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Climate of the Past
Discussions

Interactive comment on "Deoxygenation dynamics above the western Nile deep-sea fan during sapropel S1 at seasonal to millennial time-scales" *by* Cécile L. Blanchet et al.

Anonymous Referee #1

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The authors present high-resolution sedimentological (microfacies) and geochemical data records for three upper bathyal sediment cores from high-accumulation sites offshore 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. 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.

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 preserved, 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.

3) The presence of benthic foraminifera (BF) in the interruption and upper part of S1 clearly indicates the absence of permanent anoxic conditions and at least intermittently oxygenated time periods at the deepest core site (738 m water depth) since around 8200 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).

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.

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.

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

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