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Climate of the Past Discussions

Interactive comment on "Hydrological variability in northern Levant over the past 250 ka" *by* F. Gasse et al.

Anonymous Referee #2

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This paper presents the results of an important study of climatic and environmental change over the last two glacial-interglacial cycles from the eastern Mediterranean region. Most of the data are in the process of being published in a series of other papers (Develle et al QSR 2009, Develle et al Palaeo3, 2011; van Campo et al submitted), and this one adds little that is completely new. Its significance is therefore in how these different data sets – pollen, stable isotopes, sedimentology – are integrated. I am not sure that this potential for integration is fully developed in the present manuscript. Nor do I feel comfortable with the way that the study is contextualised and justified, and hence the wider conclusions that can be drawn.

In the Introduction (section 1) the authors set out a palaeoclimatic scenario derived from previous research in the Levant. In this scenario, the southern Levant is pre-

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sented as wet during glacial times, while other more northern regions of the East Mediterranean were dry. The authors' study of a long lacustrine-palustrine sedimentary sequence from the Lebanon is then justified in terms of identifying the transitional hinge line between these two supposedly different climatic domains. In fact, the evidence for this palaeoclimatic scenario is conflicting and contested. For example...

1. Bar-Matthews and co-workers have argued that the speleothem record from the southern Levant should be interpreted as indicating drier, not wetter, climatic conditions during glacial times

2. Neither Rossignol-Strick (QSR 1995, reviewing pollen evidence) nor Robinson et al (QSR 2006) found any evidence for a see-saw relationship between the northern and southern Levant

Fundamental to this debate has been how to interpret the high lake levels of the Dead Sea during glacial phases of the Late Pleistocene in terms of palaeoclimate. As with the lakes of the US Southwest, they could be explained by either a cool-wet "pluvial" climate, or by cold-dry minevaporal conditions. In the US Southwest, packrat midden and pollen data show that woodland cover expanded at the same time as lake levels were high, giving strong support to the cool-wet climatic scenario. In principle the same approach can be adopted in the Levant. However, in no previous study from the Levant spanning a full climatic cycle have isotopic, palaeo-botanical and sedimentological data been available from the same sequence, meaning that mis-matches between them have been explained in terms of dating uncertainty, regional contrasts in climate history, etc... The importance of the Yammouneh study is therefore not its location in the northern (as opposed to the southern) Levant, but the fact that this is the first time that multiple proxy-climate data have been studied together in a single long sedimentary record. Even if there are some uncertainties in precise chronology (see below) we can be sure that pollen, isotope and geochemical analyses from the same sample level are the same age. This permits different palaeoclimatic scenarios to be tested rigorously.

CPD

7, C661–C665, 2011

What the authors show convincingly in this paper is that not all cold stages were the same. In particular, MIS 6 was significantly wetter than MIS 4 and MIS 2. Whereas the former supported conifer woodland and had relatively negative δ18O lake water values, the latter supported chenopod-Artemisia steppe and had more positive δ18O values. So it seems that neither the cool-wet nor the cold-dry palaeoclimatic model is universally applicable.... That said, it is clear that AP% were higher overall during interglacial than during glacial phases, as they are in all other East Mediterranean pollen records, indicating that the climate of the Levant changed in phase, not out of phase, with the rest of the region. In other words, it was not fundamentally different to the rest of the eastern Mediterranean in terms of its climate history.

Detailed comments

1. Introduction: the statement that no previous record from the northern Levant spans more than 21 ka is incorrect. The long Ghab pollen record of Niklewski and van Zeist (1970), although not well-dated, goes back to ca. 50 ka. This sequence might usefully be compared with the pollen results from Yammouneh.

2. Some units, especially unit II, have high Cichoroideae pollen percentages. As the authors point out in 3.2.2, this pollen group resists oxidation and desiccation, and the high values may therefore be an artefact of differential preservation. I think this problem should be mentioned again under section 4.2.4, since it may give a false impression of having been the dominant vegetation during MIS2. It might even be argued that this group should be removed from the pollen sum...

3. Section 3.2.4: I am surprised that among the factors controlling δc no mention is made of evaporation. Although Yammouneh does not appear to be evaporated in its recent history, this possibility is not excluded in the past.

4. Section 4.1: I am a little uncomfortable about tuning palaeoclimatic data to orbital forcing, since this can lead to circular reasoning. In this case, tuning is particularly problematic for the AP peak during MIS3, which has 14C and magnetostratigraphic

CPD

7, C661–C665, 2011

ages of 40-45 ka BP, but is given a tuned age of 30-35 ka BP. The radiative forcing is particularly weak at this time, and may not have had a very significant impact on climate/vegetation. I see no good reason to tune an already well-dated part of the sequence, if it shifts the age by 10ka.

5. Figures

a. Fig. 5: what do the numbers 0 to 12 mean for the AP/NAP ratio?

b. Fig. 7: The Yammouneh δc vs depth would be better as part of Fig. 6, and the stratigraphic column on Fig. 7 could then be removed

c. Fig. 8: the PCA analysis is interesting, but it would also be useful to see x-y plots for individual samples through time. This would also strengthen the data integration aspect of the paper.

d. Fig. 11: it would be good to include the summary pollen data for Urmia on this diagram, since it is also discussed in the text. The author is not far away from CEREGE!

6. In section 4.3.4 it is stated that Yammouneh and the DSB are out of phase. In fact they are in phase, but opposite! (Out of phase implies that they changed at different times...). In this section there is a rather long and – in my view - improbable account of how the climate of two adjacent areas might have changed in opposite directions. At the very least, an alternative explanation should be presented, namely that the hydrological controls for Yammouneh (and Soreq) and the Dead Sea are different. Yammouneh was a small groundwater fed lake lying at 1360 masl; the Dead Sea is large amplifier type lake lying at 425 bmsl, with runoff and evaporation as the main controls over its water level fluctuations. These contrasts may go a long way to explaining the differences between the two records. It is worth noting, in that regard, that there is also abundant evidence of high glacial lake levels in Anatolia and Iran (Konya, Burdur, Van, Urmia, etc) although absolute precipitation values here are assumed to have been relatively low....

CPD

7, C661–C665, 2011

7. The text is well written, but I have made a few minor corrections to the English below.

In summary, the Introduction and parts of the Discussion/Conclusion should to be substantially re-written to include not only the glacial=pluvial scenario for the Levant, but also alternative views. I would also recommend that the authors highlight the multi-proxy approach they have adopted rather than the geographical site location as the key rationale for their study. Instead of the rather negative statement (Conclusions) that Yammouneh is not as well dated as records from Israel, and therefore by implication must be inferior to them, the authors can make a positive statement that Yammouneh represents the first true multi-proxy terrestrial sequence from the Levant that includes both biotic and hydrologic indicators of past climate.

Minor corrections

Abstract: cut first sentence (actually, this could be said of many places, e.g. France!). "time series" not times series

2.1: rainfall events, not rainfalls

2.2: syncline, not synclinal

3.1 a part, not apart; detail (not details) in Develle et al

Use "remains" rather than "rests"; superimposed on (not to) 3.2.5: loses not looses

4.2.5: delete last sentence "Monsoon rainfalls (sic). . .." since it is not relevant

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Interactive comment on Clim. Past Discuss., 7, 1511, 2011.