

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

Anonymous Referee #1

Received and published: 12 October 2015

This manuscript presents a new paleoenvironmental study in the Middle Atlas (Morocco). This work is based on complementary proxy analyses (pollen, grain size and geochemical proxies) of a core covering the last 6 000 years. The authors also present a pollen-based climatic reconstruction using a probability density function-based method. The data presented in this paper appear of great interest because paleodata coverage in northern Africa truly needs to be increased to better document the vegetation history and understand the climate dynamics in the Mediterranean region. However, the paper in its present form displays a number of inconsistencies that are mentioned below and the authors failed to cite relevant works in the introduction and discussion:

1) References to major works are missing in the introduction. The authors mention a

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long list of works showing variations in lake level from France, Italy, Spain and even the Dead Sea but they did not cite recent works on Central Mediterranean such as the synthesis of Magny et al. (2013 CP). In addition, excepting one pollen record from southern Spain, only continental pollen records from the Middle Atlas are cited. The authors should have taken into account pollen-based vegetation and climate reconstructions from the nearby marine sites from the Alboran Sea (Combourieu Nebout et al., 2009 CP; Fletcher et al., 2008 QSR; Fletcher et al., 2013 The Holocene). In addition, the synthesis paper of Fletcher et al. (2011 Catena) on rapid climate changes in the western Mediterranean region and impact on landscape should have been considered.

2) Chronology: Which CALIB software version was really used? CALIB 6.0 is indicated in the text (p.4012, l.16) and CALIB 7.0 in the table caption, with a different reference. Information on the calibration curve used and the material dated (charcoal, shell, bulk sediment. . .?) should be indicated. Presentation of the calibration results in Table 1 does not follow recommendations given in the CALIB manual (in particular indication of 2 sigma cal age ranges and relative area under distribution). In Table 1, what are SD1 and SD2 and does “Calibrated age” stand for median probability?

3) Methods: References for all methods used should be given. - p. 4101: l. 16: “stable isotopes of delta 13C” must be replaced with “carbon stable isotopes” or “delta 13C isotopic ratio” l. 18: “Pollen was” or “Pollen grains were” should be used l. 23: Justify why Cyperaceae pollen from this site is included in aquatic plants l. 26: replace “the fine grains (< 2 mm) of the sediment” with “the sediment fraction < 2 mm” -p. 4102: l. 3: Sentence meaning is unclear. l. 14-15: the method used for delta 13C measurements should be described l. 15: “University of Bordeaux” l. 21 & 26: “annual precipitation” instead of “annual amount of precipitation” -p. 4103, l. 2: give information on the database (name, reference, area covered. . .)

4) Pollen data *The description of the pollen diagram is very short and uncomplete. In contrast to the author’s statement, other taxa than Pinus and Cedrus, such as Poaceae

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and *Quercus*, present strong variations which are not described or interpreted. The authors do not give a clear description of the pollen diagram and do not present a comprehensive history of the vegetation changes at the site. Only the replacement of pine by cedar at 3750 cal yr BP is pointed out while for instance, two important forest opening episodes are clearly recorded, the first one preceding the cedar expansion and the second one between 2500 and 2500 cal yr BP marked by a drastic cedar reduction. *In the discussion, the vegetation changes recorded in the Hachlaf sequence are not compared to the other records from the Middle Atlas (only cited in the introduction) and the Alboran Sea.

5) Pollen-based climatic reconstructions *The reconstructed climate changes should be discussed in the context of the vegetation variations as recorded by the pollen diagram. Is the increase in precipitation seasonality in the second half of the record supported by the vegetation changes? Is *Quercus* taxon including evergreen or deciduous oak pollen, or both? And which pine and oak species are taken into account in the database used for the climatic reconstructions? Are cedar ecological requirements supporting higher rainfall seasonality? Which pine species are developing in the Middle Atlas and to which climatic characteristics are they associated with? p. 4105, l.25: “water persisted in the lake during the summer season which allowed the presence of aquatic plants (which flower during late spring and summer)”: Could the authors relate this assertion to the pollen diagram? p.4016: the authors state “After 3750 cal BP, Atlas cedar spread noticeably around the site while the pine forest strongly regressed. During this ecosystem transition we do not observe any major change in the reconstructed amount of annual rainfall or in winter temperature.” However, we can see in Fig. 3 that Pann become noticeably lower and Tjan more regularly higher when cedar percentages increase.

*The obtained climatic reconstructions are not discussed in comparison with previous pollen-based climatic reconstructions available in this region (Cheddadi et al., 2009; Combourieu Nebout et al., 2009). This would have been interesting to note that recon-

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structions based on the MAT from Cheddadi et al. (2009) are not consistent to those presented in this manuscript and try to explain this contrasting pattern. Is it related to the method or to an actual different climate pattern? The authors only state p. 4104: “Besides the low range, both Pann and Tjan show consistent trends. Pann decreases progressively since 6000 cal BP which is in line with the aridity trend that has been observed in other records from the Mediterranean borderlands (Risacher and Fritz, 1992; Brooks, 2006; Hastenrath, 1991; Anderson and Leng, 2004; Umbanhowar et al., 2006).” None of these works deals with records of the Mediterranean region but to Bolivia, tropics, Greenland and USA records.

*The reconstructed increasing trend in precipitation seasonality is also opposite to conclusions of other papers such as Fletcher et al. (2008), dealing with Western Mediterranean pollen records. It is also counterintuitive regarding the orbital forcing which favors a decrease in seasonality along the Holocene. Can hypotheses be proposed for explaining that contrast?

* Rapid climatic changes are presented in the introduction as a characteristic of the Holocene. Could the event at ~ 5 ka be discussed in this context? Does it correspond to some regional changes?

- P. 4105, l.11-15: instead of guessing in a complex sentence that summer rainfall is probably decreasing because annual rainfall is decreasing and seasonality index increasing, why not showing the Psum curve in Fig. 3? -p. 4105, l.21-23: Same, why not showing the Pwin curve in Fig. 3?

6) Minor comments: - The authors repeatedly refer to the “stability” of the Holocene climate (abstract, introduction and discussion). This is somehow clumsy because since more than a decade a large number of works demonstrated that the Holocene climate is far from stable, at orbital and millennial/centennial timescales. And this is also something well known from marine and terrestrial sequences of the Mediterranean region.

- p. 4099, l.6: Holocene climate fluctuations are not only related to “internal variability

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of the climate system”. Major forcing such as volcanic eruption and solar forcing are external to the climate system. - p.4099, l.9: “known” -p.4099, from l. 13 to l.17: what about the influence of the subtropical high and Westerlies on the seasonal climate of the western Mediterranean region? - wrong numbering of the figures, ex. a call to Fig. 4 is missing, and it is should appear in the first paragraph of the “Results” section, i.e. before Fig. 3 first call. - p. 4100, l. 10: delete “years” - p.4103, l.11: “dominating” – dominant - p. 4015, l.13: sentence unclear, “difference” instead of “amplitude”? - p. 4106, l.18: delete “was” - p; 4107, l.3-17: First sentences repeated twice. The last paragraph of the discussion has to be rewritten. It is repetitive and unclear. - p. 4117: TN curve is not shown in Fig. 3 while it is described in the text and mentioned in the figure caption; what is the red line? - p. 4105, l.4: “a SI” - p. 4118: in Figure 4, a curve showing the taxonomic diversity and a hierarchical clustering tree are displayed but not mentioned in the caption. In addition, the method and software used for implementing the hierarchical clustering should be described in the main text or indicated in the figure caption. What is the red line? Is the taxa Quercus corresponds to deciduous or evergreen oak, or both?

Interactive comment on Clim. Past Discuss., 11, 4097, 2015.

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