as being "*schneiderii*" without any real increase in knowledge. This was done on the basis of pure assumption and without any new evidence, solely on the basis of their self-confessed competence.

I do not question this competence - let this be explicitly and unequivocally stated here. Together with Snegovaya, however, I was of the opinion, which we put up for discussion, that it is still too early to conclusively remove a species or taxon '*vulgatissimus*' from consideration, because there is something final or conclusive about an atlas written by such competent and leading odonatologists.

Given the totally unsatisfactory taxonomic state of knowledge of the species complex '*vulgatissimus/schneiderii*' (cf. Boudot & Kalkman 2015, Schneider & Ikemeyer 2019, and especially Dumont et al. 2021), all we have done was to suggest to be a bit more cautious with the two taxa, to collect new material ('search area', our Fig. 2) and to always document precisely how an author arrived at an identification result, if it is to be published. This will make it easier for future researchers to assess why a specimen was called '*vulgatissimus*' or '*schneiderii*'.

Can a taxonomic classification based on its range alone be convincing?

Boudot & Kalkman (2015: 192) write the following about *G. schneiderii*: "This species is very similar to *Gomphus vulgatissimus* and is sometimes considered as a subspecies of the latter. In the Balkan Peninsula, where the ranges of the species meet, **there is a broad zone where intermediates are found and populations cannot be ascribed to either of the species with certainty.** The status of *G. schneiderii* as species or subspecies is still under debate and the matter can only be solved by a thorough investigation of material from a wide range of localities from south-west Europe and south-west Asia, preferably using both morphological and molecular methods."

Given such considerable difficulties in correctly identifying the two taxa, it is surprising that it was possible to produce distribution maps that give the impression that a correct assignment to a taxon was possible even in the 'overlap zones'. In the case of Boudot, who (co)authored the *schneiderii* and *vulgatissimus* chapters, there is no doubt that this unambiguous assignment can be achieved within the framework of field studies. However, since the atlas mainly uses external data, it cannot be assumed that correct identifications were made in all cases.

I see this map of world distribution in Boudot & Kalkman (2015: 194) but I didn't find a source on what taxonomic basis the map was drawn. How was *G. vulgatissimus* distinguished from *schneiderii*? Blue eyes, appendices, other structures? I see the detailed discussion in De Knijf et al. (2013) of the problems in distinguishing the two taxa with certainty, but I do not find a solution to the problems there either. I hope I didn't miss the relevant paragraph in the publications I here consider.

If you look at the illustration of *G. schneiderii* in Schneider & Ikemeyer (2019:139), for example, you will inevitably ask yourself what the difference is between *G. schneiderii* and *G. vulgatissimus* if you exclude possible differences in colouration and focus exclusively on structural characters. And even if one considers these minor differences as valid to separate two taxa, who has done this in the field?

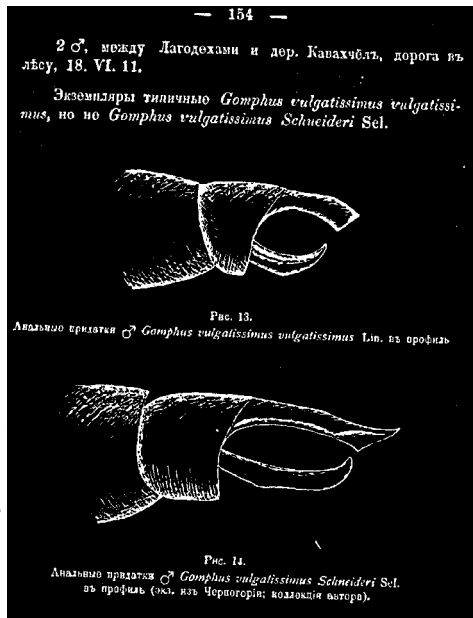
Why is it correct to assign the species described by Bartenef (1912a) to *Gomphus vulgatissimus*?

Every species identification depends on which identification keys and knowledge were available at a given time. This problem is easy to solve for the current conflict, because only two time periods have to be considered: (a) that of Bartenef around 1912 and (b) that of Snegovaya around 2012.

Regarding a): The basis on which Bartenef (1912a) determined the individual he identified as *Gomphus vulgatissimus vulgatissimus* can easily be checked in this case using the drawing in Bartenef (1912b) (Fig. 1).

These drawings of Bartenef may not be of the very best quality and for *G. vulgatissimus schneiderii* one can assume that on the way from Montenegro to Bartenef the specimens were compressed or that a lateral illumination on the drawing table changed the proportions. Unfortunately, this must remain pure speculation. However, the fundamental difference between the two individuals is recognisable (cf. lock-and-key concept, see below). And, crucially, these differences will not be depicted differently in 2020 (Fig. 2).

Abb. 1: „Figures 13 and 14 show the difference in the shape of the upper anal appendages of both subspecies of *G. vulgatissimus*. As can be seen in the figures, the upper anal appendages also differ in relative length. So in *G. v. vulgatissimus* their length is equal to the length of the 10th segment, and in *G. v. schneiderii* they are almost 1/3 longer than the 10th segment.“ (Translation Vladimir Onishko).



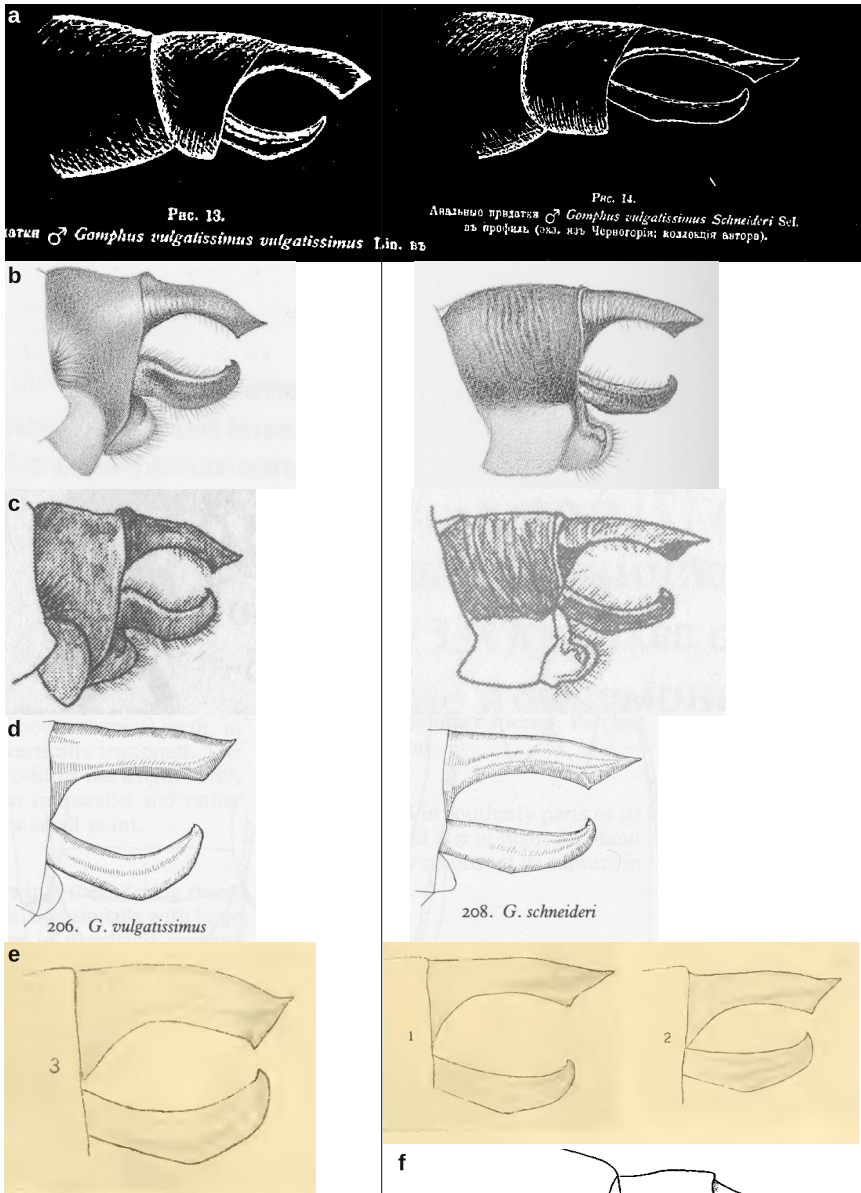


Fig. 2: Comparison of anal appendices. Left column: *Gomphus vulgatissimus*. Right column: *G. schneiderii*. a) Bartenef (1912b); b) Dijkstra et al 2020; c) Skvortsov (2010); e) Askew (1988); e) Morton (1915); f) Dumont (1991); *G. vulgatissimus schneiderii*

In the case of *G. vulgatissimus*, the upper appendages are more downward curved (Exception: Askew 1988), while the lower appendages are more upward directed. In the case of *G. (vulgatissimus) schneiderii*, both appendages appear less curved with more straight-stretched parts (see also Seidenbusch 1997a).

Considering only these two alternatives, it is clear that the specimen indicated by Ares is *G. vulgatissimus* or 'forma', 'race' or 'subspecies' *vulgatissimus*. The colourings referred to by Boudot (2022), to which Bartenev is said to refer, are not found in the two relevant publications of 1912a and 1912b, but are in the one by Bartenev (1912c) on the dragonflies of Montenegro, which had been given to him for identification.

Boudot (2022) writes: "Prior to Morton's paper on the Odonata of Constantinople (1915) these two *Gomphus* were mostly separated by colour characters of strong variability as the structural characters we can use now, which originate from Selys (1850, 1857, 1887) had never been published with drawings, making likely that Bartenev could not use them reliably. Three years after Bartenev's papers, Morton (1915) published drawings of the male abdominal appendages of one *G. vulgatissimus* and two *G. schneiderii* to allow a reliable identification of these two taxa. However the two *G. schneiderii* specimens were so different that one is equally different of the other than it is different from the drawing of *G. vulgatissimus*. Natural variability of each taxon was not accounted for by this paper, raising the issue of how representative actually they are and making their use unreliable."

Now it is not correct that it was Morton (1915) who first illustrated the two taxa in a comparative manner that allowed a reliable differentiation or determination. It was Bartenev (1912b) (Fig. 1).

Considering the high variability of the species or of the structural and/or colouration characters - especially of *G. schneiderii* - even three individuals sketched by Morton should not be sufficient to bring about a decision on the taxonomic status of the taxon.

It may be that Bartenev first identified his Montenegro specimens on the basis of colouration differences and found that both species or subspecies were present in virtually the same locality. On the basis of colouration (Selys) and structure (Bartenev), however, he succeeded in first identifying and then separating the two taxa. Since he did not find any significant differences in colouration ("The colouration of the head and legs is almost the same as that of the latter. The underside of the chest behind the legs is yellow."), he drew the appendices in Bartenev (1912b). With this he was exactly as far along as we still are 100 years later. If he had had the identification key by Dijkstra et al. (2020), he would also have ended up with *G. vulgatissimus*, because the colouration characteristics described in the latest available field guide do not help either: in the table on page 189, no distinguishing characteristics are given apart from the different geographical area of the occurrences. And also the textual descriptions given in the main text remain vague and advise caution, but do not really help to separate the two taxa. (for more see below)

Against this background, one cannot come to any other conclusion than that Bartenev had a taxon of Ares in front of him that is also *Gomphus vulgatissimus* according to today's knowledge. He compared this taxon with the two taxa from Montenegro available to him, as all relevant publications by him in this context date from the same year.

Why is it correct to assign the individual collected by Svorstov & Snegovaya 2012 to *Gomphus vulgatissimus*?

The unpleasant conflict that led to this publication is also due to the fact that especially in a difficult taxonomic situation it would be appropriate to justify on which species concept and on which basis a determination is made.

Schorr & Snegovaya (2022) have reduced the species concept to one aspect, namely morphological structures, which are of great importance in separating species and preventing interspecific mating (e.g. Gorb 1998). This results logically from the problem to be dealt with, whether a taxon occurs or not. This is primarily only a question of the unambiguous determination of a given individual.

Arnqvist (1997) discusses the importance and change of morphological structures in the course of evolutionary processes for speciation processes: "Rapid evolution of genitalia is one of the most general patterns of morphological diversification in animals. Despite its generality, the causes of this evolutionary trend remain obscure. Several alternative hypotheses have been suggested to account for the evolution of genitalia (notably the lock-and-key, pleiotropism, and sexual selection hypotheses)." Further factors that play a role in speciation can be read compactly and didactically in Wildermuth (2008: Chapter 1.2).

Even though I am aware of the problematic nature of the "lock-and-key" explanatory approach, it must be stated that this approach is still of central, if not crucial, importance in all identification books for dragonflies. For ultimately, every identification result must be checked against the genital structure, i.e. as a rule the secondary copulatory apparatus, of individuals at least in Anisoptera; only then is a reliable identification result available.

This 'lock-and-key' concept gives field odonatologists the chance to make a correct identification, if the authors of field guides have succeeded in defining criteria for lock and key and figuring them in such a way that they can be applied.

It should hardly be disputed that for the determination of the European dragonfly fauna the work of Dijkstra & Lewington (2006) and in a second edition Dijkstra, Schröter & Lewington (2020) is the gold standard, and complementary to this Skvortsov (2010) can be used going more to southwestern Asia. The book of Skvortsov (2010) is of some interest as he illustrates the secondary genitalia of both taxa involved in this paper.

Comparing the appendices shown by Dijkstra et al. (2020) with the illustration by Bartenef (see Fig. 1), there can be no doubt that Bartenef correctly identified the specimens available to him according to today's valid and best available standard work.

As Suhling & Müller state in Dijkstra et al. (2020: 188): "The separation of three species (*G. schneiderii*, [...]) from their more widespread counterparts (*G. vulgatissimus*, [...]) is still somewhat questionable, although the species in each pair are geographically (largely) segregated. A simple table, based on markings of both sexes and ranges, is provided, but for positive identification careful comparison is required, preferably in the hand. The male's appendages and female's vulvar scales may then be useful, but are fairly uniform.] The shape of the male's posterior hamules is underappreciated as a character, but the hamules are almost as easily examined with a hand lens as the appendages, and may be more informative."

The German translation says: "Ein wichtiges und immer eindeutiges Bestimmungsmerkmal ist dagegen die Form der posterioren Hamuli des Männchens.", which is explicitly referred to in a drawing on page 191 in the English edition of the book. What is peculiar, however, is that the secondary sexual apparatus with the hamuli is not illustrated for *G. schneiderii*. This leaves the user perplexed. And this leaves only the appendages of the two species for the normal user to distinguish, as it is not only in the context of the discussion that led to this article that the value of colouration differences, even of the blue eyes in *G. schneiderii*, is considered diagnostically insufficient. These anal appendages are illustrated clearly positioned, and this is what a user of an identification book expects: he does not read long texts, but looks at the illustrations: "A picture says more than a thousand words", is a German proverb).

The specimen of Nataly Snegovaya

In Dijkstra et al. (2020), the appendices superiores are not illustrated dorsally, in contrast to other current identification works. In Schneider & Ilkemyer (2019: 139) they are figured to determine different gomphids. And it is interesting what Suhling & Müller (1996: 173) provide in their book in terms of identification. However, it must be noted here that these may have been taken from d'Augilar & Dommange (1998). Whether the differences shown are actually valid in the entire range of the taxon "*schneiderii*" is beyond my knowledge. However, it is clear that the dorsally depicted shape of the appendices superiores is completely consistent with Fig. 1c in Schorr & Snegovaya (2022), and that is true for Asahina's (1986) figure, too, and there is no correspondence with the shape given by these authors for *G. schneiderii* (Fig. 3a, c). But it is not true, compared with Dumont (1991). However, in Dumont (1991) (Fig. 3d) the "superior appendices" seem to be longer than in *G. vulgatissimus*. Probably the shape of the appendices superiores - seen dorsally - is not a good character to distinguish the taxa.

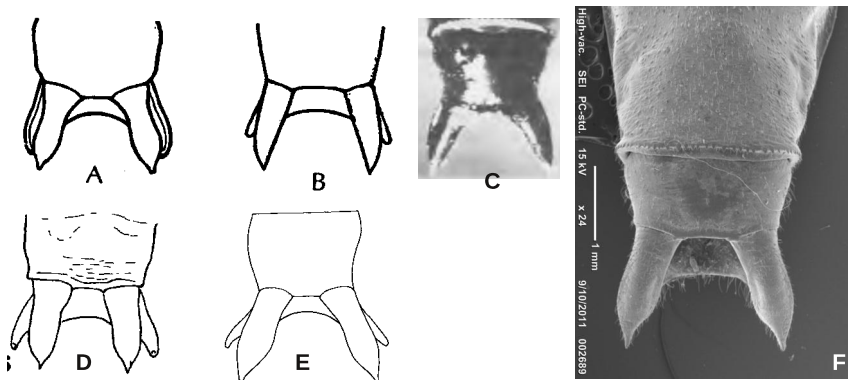


Fig. 3: Figures taken from identification key in Müller & Suhling (1996): Dorsal view of appendices superiores. A: *Gomphus vulgatissimus*, B: *Gomphus schneiderii* (above), C: Seidenbusch (1997b): *G. schneiderii*; D: *G. vulgatissimus* (Germany) (Asahina 1985) E: *G. vulgatissimus schneiderii* (Dumont 1991) and F: compared with the specimen published by Schorr & Snegovaya (2022) (right, SEM).

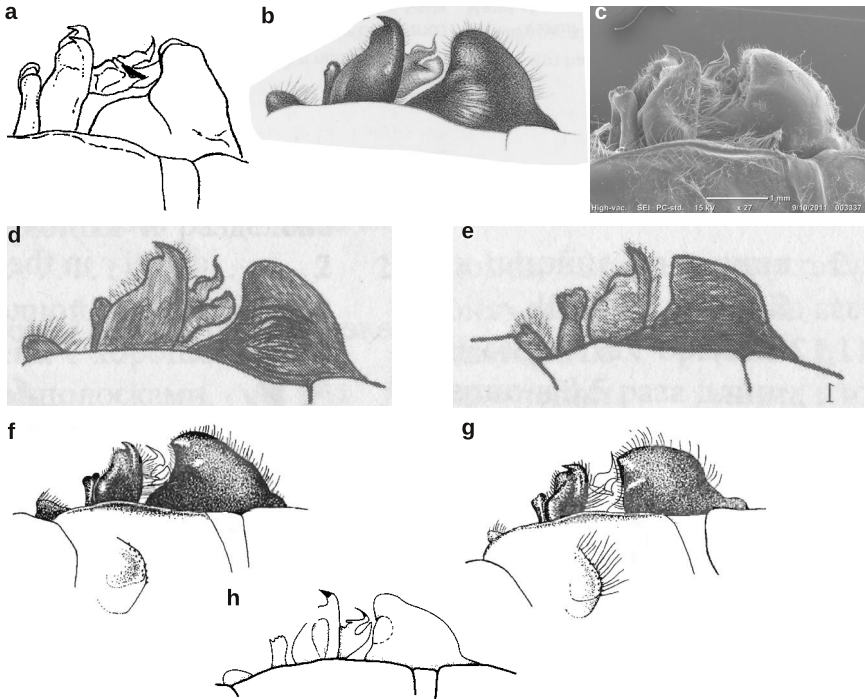


Fig. 4: Secondary genital apparatus of *Gomphus vulgatissimus*. A: Asahina (1986); B: Dijkstra et al. (2020); C: Schorr & Snegovaya (2022); D: Skvorstov (2010); E: *Gomphus schneiderii* from Skvortsov (2010), where hamulus and anterior lamina are different from *G. vulgatissimus*. The form of the genital lobe is quite interesting and should be studied more in detail when reconsidering the taxa in future (e.g. Seidenbusch 1997, pl. 2). F: *G. vulgatissimus* and G: *G. schneideri hellacidus* (Buchholz 1954); H: Du-mont (1991): *Gomphus vulgatissimus schneiderii*.

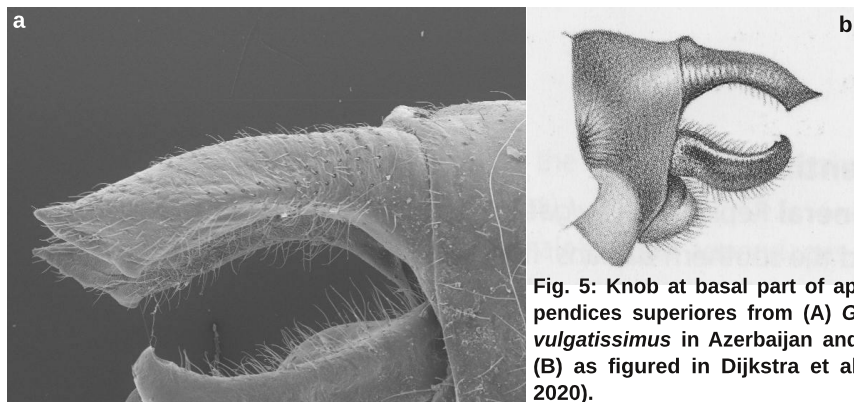


Fig. 5: Knob at basal part of appendices superiores from (A) *G. vulgatissimus* in Azerbaijan and (B) as figured in Dijkstra et al. (2020).

If one additionally takes the hamulus (see Fig. 4), which Suhling & Müller in Dijkstra et al. (2020) cite as a central identifying character (for *G. vulgatissimus*), but unfortunately do not illustrate *G. schneiderii*, only a perfect match can be found here as well. Fig. 1 e in Schorr & Snegovaya (2022) shows the pointed ends, which are explicitly referred to on page 191 in Dijkstra et al. (2020).

There is no alternative to this identification character. However, in the case of the taxon *vulgatissimus/schneiderii*, one day one will have to ask the question - e.g. considering Dumont (1991) or Schneider (1986) -, is the hamulus sufficient to separate the two taxa, if they are really two different taxa?

Studying the illustrations in Fig. 2, there are differences in the form and extension of the paraproct (nomenclatur following Walker 1953). Again, the form of the paraproct of the specimen illustrated in the SEM photograph in Schorr & Snegovaya (2022: fig. 1) resembles more the figure in Dijkstra et al. (2020) for *G. vulgatissimus* than *G. schneiderii*.

Since the basal knob at the superior appendage in Fig. 1a in Schorr & Snegovaya (2022) is also not an artefact and can be clearly seen when the SEM image is enlarged (Fig. 5), there is also 100% agreement with Dijkstra et al. (2020) here, since no knob is indicated for *G. schneiderii*. It remains unclear, whether this is true, as one can see such a knob in Fig. 6, a specimen that was identified as *G. schneiderii*. Whether and which functional role the knob plays is not to be considered here. It is given as a morphological structure in the most important identification book for European dragonflies (and in Skvortsov 2010 too).

Consequently, there can be no doubt that ***G. vulgatissimus* is found in Azerbaijan if these books by Dijkstra et al. (2020) and Skvortsov (2010) are taken into account alone.**

This alone is decisive for our criticism of the missing distribution map in Boudot et al. (2022). Regardless of the taxonomic assessment of the taxa *vulgatissimus* / *schneiderii*, a discussion of the current state of knowledge cannot simply be dispensed with just 'by opinion'. Since this is not done elsewhere and since it is also pointed out that it is difficult to make an identification "on the basis of colour pattern" the reaction of Boudot (2022) is hard to understand. Bartenev (1912a, b) did not discuss colouration variations, but focused exclusively on characters of the appendices. And Snegovaya has published black and white SEM illustrations that do not show colour variations, but only structural features.

In conclusion, with the **identification possibilities currently available** *Gomphus vulgatissimus* is figured in Schorr & Snegovaya (2022).

And now? What next?

How this picture changes when area-geographical factors are included in the consideration can only be revealed by later and detailed studies of the taxonomic complex *Gomphus vulgatissimus/schneiderii*. This is because the characters of *G. schneiderii* illustrated e.g. by Schneider & Ikemeyer (2019: 139) show considerable similarities to those I have discussed here in the context of *Gomphus vulgatissimus*.

And if one considers the specimen identified as *G. schneiderii* by Kosterin & Ahmadi (2018) from Iran (Fig. 6), one involuntarily realises that there are no, at least no signi-

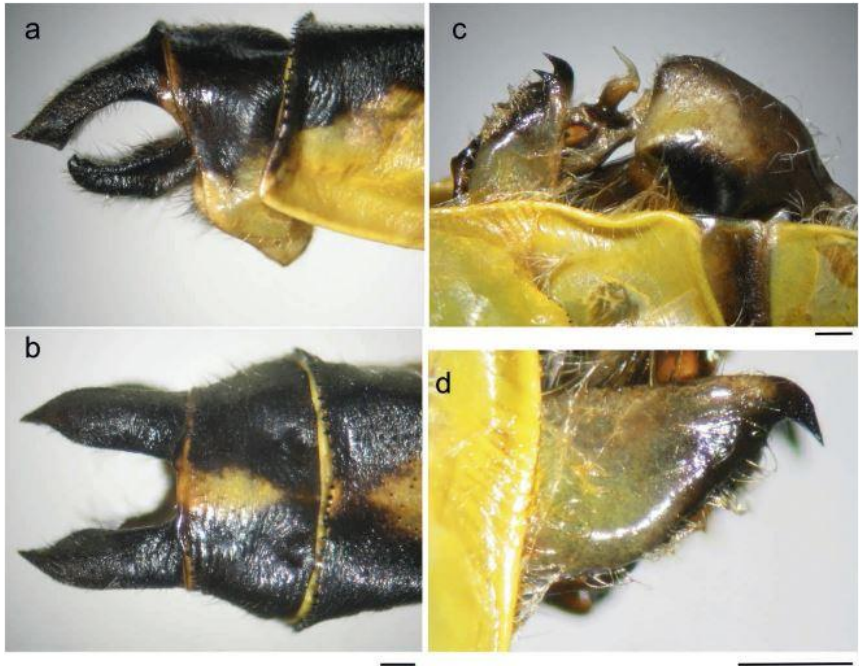


Fig 45. Abdominal details of the male of *Gomphus schneideri* collected on the Ghomrood (Anaarbar) River left bank: a – anal appendages, dorsal view; b – ditto lateral view; c – accessory genitalia, lateral view; d – hamulus posterioris, lateral view. Scale bar 0.5 mm.

Fig. 6: Taken from Kosterin & Ahmadi (2018).

ificantly different structural characters to *G. vulgatissimus*. May be that one day the form of the anterior lamina and the genital lobe will help to differentiate the taxa. Same to form of appendices and paraproct. Correlating molecular studies with such morphological structures may help to get more insight into the taxa.

Comparing the secondary genitalia figured in Skvortsov (2010) one will find some consistency with *G. kinzelbachi* (see Schneider & Ikemeyer 2019) if you set a focus on the hamulus. And upon comparing the morphological structures with further figures in some other publications more (e.g. Beschovski 1994, Puschnig 1926, Schneider 1986, Seidenbusch 1997b, St. Quentin 1968) confusing will grow even more.

In conclusion, there can be no doubt that the complex *G. vulgatissimus/schneiderii* and probably also other species must be fundamentally rethought and reworked.

Final remark

I have merely tried to add a few pieces to the mosaic, but do not intend to criticise or disparage anyone with my comments. If I have misunderstood something or overlooked important sources, please accept my apologies.

Science is a process that is in constant flux, the direction of which new perspectives and insights can influence.

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