

Interactive comment on “Climate changes since the mid-Holocene in the Middle Atlas, Morocco” by M. Nourelbait et al.

Anonymous Referee #3

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The manuscript reports a palaeoenvironmental study covering the last 6000 years in Middle Atlas, Morocco. Biological and non-biological proxies are used to observed changes in the lake and its surrounding. In this aspect, climate reconstruction is shown. Results by themselves look robust; the data are well presented in the figures. Clearly, this study has the potential to be published in CP. However, the discussion miss to explain the data relevance and to develop ideas suggested in introduction. Furthermore, numerous minor errors both in the manuscript organisation and in the data presentation shall have been corrected before the submission. My thoughts are therefore that this paper is not finalised in this present form.

Major comments:

A detailed review of the available literature must be done. This will strongly improve
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the present manuscript which doesn't present the most relevant or recent studies from the western Mediterranean, Morocco and Middle Atlas: Reille 1976; Barker et al 1994, Lamb et al 1989, 1991, 1994, 1995, 1999; Cheddadi et al. 1998, 2009; Combourieu-Nebout et al., 2009 ; Detriche et al 2008, 2009, 2013; Rhoujjati et al. 2010; 2012; Amami et al. 2013; Fletcher et al., 2008 and 2013; Zapata et al., 2013; Reddad et al, 2013; Giraudi et al. 2013, Wassenburg et al., 2014; El Bait et al. 2014; Muller et al. 2015; Lebreton et al. 2015). Also see the synthesis presented in Fletcher and Zielhofer, 2013. While the actual review covers the Mediterranean area up to the Anatolian plateau, it should mention synthesis from Fletcher et al., 2013 and Magny et al., 2013 (and Joannin et al., 2014 if the authors still wish to go eastward). This comment is to be addressed both in introduction and in the discussion where one would like to see what the processes behind the observed environmental changes are.

The MS failed to rigorously discuss the main data trend. Whether the Seasonal index trend is valuable, readers do not see much evidence. For instance, modern values of SI are not related to references, fig 7 does not show % as mentioned at p913 nor winter precipitation as mentioned p918. The whole discussion about SI is related to winter vs. summer precipitation and to the favoured evergreen oaks which are however not shown in the pollen diagram. The first part of the trend is based on a higher water level with an enhanced development of algae recorded in C/N and d13C values. The pollen diagram does not evidence this trend, and on contrary shows higher sub-aquatic plants (Cyperaceae) at first than after where Potamogetonaceae and Typha dominate. Can you please explain these opposite results? This part needs to be thoroughly revisited and discussed at the light of presented data together with other evidences available in the literature.

Discussion of the 5.5kyr event: Both Abstract, Introduction and discussion highlight a 5.5kyr event which is insufficiently discussed with no references at all. For example, you should rigorously present all data and highlight that this corresponds to the sand level deposit, which in turn certainly affects the pollen diagram. Clays develop at that

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time, but do you have any idea of which clay type it is so that you can maybe relate this event with stronger saharian winds (summer?) (Bout-Remazaille et al 2007). This would be worth a try but to read other NA papers showing this event and relate/match your record to their interpretation in terms of climate process. For example, in the Jefara, the second Saharan humid period is divided into two parts by a dry spell that occurred around 5.5–5.4 ka BP (Giraudi et al., 2013).

Middle class comments (only some are listed here but I encourage the authors to carefully improve the MS).

Age-model: No information on what is dated; you should use the 2sigma error calibrated with intercal13 and an appropriate program to plot and calculate the ages of samples. Then is artificially added an age at 0cm which is not correct. In the end, data stop at 250 years cal BP without any explanation! Does it suggest a possible water reservoir effect? Can you please detail?

Methods: methods are listed but the reader does not know which process the authors expect them to highlight. For example, C/N threshold is not argued so that the reader has no clue whether it is a good interpretation. It is mentioned a depth resolution of 5 cm for all proxies so that the study should be based on 250cm/5cm=50 samples. This is clearly not the case: 46 samples for d13C, TOC... 30 samples for pollen and climate variables. Can you please detail?

Results: the results coming from the pollen data are not insufficiently presented. We do not know the lower limit of pollen sum counted by samples. Results on vegetation are not presented in the text and the figures are not explained. For what use is the pollen zonation? What means the red line in figure 4? TN is not shown but presented in results. Should be shown or not presented in the text. Same for sand.

Discussion: p8 l2-10. Why this paragraph on human and climate relationship when the present study doesn't deal with this? To be deleted. P6 l1. What is your conclusion about the SI? P11 l15-17. I don't understand on what is based this statement. Please

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argue. TOC is only mentioned in the discussion but not discussed. Why is it used for?

Minor comments (only some of the minor errors are listed here but I encourage the authors to carefully improve the MS):

The abstract has to be reshaped and carefully corrected to make it easily readable. The sentence "Holocene climate is known to be rather stable. . ." in spite of not being new is not a good way to introduce the time window of your study. Why the authors focus on the last 6000 yrs? Is it of particular interest? 1. Introduction: p4 l13-19: this paragraph is of any use? Repetition of the abstract! What is studied, a lake? 2. Study area: where does the climate data come from? During which season or month the lake level is the higher? Is there any river inflows? The figure 1 does not show well the lake, nor the scale and nor the coring site. Oaks are differentiated in the text not in figure 1c. 3. Materials and Methods: a presentation of the coring itself is necessary. Are they twin cores?

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