Reply to anonymous Referee #1

Reviewer #1's original comments are in black and our replies are given in blue.

The authors present high-resolution sedimentological (microfacies) and geochemical data records for three upper bathyal sediment cores from high-accumulation sites off-shore the western distributaries of the Nile River delta. The sediment cores encompass the Holocene period including laminated sediments, which have been deposited during the last stagnation period of sapropel S1 in the Eastern Mediterranean Sea (EMS). The temporal resolution of the study region is unprecedented and allows for resolving even seasonal changes and thus the characterization of changes in the strength of the yearly Nile flood and associated regional environmental conditions at the core sites. In this reconstruction, the application of microfacies analyses, previously established for laminated lake sediments provides essentially novel information on the paleohydrology of the Nile River and related summer floods. The manuscript is generally well designed and follows a widely comprehensive argumentation.

Thank you for these supportive comments.

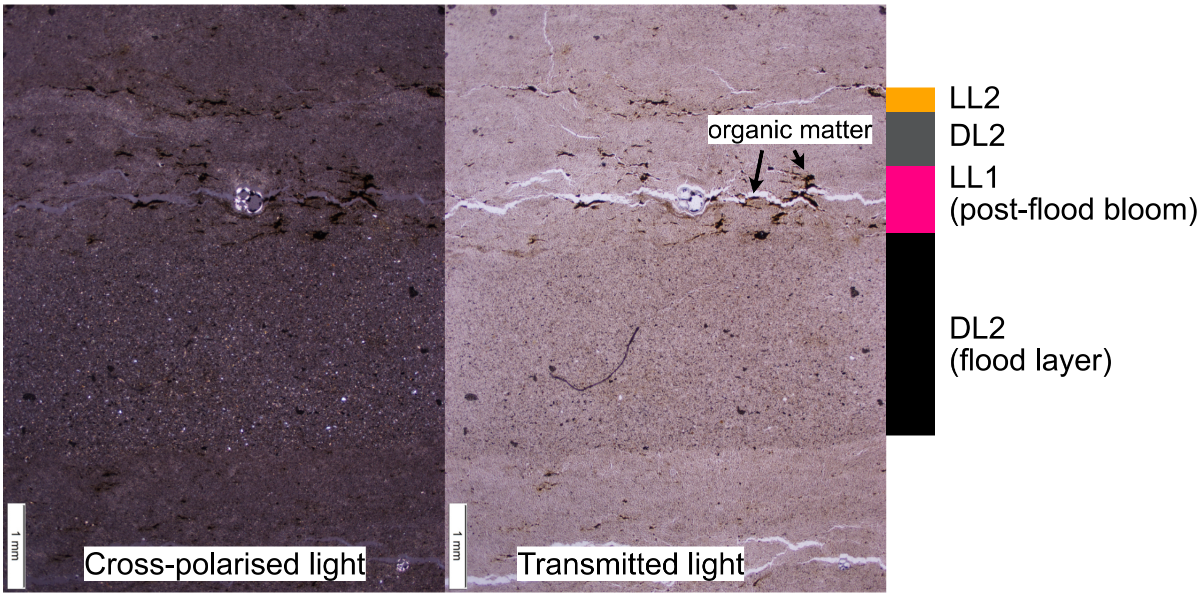
Nevertheless, the following issues should be considered in the revision process:

1) I disagree with the general conclusion concerning the role of productivity as a major driver in S1 formation in the entire EMS basin. Enhanced productivity likely plays an important role during S1 deposition in the vicinity of the Nile delta and in the eastward flowing surface water current along the Israeli coast. Eutrophication of surface waters has been documented in sediments retrieved from the Israeli continental margin. However, evidence from abyssal and bathyal core sites from the central parts of the basins and from the northern margins of the EMS provide a controversial picture, in many cases lacking evidence for eutrophication during S1 formation. In this context, the manuscript would profit from a more critical discussion of preservation issues with respect to the interpretation of geochemical proxies since syn- or post-depositional degradation of organic compounds may play a significant role. For example, the study of Moebius et al. (2010, Biogeosciences 7, 3901-3914) suggested that "... preservation plays a major role for the accumulation of organic-rich sediments casting doubt on the need of enhanced primary production for sapropel formation." In addition, the study of Grimm et al. (2015) used a sophisticated regional ocean circulation model coupled to a biogeochemical model to explore the potential role of productivity and associated organic carbon fluxes in S1 formation (this is exactly what you ask for at the end of your conclusions!). They found that the required oxygen depletion is mainly depending on the stratification intensity and stagnation history of the water column and that a basin-wide productivity increase is not required as a prerequisite for sapropel formation. These and other results suggest that the observations of upper bathyal anoxia off the Nile delta may not be representative for environmental changes in the entire basin but may rather represent a regional situation due to its vicinity to the Nile nutrient source. This suggestion is also confirmed by the restriction of sustained basin-wide early Holocene anoxia to water depths below approximately 1500 m (De Lange et al.,2008, Nature Geoscience, 1: 606–610) while sustained anoxia seem to have prevailed at shallower water depths under the direct influence of Nile outflow waters. In the revision of the manuscript, the authors should acknowledge the still controversial findings and diverse evidence from both proxy and model applications. In doing so, I suggest to abandon conclusions drawn for the entire EMS (including Figure 10) and rather focus on the Nile river dynamics and regional environmental impacts. These results are novel enough and provide a variety of essentially new insights into past hydrological changes and marine depositional responses.

We thank Reviewer #1 for this crucial comment. The interplay between productivity, preservation and stagnation is a complex issue and we acknowledge that it is probably wiser to draw conclusions for the environments “closer to home”, i.e., directly under the influence of the Nile sediment plume. In the revision of the manuscript, we will include a clearer discussion of preservation issues and the role of seawater stagnation. We will also discuss our findings as being relevant for the Nile deep-sea fan and Israeli coast and remove Figure 10, as requested.
However, please note that our point here was not to refute the role of long-term stagnation on the development of basin-scale anoxia (indeed very clearly demonstrated by model results of Grimm et al. (2015) and backed by proxy-data of Cornuault et al. (2018) -both repeatedly cited in the manuscript), but rather to discuss the additional role played by variations in primary productivity on shorter time scales. That enhanced productivity (and resulting eutrophication) might not be sufficient to explain deoxygenation is clear, but the role of changes in productivity and freshwater release on centennial-scale changes in oxygenation has not been explored by Grimm et al. (2015) (who attributed shorter time scale ventilation events to cold events producing denser waters). Last but not least, the model of Grimm et al. (2015) does not lead to the development of anoxia in water masses above 1800 m in the Levantine Sea (nor in the Ionian Sea), the occurrence of which has, however, been demonstrated by our data and other studies. Hosing experiment by Vadsaria et al (2019) recently challenged the results of Grimm et al. (2015) concerning the role of freshwater release by the Nile at higher resolution but their model also does not explore the consequences of transient changes in water-mass structure, freshwater and nutrient release for centennial-scale changes in oxygenation. At present, there is no model able to look at this succession of events in a more dynamic manner.
Organic matter preservation is indeed responsible for parts of the variability in our record (e.g., lycopane is a purely preservation-driven record), but cannot account for the changes in planktonic foraminifera accumulation rates observed on the Israeli Coast, redrawn in Fig. 9b (Mojtahid et al., 2015).
We will take into account this important comment and focus our revised manuscript on building a picture of changes occurring along the path of the Nile sediment plume. If we are able to reconstruct past oxygenation dynamics for the Nile plume, the question remains indeed to what extent these processes have influenced other locations in the Levantine Basin.

2) You argue that the Nile flood triggered blooms of planktonic foraminifera and calcareous nannoplankton in autumn, similar to natural Nile blooms prior to the construction of the Aswan dam. Typically, eutrophication in surface waters influenced by riverine nutrient input results in diatom blooms as described for the historical Nile floods (Halim et al., 1967). To date, Halim et al. (1967) do not mention planktonic foraminifera and coccolithophorids as typical groups responding to the seasonal nutrient input. In the modern EMS, planktonic foraminifera and coccolithophorids are widely distributed and even thrive in the ultraoligotrophic parts of the basin as in many other oligotrophic oceans. Did you observe any layers rich in opal? In the EMS, opal remains are often not pre-served, which may inhibit documentation of the proper succession of nutrient-driven phyto- and zooplankton association in the sediment. Your discussion and interpretation should be more specific here, acknowledging a correct assessment of plankton response to river-induced surface water eutrophication.

Reviewer #1 is correct: Halim et al. (1967) did not specifically report carbonate zooplankton for historical floods of the Nile. We will revise this part to be more precise, acknowledge assumptions and provide additional information. Unfortunately, we did not observe any diatoms or opal in the sediments, which, as Reviewer #1 said, probably results from poor opal preservation. However, the deposition of foraminifera and coccoliths layers is associated with distinct layers of organic matter (see image below as an example) and we therefore assume that they represent post-flood layers following a fertilisation process similar to that occurring during the Nile blooms described by Halim et al. (1967).



3) The presence of benthic foraminifera (BF) in the interruption and upper part of S1clearly indicates the absence of permanent anoxic conditions and at least intermittently oxygenated time periods at the deepest core site (738 m water depth) since around8200 years B.P. It is a pity that no BF data are available for the other two cores. On the other hand, the presented data on the genus level do not provide sufficient insights into the BF fauna and thus benthic ecosystem state since different species of the same genus often have contrasting ecology. In addition, proper taxonomy is required for excluding the potential effects of down-slope transport. The authors mention the genus Cibbides(?). I guess you refer to the genus Cibicides or Cibicidoides, do you? Species of the genus Cibicides commonly inhabit shelf environments in the EMS and thus would represent reworked tests when found on the continental slope. On the other hand, Cibicidoides is an autochthonous deep-sea taxon. In order to avoid misinterpretations, it would be more honest to lump all species together and present data on the BF presence/absence or concentration (e.g. individuals per g dry sediment).

Thanks for this comment and spotting this mistake. Indeed, there was a typo in the text: we have identified specimens of Cibicidoides. We agree that a full investigation of benthic foraminifera would be extremely valuable for these cores to get a more precise picture of bottom-water environments during Sapropel S1 at the Nile mouth. As suggested by Reviewer #1, we will combine the specimens of different genera and show the BF data as individuals/g sediment.

4) I wonder if you have observed any gypsum crystals in the sediment. Post-depositional oxidation of iron sulfides and calcium carbonate often result in precipitation of gypsum in sapropelic sediments.

No gypsum was observed in this core, which might be due to the relatively low Corg content (±1%) compared to other sapropel layers (our cores rather represent sapropelites *sensu stricto*).

5) Some of the figures appear too busy and the blueish colors used do not contrast sufficiently. This is particularly visible in figures 6, 8 and 9. I suggest using more contrasting colors and also avoid overlying symbols and lines.

The figures, esp. the colour codes, will be modified in order to improve readability.

I hope that my suggestions prove useful for the revision of the manuscript!

Yes, they are, thank you very much!