William Fletcher
Thanks to his review, we added and clarified specific changes.

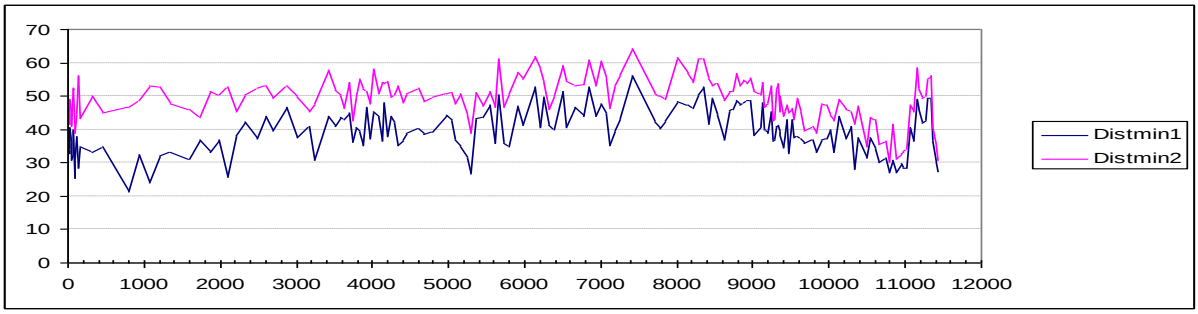
General comments
-Avoid common or English language names for taxa entirely. There is no clear logic or benefit to using names such as alder, beech, etc. instead of Alnus, Fagus, etc. and I would strongly advise against their usage. It's not really possible to do it consistently; i.e. if alder, beech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

-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 analysis of modern pollen samples and its comparison with actual vegetation has been very useful for us in order to interpret in a better way the fossil pollen spectra. Without this we could only postulate (and not demonstrate) that the fossil pollen spectra include both regional and local vegetation components. As an example, identification of *Eupatorium* on modern pollen spectra help us to identify it in the fossil record and therefore to interpret its abundances as it is today associated with the vegetation belt which develop at the edge of the lake.
We added in part 3.3.1 surface samples:
"Studying the relationship between pollen rain and actual vegetation is essential for the interpretation of fossil pollen spectra. The pollen rain depends primarily on the internal parameters of the plant (production potential and dispersion) and external factors (topography and climate). Barthelemy and Jolly (1989) consider that the most important factor is the topography: the wind that rises along the slope, carrying the pollen that falls to the ground where the slope is interrupted by a shelf. The deposit is then behind the edge of the shelf into the basin (Brugiapaglia et al., 1998). At Trifoglietti, the filter effect operated by the dense forest vegetation that surrounds the lake is also to take into account. In order to interpret the fossil diagram, five…"
We then added in part 4.2.1 surface samples:
Lake Trifoglietti pollen rain broadly mimics the corresponding types of vegetation: in fact the filter effect therefore determines that the regional pollen rain is poorly recorded, while a good assessment of the local vegetation is well demonstrated by the results obtained in the diagram (Fig. 6).

-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 analogues, and are dissimilarity coefficients acceptable in this part of the reconstruction?

The quality of the MAT reconstruction appears acceptable: only 2 samples between 7500 and 7950 yrs cal BP have been removed because the number of analogues selected was too low. For all other samples, 8 modern analogues have been selected (7 for 2 samples). The adopted threshold (61.99) is also acceptable together with the distances (see the figure).



For the Preboreal Oscillation, the distance is quite good, even if the analogues selected are mainly from high elevation East regions (Greece, Turkey). Then for the Holocene, the analogs selected are from Italy (samples close to Trifoglietti), Alps, and Pyrenean areas.

The authors should also indicate why they focus on the MAT precipitation reconstruction, and not other climatic variables.

Winter and summer precipitations are reconstructed and discussed in the paper of Peyron et al (same issue) and are compared with a new climatic reconstruction based on 3 others pollen sequence from Italy. In this paper, 4 methods (MAT, WA, WAPLS, NMDS/GAM) are applied to these 4 records (Lakes Ledro, Accesa, Trifoglietti, Pergusa).

-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 _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 steplike 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.

We are aware that persistence of dry conditions in southern Italy as well as in other area from Mediterranean (cf discussion in 5.1.1) may have been superimposed by one or multiple PBOs. To our point of view, dates, length and resolution of the record do not allow us to go further in the interpretation. We cannot exclude a stronger response due to local particularities (Is the *Fagus* pollen drop driven by climate sensibility and/or by a taphonomical bias related to its pollen dispersal potential?)

Detailed scientific comments/corrections:
P 2225. Line 20. We added 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.*
We suppressed including cloudiness

P2230. Line 6. *Indicate on what basis (total depth, stratigraphy, etc.) core S2 was chosen for further analysis.*
We write: "Three cores were taken (S1, S2 and S3; Fig. 2b) along a transect from centre of the lake toward the northeastern shore to find a sediment sequence capable of documenting the entire Holocene in high resolution. Thus, the core S2 sequence was chosen…"

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.*
We completed by adding: The radiocarbon ages have been calibrated in years cal. BP by using program Calib 6.0 (Stuiver and Reimer, 1993) (calibration curve IntCal09; Reimer et al., 2009).

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%".*
We change (here and elsewhere) to: "A pollen diagram of selected taxa"

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?*
We change to: In this last level, strong MS values are not explained as no volcanic minerals have been found.

P2234. Line 19 (and elsewhere). We change the term "rate" 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?*

We change and added “The decrease of AP percentages could be due either to a lowering of the tree limit or to a poor pollen productivity of the temperate trees as a consequence of the cooling."

P2236. Line 28. We erased the sentence “This is also the extremely rare Pistacia.”

P2239. Lines 16-19. *The authors should remove the phrase “Unfortunately, CONISS software did not identify: : :” – it implies that the zonation was undertaken with a particular outcome preconceived or desired (unscientific).*
We change: Within T-11 (ca. 2100-800 cal. BP), two sub-zones can be distinguished T-11a (ca. 2300-1500 cal. BP) and T-11b (ca. 1500-800 cal. BP) (Fig. 8),…

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.*
We agree, our phrasing was not exact. We change to: First order clustering obtained from CONISS analysis clearly separates samples where algae (Botryococcus) and more terrestrial taxa (spores and Alnus) are dominant (Fig. 7).

P2240. Line 25. We change “coincided” to “suggests”.

P2242 Line 6. We change “arrive at” to “reach”P2243. Line7. We change “to correlate it”

P2243. *Why is the PBO considered an example of millennial-scale trends, and not in the next main section on centennial-scale variability?*
We did that because this PBO event is the beginning of this record so that it has to be discussed before the rest.
P2243. Line 12-14. *Consider also evidence for Preboreal Oscillation(s) in western Mediterranean in Fletcher et al., 2010.*
We added the reference.

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)).*
We added the reference.

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.*
We added the reference.

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?*
We cannot corroborate this interpretation with archeological evidences which are, unfortunately, very rare at high elevation in this area. We discuss this phase in response n°3 of the reviewer #4.

P2249. Line 5. *What is meant by "declination"?*
We change the word to "effect"

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*.
According to a figure from Allen et al. (2002), this interval ran from 9000-8500 to ca. 4800 cal. BP. But we cannot confirm the reliability of such information as many archeological reports give no precise ages for the end of the Upper Neolithic, we therefore cannot give it in the manuscript.

P2252. Line17. We removed the word "puzzlingly"

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*
We change to: The pollen analysis supports a southward delay in the thermophyllous forest expansion dated to ca. 13500 cal. BP at Monticchio, ca. 11000 cal. BP at Trifoglietti, and finally ca. 9800 cal. BP in Sicily. Persistence of arid conditions is expected to explain the increasing delay from northern to southern Italy.

Table 1. *Why are the dated materials described as "Wood-Peat-Charcoal" – which is it?*
We change to wood-Charcoal. See response to Zanchetta's comments.

Technical corrections done:
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 "Thoughf" 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. "hard-leaved" not "hard-loving" plants(!)
P2245. Line 6. Change "More to the point" to "Specifically" or "In this case"
P2245. Line 22. Change "Generaly dryier" to "Generally drier"
P2246. Line 13. Change "coevals" to "coeval"
P2252. Line 1. Change "the Southern Italy" to "Southern Italy"
Table 2. Caption. Change "surronding" 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”