

Interactive comment on “A Late Glacial to Holocene record of environmental change from Lake Dojran (Macedonia, Greece)” by A. Francke et al.

Anonymous Referee #1

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"A Late Glacial to Holocene record of environmental change from Lake Dojran (Macedonia, Greece)" by Francke et al.

This is a very-well-written manuscript contributing to the understanding of environment and climate development in the Mediterranean region since the Younger Dryas. The authors have used a wide array of generally sedimentological methods in order to identify regional changes in the ecosystem of a lake in Macedonia.

Concerning most aspects, this is a standard example for good manuscripts: The methodology is well-chosen and well-described, the results are well-described as well,
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and the discussion includes almost all relevant works from other authors I am aware of and sets the own results in logical context.

The only aspect that was not dealt with well enough in my opinion is the chronology. While I think that the used age model is probably very close to the truth, the authors should have discussed possible problems in more detail - see below!

The methodology part is a little long for my taste, but other readers may appreciate that each method is described in detail.

One general point to be mentioned is that the conclusions drawn from the investigation of the Lake record do not feature really new results on an over-regional scale. The results "only" show that cold/drought events already known from other Central and Eastern Mediterranean records have also influenced the ecosystem at the lake in Macedonia. However, firstly it is surely not a mistake to have another record in an intermediate position, particularly since other well-known records like Lakes Ohrid, Kopais, Xinias, or Tenaghi Philippon have regional peculiarities that may alter the climate signals seen there (e.g., as mentioned by the authors, different lake depths, or different altitudes or positions relative to mountain ranges and the sea). Secondly, I regard this manuscript to be of high regional importance. For example, the findings for the younger Holocene may be of significant interest for archaeological projects. It would be interesting to know if the Co1260 allows palynomorph analyses to test and complement the results from the lakeshore cores (Athanasiadis et al.)

Following are some remarks to the different sections of the text and to the figures.

1 Introduction

Page 5744, line 20: "... this region can help..." This is too weak in my opinion. You should perhaps bring the point mentioned later (page 5745, line 15), that shallower lakes have advantages, already here and state that such records "are needed".

2 Site descriptions

Page 5746, line 8: "The climate at Lake Dojran is influenced by the..." Lake Dojran would surely be influenced by the Mediterranean Sea in any case (even if the Thessaloniki Plain was replaced with a mountain range). You should perhaps describe how the different aspects you mentioned influence the climate.

Page 5746, line 13: "... leading to warm summers" This sounds like mild winters automatically lead to dry summers.

Page 5746, line 16/17: "... there are... change..." "... there are... changes..."

4.1 Hydro-acoustic survey

Page 5750, line 25: "... reflector 8 (1.3 m) Just for consistency, you should write "... reflector 8 (1.30 m)..."

4.2 Lithostratigraphy and biogeochemistry

In section 4.1, you use m, in 4.2, you use cm. Not really a problem, but inconsistent.

4.4 Chronology

Your results, as discussed later, fit generally well with your used age model. Still, I think that some of your presumptions are too optimistic. E.g., considering that terrestrial plant material and charcoal show now reservoir effect may be problematic. As shown by Pross et al. (2009, Geology), even pollen-grain- and charcoal-based radiocarbon ages can be influenced by a significant hardwater effect. Plants do not only use atmospheric carbon, but also carbon from the lake water if they are close enough. Your own data shows that a plant part from 406.4 m shows a 1300-yr older age than a carbonate shell. I still think that your age model is convincing, but you should refer to possible errors in more detail. See also comment to 5.2!

5.2 Early Holocene

I am wondering if your lithofacies 2b may belong to the YD. This would be possible if you had incorporated some other of your radiocarbon ages in your age model. In

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other records from Greece/the Aegean, the YD reveals a second cooler/dryer phase around 11.8 kyr (e.g., Kotthoff et al., 2011, JQS), similar to the ice core records from Greenland. To me, the changes in your sedimentology record between lithofacies 2b and 3a could rather reveal the rapid changes after the YD revealed in other records.

6 Conclusions

Page 5767, line 9: "The separation..." As written above, I am a little sceptical concerning the length of the YD in your record, but even if your interpretation is correct (probably), the sentence should be re-organized like: "... similarly to the western, contrasting findings from the eastern Mediterranean region and the northern high latitudes, the YD was separated..." or something like that.

References:

I have not found any mistakes in the references.

Figure 1:

Maybe you could show the positions of some other records referred to in your manuscript.

Figure 6:

Compare remarks concerning section 4.4!

Generally, I regard this manuscript as an interesting contribution in a really excellent form (concerning writing, figures, methodology) and recommend acceptance after minor revisions.

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