Response to reviewer 2

We thank Reviewer 2 for his/her time spent on our manuscript, and for the generally constructive comments and suggestions, as well as criticisms. All points arising from review 2 have been carefully considered. We discuss below how they can be integrated in a revised version, or why the reviewer's remarks are not regarded as relevant.

General comment p. 661.

• It is not true that « this paper adds little that is completely new » . The pollen record (Van Campo, unpublished, not « van Campo et al., submitted ») and the long $\delta^{18}O$ profile are new. Fortunately, their interpretation generally agrees with that of the published sedimentary and the 21 ka isotope records. Thus, the conclusions are not completely new, but that does not mean that the paper is fully redundant with previous ones. We will also shorten the summary of published data and highlight new results in a next version.

• We agree that the study is not fully justified in the introduction. Indeed, the integration of pollen, stable isotopes and sedimentology data sets is a significant point of the paper, and our study provides the first multi-proxy record derived from a single long sedimentary sequence in the Levant, combining both biotic and hydrological indicators of past climate. The introduction can easily be reworked, accordingly. In our opinion, the N-S heterogeneity in data coverage remains, however, important to underline.

General comment p. 662.

• We are well aware that the Bar-Matthews et al.' initial interpretation of Soreq and Peqiin Caves conflicts with the Dead Sea level level reconstructions. In the introduction, we did not aim to answer this question (discussed in Section 4) but only summarized some facts. Bar Matthews et al. (2003) clearly showed the good relationship between the isotopic Soreq-Peqiin and EM records, and suggests that rainfall amount is an additional factor. It is what we have said.

• We do not agree to use the Rossignol-Strick (QSR, 1995) paper as an evidence for climate homogeneity between northern and southern Levant. This author postulated a priori that the Mediterranean basin from Sicily to the Levant experienced same climatic fluctuations and modified initial chronology accordingly. She thus could not see difference or diachronism between the Ghab and the Huleh records (the only ones for the Levant, both in northern or northern central Levant). The Robinson et al. (2006) paper is a review and does not bring new data. A better reference for the two last climatic cycles is Cheddadi and Rossignol-Strick (Paleoceanography, 1995) which, unfortunately, only consider marine cores from the southern Levantine basin. This reference will be added in a revised version, together with references of Horowitz (e.g., 1992) and Weinstein-Evron (e.g., Paleorient, 1983) which conflict with Cheddadi and Rossignol-Strick (Paleoceanography, 1995).

• Reviewer 2 wrote « 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 ». We agree, but several Israelian authors clearly claim that high Dead Sea lake levels are related to enhanced precipitation, not to low temperature-induced evaporation rate. We have no solid evidence to contradict this statement. We can only evoke this question.

General comment p. 663

• Reviewer 2 wrote that the climate of northern Levant « was not fundamentally different to the rest of the eastern Mediterranean in terms of its climate history ». This should be discussed. First, it depends which authors are considered. For example, considering pollen only, Bottema (1995) suggested that "palynological development in parts of the EM differ appreciably ", and suggest "that the climatic development in the various parts of the region was not the same". Second, our record suggests that MIS 6 was significantly wetter than the last glacial period, in contrast with what is observed in NE Mediterranean.

Detailed comments

1. We agree, but the Ghab pollen record of Niklewski and van Zeist (1970) is very poorly dated especially before 14 ka. This record is considered for comparison in section 4.

2. We agree with this remark. Some words will be added in the discussion, explaining also that removing the Cichoroideae from the pollen sum does not change the interpretation.

3. We do not understand this comment. Evaporation was taken into account in section 3.2.4. (e.g., p. 1527, « amplified seasonal contrasts, as expected from increased summer insolation, would have increased 18O enrichment of δ_{in} through evaporation…… »).

4. The section tuning palaeoclimatic data to orbital forcing can be cancelled, just saying that further efforts to obtain a better constrained chronology should be done, and although the short wet phases observed at Yammouneh during MIS 3 likely coincide with those recorded in marine core MD 9501.

5a. Fig. 5. Apparently, reviewer did not receive the good version. The AP/NAP ratio has be delayed from Fig. 5 in the version online.
5b. Fig. 7. The text and figure order will be reorganized in a revised version.
5c. Fig. 8. We will try to add this information, also it risks to be redondant with Fig. 8A and with Figs. 5-7.
5d. Fig. 11. No problem to add summary pollen data for Urmia.

p. 664-665.
6. We thought to have explained the differences in the hydrological controls between Yammouneh (and Soreq) and the Dead Sea basin. Apparently, it was unclear. The text will be reworked. Some references to the Turkish lake level/pollen records will be added.

7. We agree that the introduction and parts of the discussion/conclusions were heavy, unclear, and should be re-rewritten. However, we never claimed that glacial=pluvial in the Levant, and rather suggested an alternative view. We will attempt to better highlight the positive (rather than the negative) points of our study in the conclusions.