

***Interactive comment on* “Pollen-based reconstruction of Holocene vegetation and climate in Southern Italy: the case of Lago di Trifoglietti” by S. Joannin et al.**

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Pollen-based reconstruction of Holocene vegetation and climate in Southern Italy: the case of Lago de Trifoglietti, S. Joannin et al.

Overview

This manuscript presents the results and interpretation of new palynological analyses at high (sub-centennial-scale) resolution of a Holocene lake sequence from southern Italy. The paper presents a thorough palynological study based on tried and trusted techniques, which aims in particular to evaluate hydrological impacts over the Holocene on the regional and local vegetation through the use of palynological indices and

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Modern Analogue Technique precipitation reconstructions. The manuscript contains a wealth of original pollen data, and displays care and attention to detail in the presentation of the results. The interpretations appear valid, and are supported by useful references to the regional literature for southern Italy. The paper makes an important contribution to the growing literature on the regional specificity of Holocene climate variability in the Mediterranean region and I would recommend publication in *Climate of the Past*, subject to a number of corrections/amendments suggested below.

### General comments

-Avoid common or English language names for taxa entirely. There is no clear logic or benefit to using names such as alder, beech, etc. instead of *Alnus*, *Fagus*, etc. and I would strongly advise against their usage. It's not really possible to do it consistently; i.e. if alder, beech etc., then why not wild olive, hop-hornbeam, blunt-leaved bog-moss etc.?

-While the analysis of modern pollen samples is clearly a key component of vegetation and landscape reconstruction via pollen analysis, I'm not convinced that section 4.2.1 on modern surface samples adds a great deal to the overall manuscript. Perhaps it could be summarised in one or two sentences to indicate that study of modern surface samples supports the detection/characterisation of regional and local vegetation types by sedimentary pollen analysis. The modern surface sample dataset is rather small, and it is ultimately only used in a qualitative way to improve the interpretation of the record.

-The authors should comment on the quality of the MAT reconstruction (i.e. the dissimilarity coefficient and the threshold considered to represent robust analogues) and the location of analogues. It appears that these details may be considered in another paper, but they should be covered here, at least briefly, to support the validity of the reconstruction presented. For example, is the rather strong precipitation anomaly linked to the Preboreal Oscillation (PBO) driven by a switch from regional to extra-regional

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analogues, and are dissimilarity coefficients acceptable in this part of the reconstruction? The authors should also indicate why they focus on the MAT precipitation reconstruction, and not other climatic variables.

-Regarding the Preboreal Oscillation (PBO), the authors state that pollen zone T-2 and the strongly dry reconstructed climatic conditions were initially thought to have been related to the Younger Dryas, but in light of the dating are more likely to relate to the PBO. They also voice concern that the magnitude of the climatic anomaly appears surprising (P2243). It is true that the signal of the PBO (or indeed multiple PBOs (cf. Bohncke and Hoek, 2007)) may often be rather weak (e.g. Fletcher et al., 2010). I wonder whether the observed minimum in Arboreal Pollen and associated reconstruction of low precipitation in zone T-2 reflects not just the PBO event, but rather may have been more typical of the whole Preboreal interval; in other words the record may reflect the superimposition of a centennial-scale anomaly on more pervasive dry conditions prior to  $\sim 11$  cal ka BP. Although this cannot be fully determined due to the length of the record, this would appear likely in the wider Mediterranean context, where many vegetation records suggest protracted aridity during the earliest part of the Holocene (Tzedakis, 2007). In the SW Mediterranean, for example, we have described a step-like increase in moisture availability at 10.6 cal ka BP (Fletcher et al., 2010). Perhaps the authors could take this into account in their discussion of this section of the record, also taking into account my question regarding the analogues above.

Detailed scientific comments/corrections:

P 2225. Line 20. Add additional references to studies highlighting this climatic connection between the high-latitudes and the Mediterranean region on rapid timescales (Fletcher et al., 2010; Combourieu Nebout et al., 2009; Pross et al., 2009)

P2228. Line 10. The meaning of “annual rainfall (including cloudiness)” is unclear; please specify.

P2230. Line 6. Indicate on what basis (total depth, stratigraphy, etc.) core S2 was

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chosen for further analysis.

P2231. Line 4. The authors should indicate the calibration curve used (e.g. IntCal 09) as well as the software used for making the calibration (Calib 6.0) – the two things appear conflated here.

P2231. Line 15 (and elsewhere). I'm not convinced that the term “semi-detailed pollen diagram” is clear or standard – it implies selection of some pollen types and omission of others (as per usual in the publication of pollen data) but does not make clear the basis for selection. I would recommend using simply “pollen diagram” in-text, and then, in the figure caption for the diagram, indicate the basis for selection of pollen types, for example “showing select pollen taxa indicative of regional vegetation” or “showing main pollen types occurring at least once at abundances of >5%”.

P2234. Line 4. The authors indicate that “No volcanic materials have been found in this last level.” On its own, this sentence does not add much, and opens a series of questions: Did the authors look for volcanic materials (tephra) systematically in the core? Was tephra noted elsewhere? Was this depth of particular interest because of previous knowledge about a key regional tephra time-marker?

P2234. Line 19 (and elsewhere). The term “rate” is misused in this section, and should be changed to “abundance”

P2235. 26. When interpreting zone T-2, the authors should consider a change from “the site may have been above the timberline at that time” to “the site is likely to have remained above the timberline at this time” - because they have just said that the site was probably above the timberline in the previous paragraph for zone T-1. Can they go further and suggest additional lowering of the timberline?

P2236. Line 28. Meaning/purpose of sentence unclear: “This is also the extremely rare *Pistacia*.”

P2239. Lines 16-19. The authors should remove the phrase “Unfortunately, CONISS

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software did not identify. . .” – it implies that the zonation was undertaken with a particular outcome preconceived or desired (unscientific).

P2240. Lines 2-4. The authors suggest that CONISS “identified a first order opposition between algae and terrestrial taxa”. This isn’t strictly speaking true – CONISS is used for stratigraphically constrained clustering of samples, not clustering of taxa. So, the authors need to clarify what they mean here, and how exactly the CONISS zonation has lead them to create taxonomic clusters.

P2240. Line 25. Change “coincided” to “suggests”. As the increase in Fagus pollen most likely drives the MAT reconstruction of higher rainfall, it is obviously not a “coincidence” and the wording should reflect this.

P2242 Line 6. Change “arrive at” to “reach”P2243. Line7. Delete “it”?

P2243. Why is the PBO considered an example of millennial-scale trends, and not in the next main section on centennial-scale variability?

P2243. Line 12-14. Consider also evidence for Preboreal Oscillation(s) in western Mediterranean in Fletcher et al., 2010.

P2246. Line 24. The authors indicate oscillations recognised at higher latitudes, but should also recognise records indicating similar oscillations elsewhere in the Mediterranean region (e.g. events at 10.1, 9.3, 8.2 and 7.4 cal ka BP in the vegetation record in MD95-2043 (Fletcher et al., 2010)).

P2247. Line 11. Again, reference to Fletcher et al., 2010 can be made with respect to detection of 8.2 ka event in the wider Mediterranean region.

P2248. Line 14/15. The authors infer a localised increase in human activity between 6.8-6.5 cal ka BP – is there any archaeological or otherwise independent evidence to support this?

P2249. Line 5. What is meant by “declination”?

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P2251. Line 4. The authors refer to “that time interval” for the regional Neolithic – it would be useful to give the best available dates for this cultural interval at this point.

P2252. Line 17. The authors should remove the word “puzzlingly” – it seems to me that this is sufficiently well-explained as to not present a “puzzle”

P2252. Line 22. Opening word “It” is vague – specify what supports the southward delay. The authors should also try to summarise here the implications in terms of climate, soils, vegetation dynamics, etc. of this delay Table 1. Why are the dated materials described as “Wood-Peat-Charcoal” – which is it?

Technical corrections:

P2229. Line 1. “swamp of Carex paniculata swamp” – delete “swamp of”

P2229. Line 20. Change “man-induced rise” to “anthropogenic rise”

P2231. Line 6. When describing the similarity of the ages, either give both ages in the text (instead of just one) or none, and refer to the table instead.

P2235. Line 17. Change “Thoughtf” to “Although”

P2236. Line 11. Change “settlement” (typically reserved for people/buildings, etc.) to “establishment”

P2237. Line 18/19. Change “the anthropogenic” to “anthropogenic” and change “a puff” to “a minor increase” or something similar.

P2238. Line 5/6. Change “begin” to “begins” and “all along” to “throughout”

P2241. Line 22. Change “example of well-dated pollen sequences” to “example of a well-dated pollen sequence”

P2244. Line 14. Change “is scarce” to “is absent” or “is weakly expressed”, depending on the desired meaning

P2244. Line 19 (and check elsewhere). Change “sclerophilous” to “sclerophyllous” i.e. C959

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“hard-leaved” not “hard-loving” plants(!)

P2245. Line 6. Change “More to the point” to “Specifically” or “In this case”

P2245. Line 22. Change “General dyier” to “Generally drier”

P2246. Line 13. Change “coevals” to “coeval”

P2252. Line 1. Change “the Southern Italy” to “Southern Italy”

Table 2. Caption. Change “surrounding” to “surrounding”

Figure 4. Caption. Change “Fagus wood” to “Fagus” – descriptors of vegetation structure are not given for any of the other categories.

Figure 6. Change “Ericacea” to “Ericaceae”

Figure 7 (possibly elsewhere) Change spelling: “exageration” to “exaggeration”; Change “Lamiacea” to “Lamiaceae”

Figure 8. Change “peat-land” to “peatland”

#### Additional References

Bohncke, S. J. P. and W. Z. Hoek (2007). "Multiple Oscillations during the Preboreal as recorded in a calcareous gyttja, Kingbeekdal, The Netherlands." *Quaternary Science Reviews* 26: 1965-1974.

Combourieu Nebout, N., O. Peyron, et al. (2009). "Rapid climatic variability in the west Mediterranean during the last 25 000 years from high resolution pollen data." *Climate of the Past* 5: 503-521.

Fletcher, W. J., M. F. Sanchez Goñi, et al. (2010). "Abrupt climate changes of the last deglaciation detected in a western Mediterranean forest record." *Climate of the Past* 6: 245-264.

Pross, J., U. Kotthoff, et al. (2009). "Massive perturbation in terrestrial ecosystems

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of the Eastern Mediterranean region associated with the 8.2 kyr B.P. climatic event." *Geology* 37: 887-890.

Tzedakis, P. C. (2007). "Seven ambiguities in the Mediterranean palaeoenvironmental narrative." *Quaternary Science Reviews* 26(17-18): 2042-2066.

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