

Response to reviewer 1

We thank Reviewer 1 for his/her time spent on our manuscript, for some constructive remarks, and some references of recent interesting papers on Israelian karstic and lake systems. In the following we will answer reviewer's comments point by point.

Novelty of the data

Reviewer 1 wrote : " this manuscript is in principle a repetition of the paper of the same group that was published recently »... (Develle et al., 2011) ; « there is no one major conclusion that is new in the submitted manuscript » « why to publish this » etc...

We agree that our manuscript may appear, at a first glance, redundant with previous papers when looking at the first sections only. However, these comments are not acceptable for the following reasons : first, the pollen record and the long $\delta^{18}\text{O}$ profile were not published before. Pollen data provide the first long terrestrial vegetation record derived from a single core in the Levant. However, no single proxy is univocal and the best way to understand the paleoclimatic-paleohydrological evolution in a given basin is thus to cross-check interpretations from several proxies from a single sedimentary sequence. To our knowledge, an approach combining biotic, isotope and sedimentary data sets (as proposed here) was never performed in the Levant for long-term sequences ; second, new conclusions arise from our data : (i) the successive interglacial peaks reflect different environmental conditions ; (ii) the penultimate interglacial (MIS 6) was much wetter than the last glacial period (and possibly that MIS 5.5) ; (iii) there is an overall trend toward increased aridity over the two last climatic cycles ; (iv) temperature is certainly an important controlling factor on our $\delta^{18}\text{O}$ signal in addition to the « source effect ».

In general, our interpretations suggest that all these environmental conditions is an interplay of several control factors (temperature, P amounts, evaporation, seasonal distribution...) as emphasised by the reviewer.

Age control

We agree that more caution is due when correlating our record with EM sapropels, Peqiin, Soreq and Dead Sea records due to uncertainties on our age model (uncertainties that have been widely discussed in the paper of Develle et al., 2011). We should more clearly say that our rationale is the characterization of successive interglacial peaks and full glacial periods, and that these correlations are proposed as hypotheses - not as statements- to leave the door open to potential alternative interpretations. The section proposing a revised time scale based on orbital tuning would be cancelled in a revised version. Although such a tuning is commonly done, it can only be regarded as an hypothesis. Note, however, that the long-term pollen records from the Hulah-Dead Sea basin, commonly used as an evidence for the Dead Sea basin climatic evolution, were tuned with EM marine isotope records by postulating wet glacial phases and dry interglacials (e.g., Horowitz, A., 1979. The Quaternary of Israel. Academic Press, New York, 394 pp.??). No absolute date is available yet for Hulah (Weinstein-Evron et al., 2001), and the marine pollen records from the Levantine basin suggest the opposite (e.g. Cheddadi et Rossignol-Strick, 1995; Langgut, 2008, 2009).

Other Reviewer 1 comments were previously addressed to the Develle et al.' manuscript (published in 2011) and are dealing with chronology, sediment components and sedimentary processes only. These comments have been taken into account in Develle et al. (2011). Obviously, the interpretation suggested by Reviewer 1 (cold wet glacials vs. warm dry interglacials) would well agree with that of data from the Dead Sea basin. However, at Yammouneh, we show that the successive glacial or interglacial phases are different. The scenario proposed by Reviewer 1 is applicable to the relatively wet penultimate glacial period, but not to the last Glacial period. At that time, cooling and low CO_2 atmospheric

concentration may have partly inhibited tree growth, but the development of a steppic landscape, the absence of aquatic organisms and the occurrence of many oxidized layers imply low local water availability. We did not conclude that precipitation was drastically reduced, but that water could have been stored in glaciers and permafrost.

P-E characterisation

Yes, we use the term P-E. A priori, changes in P, E or both may have acted on the local water balance. In contrast with the Dead Sea studied since decades, the Yammouneh site is investigated since a couple of years only, and no modelling of the Yammouneh system has been performed yet. Let us remind that there is abundant evidence of high glacial lake levels in Anatolia and Iran (e.g., Konya, Van, Urmia...) although pollen-inferred absolute precipitation values here are assumed to have been relatively low (e.g., Roberts et al., 1999, Djamali et al., 2008). We know that long-term drastic changes the Dead Sea basin lake levels are attributed to drastic changes in P (e.g., Kolodny et al., 2005, among others), but Shafer and Alpert (2010) also show that local temperature and related evaporation rates are crucial factors controlling the Dead Sea water balance in present times. At Yammouneh, we tentatively interpret the $d^{18}O_{\text{carbonates}}$ values to understand the relative role of P and E on the system (section 3.2.4). Our approach suggests that simultaneous changes in P and E associated with changes in seasonal thermal and rainfall contrasts may explain the signal. We are happy to see that our hypothesis of changes in rainfall seasonal distribution and length of the wet season is supported by the recharge model of the modern Western Mountain Aquifer of Israel (Sheffer et al., 2010; thank you for the reference).

General comments about our data interpretation

Reviewer 1 evokes the alluvial fan formation as indicative of wet glacial conditions. We have discussed with authors (M. Daëron and L. Benedetti) of the paper by Daëron et al. (EPSL, 2004) who have studied and dated (Cl-36) the Yammoûneh alluvial fans. According to them, the ages of these fans, which formed during interglacial and glacial periods, reflect disequilibrium of the steep slopes due to paleoseisms, not climate.

We do not feel reasonable to apply conclusions of the excellent studies on aragonite precipitation in the Dead Sea to the small Yammouneh lake. The Yammouneh paleolake remained fresh and shallow, and was certainly never meromictic in contrast with the glacial Lake Lisan (Barkan et al., 2001). Develle et al. (2011) have showed, by comparison of isotopic values of lake sediment and bedrock carbonates, that most calcite precipitated in the waterbody during interglacials and was mainly detrital during glacials. Gelifraction and open vegetation cover certainly favoured the bedrock physical erosion during the last glacial phase, when sediments are mainly composed of detrital carbonate and windblown material. If this is correct, sedimentation rate was possibly lower during dry phases. This was suggested for the past 21 ka; Develle et al., 2010), but we are not able at that stage to confirm this hypothesis for longer time scale.

Our results may appear as a re-opening of the major dispute between the Dead Sea and the speleothem (from western Israel) communities, and with the marine pollen community which documents relatively wet/dry interglacial/glacial periods. In our mind, it may be not so controversial when considering the great geomorphological and hydrological differences between a small mountainous groundwater-fed lake and a large amplifier lake below 400 mbsl with runoff (and P) and evaporation as major controls over the water level fluctuations. In a region with drastic environmental changes on very short distances, local factors may generate significant differences in the behavior of individual eco- and hydrosystems.

To conclude, we propose to: (i) re-write the introduction and conclusions showing the interest of a multi-proxy approach as adopted here; (ii) shorten the sections dealing with already published data/results and focus on the new results, and (iii) modulate the record interpretation by discussing alternative scenarios and better explaining our opinions that we maintain. Any way, whatever the interpretations, which of course could be refined by further analyses and/or other multiproxy records from the region, data are there. We consider that they should be delivered to our scientific community.