Gasse et al. "Hydrological variability in northern Levant over the past 250 ka”

This manuscript is in principle a repetition of the paper by the same group that was published very recently: “A 250 ka sedimentary record from a small karstic lake in the Northern Levant (Yammoûneh, Lebanon): Paleoclimatic implications (Develle, Gasse, et al 2010a. Palaeogeography, Paleoclimatology, Palaeoecology v. 305 p. 10-27). An even earlier paper last year by Develle et al., (2010b) in Quaternary Science Review discussed similar proxies of last 21 ka of the same Yammouneh core. This 2010b paper also includes a discussion of the Oxygen isotopes that is only marginal in the 2010a paper but somewhat expanded here. I must say that in my opinion there is no one major conclusion that is new to this submitted manuscript.

I was a reviewer of Develle et al. (2010b) and provided significant comments but with a recommendation for publication based on the presentation of new data. Now the data are out and here we have another manuscript. I must say that there are some additional data in the current manuscript but they do not provide clearer insights or directions how to interpret the Yammouneh core better in term of paleohydrology or paleoclimatology. I truly would have hoped that rather than repeating the same data and ideas the authors would have taken their time to provide us with all the alternative explanations rather than propagating similar aspects. For example, in such an environment, the record is an interplay of temperature (including freezing) and hydrometeorology (P amounts, seasonal distribution, availability for runoff and streams, springs and karst, etc. that changed with time, elevation, etc.). The simple explanation used in Develle et al is good for one time. I think a wider view of possibilities is due. Otherwise, why to publish this.

I decided to present my earlier comments and strengthen them where due. (**The earlier comments are marked in bald**).

A) <u>Age control</u>: **I found it a little disturbing that with the** grave **problems associated with your age model and the unlikely constant sedimentation rates (which you honestly stated yourself** in Develle et al 2010b **), you make** such strong **assertion of correlation with EM sapropels, high resolution records Pkiin and Soreq caves,** Dead Sea and the rest of records **. I think caution is due.** Now that I see how this is propagated further into the literature, I must stress that you need to raise the problems more and be much more cautious. This is actually shown by the changes you made here in the magnetostratigraphy. Such fitting and the tuning you mention are far from perfect. This problem is large: In your reassessment of your age model (section 4.1), you stress up to ~10 ka! (11, 8.9 ka) differences; and this is AFTER tuning. Unfortunately, in the tuning and the interpolation of ages and the extrapolation out of the 124 ka U-series age you incorporated your bias!

This point weakens the entire manuscript (I am aware this is a common problem but once published it should not become a fact). Again, when I balanced it with the new data presented earlier, I recommended publication; now I feel it is too much manipulation of paleoclimatology with little additional data.

B) **I have a few comments on the basic principles/rules they chose to use in interpreting the data. Below I provide comments and in addition, alternative interpretations based on the authors' results. I am thinking of the future possible**

misuse of their interpretation by other researchers in the region and elsewhere. It is very difficult to future researcher to raise alternative ideas and therefore, I think that at least the authors should consider these proposed alternatives.
Now, I see that the authors themselves propagate what can be considered only one interpretation of a few alternative ones. i.e. their own data is not narrowing enough the paleohydrologic/paleoenvironmental interpretation and then their choice of only one interpretation (with nothing new in it), when put in a regional Levant and EM paleoclimatic picture, is a simple propagation of thoughts not of rigorous results.

1) **A different interpretation**: **Let us assume, based on your data a different but very reasonable** (at least one) **scenario:**
(a) Peak interglacial are warmer, allowing more AP vegetation, less stormy and therefore less flood [but still, in this area of 600-1200 mm a year of precipitation in the current interglacial there are very active aquifer recharge and Karst (e.g., Dafny et al. 2010, J. Hydrology 389**:** 260-275**;** Sheffer et al. 2010: Water Resources Res. 46 Article # W05510) and springs that develope carbonate deposits as in other EM areas, where the karst is most developed where snow and seasonal freezing are more common!) **. As a result the fine grained deposits are maintained in the soils on slopes and in the drainage basin and are not washed into the lake and therefore do not blur the carbonates that are always at the background (either because of soil respiration under vegetation or more CO2 dissolved in the water under colder T- but obviously root zone respiration is more important). (This is more the scenario for early Holocene and not today as deforestation since early Holocene altered the vegetation composition probably dramatically). Therefore, there are whitish carbonate marls as discharge deposits with isotopic composition indicating evaporation!**
(b) In glacial times, much colder, less vegetation cover (Steppe or low temperature surviving trees such as Juniper?**) with increased dust transport and Terra Rosa formation (as in the rest of the E Med dust-induced soil formation), more sediment is washed under more storms and snow melt, surface runoff increase as well as spring discharge increase; then the alluvial fans are formed and thick detrital deposits fill the basin. You cannot have erosion and transport of sediments (as evident by the thicj clastic fill) without increased storms/runoff/floods. You make it windier and more dust transport but (as I suggested above) the dry dust fall cannot fill your basin! You have to get material from slopes and they arrive only by surface runoff (unlike the interglacials!). Moreover, in today's or Holocene Hermon springs there is enough pure carbonate to be deposited in water bodies.**
i.e., in this interpretation the clay silts indicate flowing surface water (many more rainstorms/ and snowmelt in late spring**) and colder climates. And the pure carbonate marls are warmer** and with similar **temp as today without clastic sediments reaching the basin.**
2) **The above two suggestions can make your work in better agreement with what was proposed to N. and C Israel (according to the Dead Sea lakes-** probably the better rain gauge in the area**), the cold wet glacial but drier interglacials, with early Holocene wetter than today). It may make a better sense as I see that you have struggled with it because of the short distances between Dead Sea headwaters** in Mt Hermon **and Yammuneh.**

3) You use the term P-E, which frequently used in the literature where (and mostly) we have no idea on either parameter, although the reality is that range of changes in E is usually smaller than the potential changes in P). In the recent Dead Sea literature, you cannot find the use of P-E**; surely not with either increased P or reduced E as equivalent alternatives. This is mainly because of a simple and basic necessary condition that is independent of this ratio: There is a basic need of a huge increased HCO_3^- input to the lake for the carbonate mass balance** (Barkan et al, 2001. Geochimica et Cosmochimica Acta 65,355–368). This needed six-fold increase in HCO_3^- input **can be achieved only by increased water input and NOT by reduced evaporation!) - it is not the simplistic game that we all play with lakes- there are other limitations on the various budgets- water budget is not everything! However, even the water budgets indicate drastically increased P to a level that cannot be equaled by reduced evaporation (see Kolodney et al., 2005; Enzel et al., 2008). And yes, Lebanon dry when Dead Sea headwaters are wet and discharging is a problematic scenario.**

This is the major dispute with the speleothems community, who gave equal importance to reduced evaporation. Your earlier paper (Develle et al., 2010b**) followed the problem of source vs. quantity (see Frimkin et al., 1999?) as first and second order) in interpreting oxygen isotope records in the region. I think you were correct there. That means: why are you returning to the original interpretation of rain amount from speleothms?** Remember, that only during glacial times **speleothems formed in Vaks record in central Israel.**