

## General comments

Despite I'm not a pollen specialist this contribution greatly improves our knowledge of vegetation and climatic evolution of Southern Italy and Mediterranean from a strategically located area at high altitude and with relatively low history of human impact. This represents a good contribution of very general interest and surely to be published on "Climate of the Past". Moreover, the use of pollen data to infer lake level oscillation is an interesting application and could be tested in other lake archives where lake level oscillation have been inferred using different proxies. The manuscript is clear and well written and I have not specific general comment to be addressed. In the following I just suggest some punctual clarifications and suggesting some additional citations hoping this can be useful for further improving the quality of the manuscript.

## Specific comments

Abstract line 27, change: At Trifoglietti, the 8.2 kyr event corresponds to the onset of cooler.....

Pag. 2225 line 19-20. I think here should be inserted also Favaretto et al., 2008 Quaternary International, 190, 89-102.

Pag. 2226 line 13: Just a general note of discussion out of the manuscript. The "possible short-term time-transgressive climate oscillations .....as reported by Di Rita and Magri, 2009, is very probably an artefact of different age model on different archives or due to the correlation of different climatic events. I suggest care in transform some based on dubious and very weak evidences in a sort of paradigm to be quoted (for the rest the paper of Di Rita and Magri, 2009 is an excellent paper).

Pag. 2227 line 24: "...following the direction of subsidence, ..." do you mean depression created behind the landslide body?

Pag. 2231 line 1-2: "14C ages measured on terrestrial plant macrofossil". In table 1 should be better specified if these are performed on wood, peat or charcoal. On the other and the use of bulk peat does not guarantee that all material considered use CO<sub>2</sub> for photosynthesis from the atmosphere (i.e. not all material is strictly terrestrial). Nothing against it (we usually use what we have) but just to be consistent between table and text.

Pag. 2240 line 23: why here is quoted Peyorn et al., 2012?

Pag. 2242 lines 1-10. This sequence of sentences is not very clear to me. Please can the authors be more explicit?

Pag. 2244 line 25, in this long successions of quotations could be inserted Zanchetta et al., 2007 QSR and Leng et al., 2010 Biogeosciences.

Pag. 2246: line 15. Using tephrostratigraphy (as complementary work of Giraudi et al., 2011) Zanchetta et al., in press QR indicate that re-growth of the Calderone Glacier on the Appenine probably started later than 5.7 cal BP.

Pag. 2246 line 25 probably some quotation on Greenland ice core would be useful (e.g. Rasmussen et al., 2006 QSR?).

Pag. 2247. The discussion on reduction in precipitation between ca. 8.2-7.5 ka is particularly interesting because data in the Central Mediterranean (e.g. Adriatic Sea) seems to suggest that this event lasts more than the so-called 8.2 ka event. Moreover, there are data that suggest a clear event identifiable as the 8.2 (quoted references), but other could suggest some occurring at 8.2 (8.6) but also younger. So the event 8.2-7.5 could be a mix of different events which need to be resolved in details. I have also the general impression that in some records quoted (e.g. Aritzegui et al., 2000) the main concern is chronology. So some events lasting some centuries are the mix between more than one event, but in some cases it is a real problem of chronological expansion of a short events. In the list of citation is surprisingly lacking Siani et al., (2001 Science) and Siani et al., (2010 JQS), in which the problem of chronology is very well discussed. These papers are the base for the work of Siani et al 2012 (quoted). Here the sapropel interruption is identified unequivocally with 8.2 event (whereas in other marine cores the chronologies are more dubious) and it correspond to a clear temperature reduction.

Pag. 2247 lines 19-20. I'm not so sure that this is clearly seen in oxygen isotopes from Carburangeli speleothem by Frisia et al (2006): it is more an "ensemble" of evidences (petrography, isotopes, growth ect).

Pag. 2249 Recently Roberts et al., 2011 (The Holocene) have discussed the presences of some general period of dry condition in Eastern Mediterranean: I suggest to spend a couple of sentence in comparing these period with those identified in this paper.

Pag. 2249. Here I suggest to consider also the paper of Drysdale et al., 2006 (Geology) about dry event at ca 4-4.2 ka.

Overall, I consider this is a good contribution particularly useful for disentangling the climatic and environmental condition in the Mediterranean area. I hope my comments will be useful for improving the manuscript.

Sincerely

Gianni Zanchetta