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JQS Journal of Quaternary Science QRA Quaternary Research Association

A response to the comment on the article: Plant—insect interactions … (Pokorný and Borges, 2023), by Góis-Marques, Madeira and Menezes de Sequeira

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We gratefully appreciate the comments made by Carlos A. Góis-Marques, José Madeira and Miguel Menezes de Sequeira (hereafter CAG-M et al.) on our recent paper in the *Journal of Quaternary Science* (Pokorný and Borges, 2023). We would like to respond regarding the content and partly also the tone of their comments.

We were surprised that CAG-M et al. chose this means of communication given that they were involved in the early stages of the work and that they declined the offer of co-authorship. We clearly acknowledged this help at the end of our article.

We first note that many of the specific c riticisms and irregularities reported by CAG-M et al. focus excessively on written formulations and expressions, or comment on information taken from the literature that could not be verified. For the most part, we are able to refute the objections of CAG-M et al. Where we agree with them, we present revised wording here, highlighted in bold type.

1. Our wording here is not quite appropriately conceived, and would be better expressed as '… the fossil record of the Azores still **almost** lacks **any** direct **somatofossil** of terrestrial **insects** …'. However (and if we are to be consistent in wording and terminology), a number of sources cited by CAG-M et al. mention sub-fossils, i.e. Holocene (e.g. Petuch, 1988), and not fossil evidence of invertebrates from the Azores (e.g. Raposeiro et al., 2021b; Ritter et al., 2022); the finds of snail shells of *Leptaxis vetusta* from Prainha Bay on the island of Santa Maria, reported by Callapez et al. (2003), probably fall into this category.

In addition—and unless we have missed something—in the aforementioned article by Góis-Marques et al. (2019c), the work of 'Machado (2020)' is not cited, as stated CAG-M et al. We assume that they are referring to the masters thesis of Machado (2019). Regardless, we are glad that they mention this dissertation, because at one time Pedro L. A. S. Machado and Richard Pokorný also dealt with the issue of plant—animal interactions in the Azores (e.g. see p. 49 in his thesis; Machado, 2019).

2. We do not consider it logical to cite any references to support a claim of this type. If fossils were known from these islands, they would be mentioned along with a link to the relevant article or source. Regarding rhizolites—they belong to the category of trace fossils, not somatofossils, so they were not mentioned by us. Regarding the plant remains at the Lake Caldeirão site (Island of Corvo), again these are young, i.e. sub-fossil, samples (see Raposeiro et al., 2021a, 2021b).

3. The wording we have used is sufficient for the purposes of our article. Ou main goal was not a volcanological or sedimentological study, but rather a palaeoecological study of the Azores. We do not understand why CAG-M et al. explicitly mention flower fossils—we do not mention these in our article.

4. CAG-M et al. provide a detailed critique of Table S1 in our Supporting Information. We discussed this summary table with CAG-M very extensively at the beginning of the manuscript preparation, and CAG-M contributed many important additions and observations to it. We agree that the table was created based on a search of a large amount of literature, but it certainly cannot be called 'data recycling'—it provides important background for understanding the fossil plant communities of the Azores.

Regarding *Pittosporum undulatum*, CAG-M et al. are correct that, as a non-native species, it cannot be present in the fossil record, but only in the sub-fossil record presented in Table S1, so we should have correctly named it as follows: Summary list of fossil and **sub-fossil** plants described in the Azores.

5. We agree that the text here gives a misleading generalization, and the text should be reworded as: 'Based on **previously published** radiocarbon dating, **as mentioned also** in Góis-Marques et al. (2019), **these fossils are dated** to the Late Pleistocene to Holocene (<50 ka).'

6. This has already been explained in point 1.

7. We do not fully understand the criticism made here by CAG-M et al. The text of the article discusses the evolutionary history of the Azores in relation to the phylogeny and phylogeography of selected arthropods, concluding with a timeline of the three islands: Santa Maria (oldest)—São Miguel (younger)—Terceira (youngest), with reference to Florencio et al. (2021) and their table 1. The island of São Jorge is not relevant here.

8. Although our article mentions the island of Santa Maria only in passing, we largely agree with the comments of CAG-M et al. on this point. The mentioned study on the evolution of this island was indeed published a year later, so the correct citation is Ramalho et al. (2017). We neglected to mention the publication by Ramalho et al. (2020), but we consulted Professor M. Ramalho directly when preparing the manuscript regarding the dating of the age of the islands. However, and as also confirmed by Ramalho et al. (2020), that there are no onshore records of eruptions younger than ca 2.8 Ma on the island of Santa Maria.

9. We appreciate that the maps are not our original work. All were created on the basis of open GIS data on the map server of the Laboratório Nacional de Energia e Geologia. We should therefore add the following text to the description of Figure 2:

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Simplified based on raster open GIS data at: https://geoportal.lneg.pt.

Regarding the legend: No. 2–recent/subrecent is a synonym for the term 'post-settlement'; No. 3–we agree with CAG-M et al. and the caption should read 'Quaternary volcanics (**Pleistocene**/Holocene)'; No. 4–we agree that this is an overgeneralization and the description should be '**Lower** Pleistocene volcanics'.

10. This has largely been covered in point 4. Regarding the identification of the MV-AdH_190 sample, we agree with CAG-M et al. that it is more appropriate to use an open taxonomy as used by Pokorný and Borges (2022). In the case of the specimen with an ophionome mine, the text should therefore say '***?Laurus azorica***' throughout.

11. Regarding the Espalamaca locality, we relied only on an imprecisely drawn location given in Forjaz (1960), as it was no longer possible to identify it in the field (more than 60 years later). If CAG-M et al. have more detailed information on the location of this outcrop, we would welcome an update of the coordinates.

Regarding the stratigraphic affiliation of the volcano-sedimentary sequence, it would be of value to carry out a detailed revision of the stratigraphy of the whole island, which has still not been completely satisfactorily resolved, but for the present purposes we used the classification published by Larrea et al. (2018).

12. According to our observations, andesite predominates in the ignimbrite deposit at the location mentioned, which is also confirmed by geological maps (e.g. Zbyszewski, 1959). However, it is possible that trachytes are also locally part of its composition, and therefore we have no objection to the relevant sentence in the article being amended to: '… section is composed of reddish-brown to greyish **trachy**andesite ignimbrites…'.

13. The criticism made by CAG-M et al. is based on the fact that they have taken the mentioned sentence out of a wider context. In full, the article states that: '*The largest island of the archipelago was formed by volcanic processes, which can be subdivided into six geologically contrasting areas*.' The second part of this sentence puts everything into perspective.

14. The first comment dedicated to the Terceira Rift is meaningless. In the text of the article, we do not make any contradiction to the fact that it is related to the creation of other islands. The volcanological affiliation of pumices and ingimbrites to the Cinco Picos volcano is consistent with both geological maps and previously published studies (e.g. Larrea et al., 2018).

The criticism relating to rhyolites is incorrect; the presence of rhyolites, typified by their high content of basic amphiboles and pyroxenes (also termed pantellerites), is well described from the island of Terceira (Mungall & Martin, 1996; Jeffery et al., 2017; Larrea et al., 2018). In our opinion, the text '… *the production of ignimbrites* …' is sufficiently comprehensible.

The final criticism of the Fanal Bay sediments/volcanics is again taken out of context. The relevant sentence states: '*Pyroclastic sediments of this area are represented by ignimbrites and tuffs …*'. This makes it sufficiently clear to the reader what type of deposits we are referring to.

However, we agree with their comment regarding the location of the Pico Alto caldera. The text in the article should therefore correctly read: 'The young cone of the Pico Alto volcano lies on the **northern** slope of the older Guilherme Moniz volcano …'.

15. We have given the coordinates of Ponta do Cintrão, Ribeirinha, on the island of São Miguel as accurately as possible, based on both published sources cited by CAG-M et al. Moreover, in Góis-Marques et al. (2019, figure 3), this locality is shown as just one graphical marker among many others, without a specific description. However, we acknowledge that we could have been more specific, and instead of the sentence 'One can therefore only guess that the locality lies approximately at midpoint of the northern shore of the São Miguel Island, north of the Ribeirinha community …' we could have stated: '… **the locality lies on the Ponta do Cintrão Peninsula, north of the Ribeirinha community** …'

16. A brief article dedicated to the description of the fossil mine *Cuniculonomus* isp. (MV-AdH_190) was created at the request of the Museu Vucanoespeleológico 'Os Montanheiros', in whose depositories the relevant specimen is located, and the museum staff very willingly provided it for study. The manuscript to *JQS* was submitted for review earlier than the article to the museum journal *Pingo de Lava*, so none of our statements was violated. Moreover, we believe that the popularization of science results in regional journals is as important as publishing in international periodicals.

Regarding the issue of ichnological taxonomy, we certainly would not contradict the professional knowledge of CAG-M et al. in the field of botany, or even the geology of the Azores. However, none of them is a well-founded specialist in ichnology. Therefore, although we agree that a taxonomy based only on photographic material can be problematic in many cases, in this particular situation their comment is not of relevance.

17. This follows on from the previous point. To date, there are only 17 ichnogenera described from fossil wood, and their diagnoses are significantly different. It is thus possible to safely identify not only the ichnogenus (*Xylonichnus*) but also the specific ichnospecies (*X. trypetus*). We have sufficiently explained our taxonomic identification in the text of the article (see Remarks and Description).

At the same time, we are grateful that CAG-M et al. found our extensive Discussion to be of value, in which we presented a number of hypotheses regarding the taxonomic affiliation of possible tracemakers responsible for the plant—animal interactions we found.

In conclusion, we are certainly not against constructive criticism, but at the same time we are not supporters of unnecessary wordplay and 'scientific bureaucracy' in the form of long and ongoing public discussions in professional periodicals, which consume a large amount of time that could be used much more usefully. We believe that there are a number of other, more appropriate solutions for similar situations, which are also simpler, collegial and, dare we say, friendlier.

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Data availability statement

Data sharing is not applicable to this article as no new data were created or analyzed in this study.

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