

## Reply to anonymous Referee #2

Reviewer #2's original comments are in black and our replies are given in blue.

Applying multiproxy approach (micro-facies analysis with inorganic and organic geo-chemistry) to three cores of high sedimentation rates from the Nile deep-sea fan, Blanchet et al. reconstructed seasonal to millennial-scale variability of detrital inputs, biological productivity and bottom/pore water oxygenation states during the Holocene covering sapropel S1 deposition period. The highly resolved records provide very useful information on the impact of Nile flooding as both freshwater and nutrient supplier. The quality of data is high and scientific subject fits well the field covered in *Climate of the Past*. I, however, have several concerns that should be solved during the revision process. I develop the points below.

Thank you for this positive assessment.

1. Local vs. regional impact of Nile discharge: circulation and productivity The most innovative results of the study are (i) annual cycle of sediment deposition during S1 period in the western Nile deep-sea fan and (ii) additional evidence for heterogeneous bottom/pore water oxygenation states in the coastal regions during S1. Point (i) is well described whereas the transition from (i) to (ii) is abrupt and some key points of (ii) are missing. The most exciting but not fully addressed subject is how the identified annual cycle of Nile discharge and the productivity changes was interacted with basin-scale water circulation and oxygenation states. The authors briefly proposed the impact of nutrient supply by Nile river discharge in relation to the mega-summer monsoon of the AHP by analog with "Nile bloom". However, there exists a growing body of evidences for the leading role of stagnant circulation that pre-conditioned the S1 deposition based on proxy reconstruction and a numerical modeling (Grimm et al., 2015). Since the authors provided unprecedented resolution data, it could be possible to revisit the role of the Nile at finer timescale. For instance, the authors proposed that fine-grained clay-rich particle deposition in laminated layers as a sign of stagnant circulation. How the stagnant circulation was realized and what was the relationship between the observed slower circulation in the Nile deep-sea fan with the ventilation of the other parts of the Levantine Sea? In general, the present manuscript did not sufficiently describe the role of the circulation to the sapropel formation. The present-day circulation pattern is very shortly shown in Section 2 (Regional context) and there is no statement about water mass occupies the three core sites and how the mass is oxygenated under the present condition. This information should be added. Besides, the sketches of an annual cycle shown in Fig. 5c are difficult to understand. What is the size of geographical extension of area for the proposed sedimentation processes?

We thank Reviewer #2 for this important comment. In the revised manuscript, we will provide more information on the present-day water-mass structure to the best of our knowledge. In addition, we will review the state-of-the-art on sea water circulation during sapropel deposition. Reviewer #1 suggested to focus on deriving paleoenvironmental information from archives located along the path of the Nile sediment plume, i.e., the Nile deep-sea fan and Israeli coast. We will therefore use our new records to draw a finer picture of seasonal variations on the Nile deep-sea fan and improve the sketches of Fig. 5c (with a better scaled representation). The interesting point made by Reviewer #2 about the possibility to derive further-reaching interpretations about water-mass structure and circulation from our seasonal record will be explored. Generally speaking, we will aim to better exploit the new results provided by the seasonal record.

To clarify the local vs. regional impact of Nile discharge, I would suggest reorganize section 5.3. Do the titles indicated on lines 574, 601 and 653 correspond to the subsection of 5.3, thus the section

extends for 7 pages? It is better to start by coastal regions (Nile deep sea fan and Israeli coast) and extend the discussion to the Levantine basin. The authors should be careful about the terms of geographic definition. For instance, the whole Eastern Mediterranean Sea, including the Adriatic Sea and Ionian Sea, is not treated in the present manuscript, therefore it is inappropriate to use this term in the section title.

Section 5.3, as well as other parts of the discussion, will be restructured following comments by both reviewers and we will carefully harmonize and correct the terminology (i.e., Levantine Basin instead of eastern Mediterranean Sea).

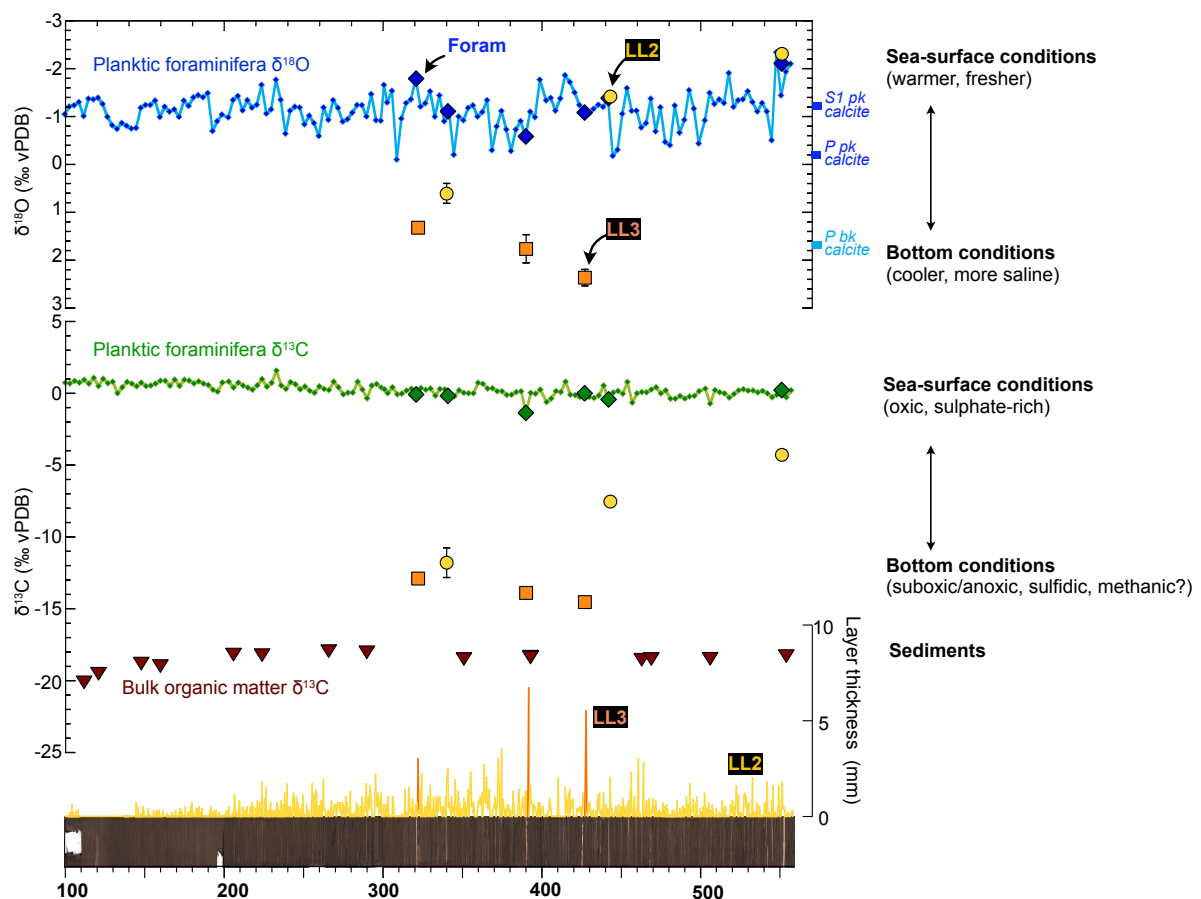
2. Do the reconstructions indicate bottom or pore water oxygenation conditions? It is necessary to clarify whether the geochemical signals represent bottom or pore water conditions. Based on the low  $\delta^{13}\text{C}$  values of authigenic carbonates from the HL layers during S1 deposition period, the authors proposed anoxic bottom water condition at site P33 (740 m water depth) and possibly at site P73 (570 m water depth). These sites are much shallower than the previously reported anoxic sites during S1 (1000 m and 2000m) based on authigenic carbonates (Aloisi et al., 2002; Bayon et al., 2013). It is not clear what the authors observed is pore water or water column oxygenation state. It is possible that the reconstructed oxygenation state was very localized with patchy distribution that is not suitable to generalize the whole basin.

About  $\delta^{18}\text{O}$  values of LL2 carbonates, the authors proposed that “temperature and salinity gradients were homogenous throughout the water column”. Does it mean that no density gradient existed during authigenic carbonate precipitation in 740 m water column? Is the hypothesis of the homogeneous water column consistent with oxygen depleted bottom water? If so, why the shallowest core (P99) from 400 m water depth showed less lamination despite and higher or comparable alkenone flux than at the deeper core (P33, Fig. 7)?

The point raised here by Reviewer #2 is an important one indeed. We propose here that the formation of the authigenic calcite layers results from processes occurring at the seafloor or at the sediment-water interface and reflects bottom-water chemistry since similar layers with an identical seasonal succession were found at locations 100 km apart from each other. These thin and distinct carbonate layers formed regularly in the sequence and are interlayered between other seasonal deposits, without any disturbance of the sedimentary fabric (esp. the soft LL2 layers). We therefore suggest that these carbonates formed during seasonally-occurring bottom-water supersaturation states. We ruled out a diagenetic origin of these layers resulting from chemical processes occurring deeper in the sediment column (i.e., with an isotopic signal reflecting pore-water chemistry) since such processes are often associated with local non-steady state diagenesis and are generally not found at nearby core sites. Only larger regional sedimentary disturbances (such as a turbidites or slumps) have been shown to lead to the formation of diagenetic carbonates (often deeper in the sediment column close to the sulfate-methane transition zone) that were found at several core sites (Kasten and Jørgensen, 2006). The precipitation of authigenic carbonates at cold seeps located deeper on the Nile DSF (Aloisi et al., 2002; Bayon et al., 2013) shows that anoxic conditions favoured carbonate precipitation over oxidation and degassing of  $\text{CO}_2$ . Not mentioned in the paper, personal conversations with Dr. M. Revel about core MS27PT suggest that the thin seasonal carbonate layers were found at deeper sites MS27PT (1500m). We, however, cannot transfer our geochemical reconstruction to the entire basin because the observed layers may indeed only reflect local chemical conditions occurring under the influence of the Nile runoff. However, they provide a window of opportunity to assess seasonal changes in water chemistry based on sediments in which classical markers (benthic fauna) are missing. Future work will be focused on carrying out a systematic isotopic analysis of these sub-mm layers.

Concerning the second part of the comment about  $\delta^{18}\text{O}$  values and water-mass structure: It is very difficult to interpret the pilot  $\delta^{18}\text{O}$  values on authigenic calcite layers because we do not have any

constraints on bottom water d18O values or water temperatures during S1 sapropel deposition. However, we will add the figure below to the revised version of the manuscript in order to better visualise the changes in water-mass structure derived from our data. We can compare the biogenic and authigenic (inorganic) calcite isotopes because the offset is generally on the order of 0.1 permil (Bemis et al. 1998) and therefore much lower than the variations observed. This new figure shows that for some LL2 layers deposited in the older parts of the sapropel, the d18O is very similar to that of planktonic foraminifera, whereas LL3 layers show a d18O value similar to present-day benthic foraminifera. This suggests drastic reorganisation of the water-mass structure in the course of sapropel deposition. Our data indicate that at the beginning of S1, the water-column was largely influenced by isotopically lighter water, potentially deriving from large Nile freshwater input (with a dominant Blue Nile signature). With these few pilot samples, it is impossible to derive any detailed conclusions regarding water-mass structure and deoxygenation. Regarding P99, the depth of this core is closer to the mixed layer and therefore likely receives larger supply of oxygen from overlying water-masses. Therefore, anoxia would not develop even with a large carbon input.



At last, I have a comment on the general structure of the manuscript. The authors mixed result presentation and some interpretation in section 4 then more detailed discussion followed in section 5. Personally, I prefer to separate result and discussion to avoid mixed objective and subjective descriptions and redundant statements. I understand that this organization is due to multi-proxy reconstructions that necessitate to explain the meaning of numerous proxies. To overcome this complication, the authors may present the tool box classified into target variables before the result section.

Thank you for this comment. This was a point of discussion between co-authors as well for the reasons mentioned by the reviewer (mostly readability issues). We will follow the judicious advice of Reviewer #2 in the revised manuscript and restructure to separate results and interpretation. We will also provide a reading grid for the proxies.

I recommend to accept this work after major revision.

Minor / specific comments

Line 28 “the entire Levantine Basin”. See my comment 1.

Will be modified accordingly.

Line 48 “decreased” should be replaced by “increased”.

Done

Lines 51-52. “ the consequently low or quasi-absent primary productivity (Krom et al.,2014)”. Delete “or quasi-absent”. Pujo-Pay et al. (2011) is probably more appropriate to cite.

Will be modified accordingly

Line 59. “the 1980’s” should be “the 1980s”.

Will be modified accordingly

Line 67. Somot et al., 2018. This paper treats the circulation in the NW Mediterranean Sea, not in the Eastern Mediterranean Sea. Adolff et al. (2015) is more suitable to this sentence because the circulation of the whole Mediterranean Sea is studied.

Will be modified accordingly

Line 72. “Sapropels have proven a valuable laboratory”. This sentence is strange and should be revised.

This sentence will be modified accordingly.

Line 186. “Aavatech” should be “Avaatech”.

Thank you for spotting the mistake, it will be corrected.

Line 218. “trialkyl” or “dialkyl”?

The C46 is a trialkyl tetraether lipid and so trialkyl is correct, however the acronym following this should read GTGT. This is now corrected.

Lines 204-212 and Fig. 4. Indicate the core name of which stable isotope data were obtained.

All stable isotope data were obtained on core P33 and this information will be added to section 3.6, table S4 and Fig. 4.

Lines 282-284. The occurrence of HL in both cores P33 and P73. Is the appearance of HL synchronous for the two cores to support the hypothesis of reduced oxygenation state at intermediate water depths?

This is a good point but unfortunately impossible to answer at present. We plan to count and measure layer thickness of core P73, too, which will help to align records and assess the synchronicity of these layers. Radiocarbon dating on such rapidly accumulating sediments is unfortunately too imprecise to tackle this question.

Line 367. Replace “contents” by “fluxes”.

Will be modified accordingly.

Line 375. Add the corresponding water depths of the indicated cores after “deeper cores MS27PT and GeoB7702-3”.

Indeed this information will be added.

Line 385. Add “variability” after “runoff”.

Will be modified accordingly.

Line 400 and Fig. 8a,e,h. Why Ti-enrichment occurred at the Late Holocene?

This is a good question that has intrigued the authors for a while. A potential response to this question was provided in Blanchet et al. EPSL 2013. It is related to the fact that Ti/Ca traces relative variations between terrigenous and marine-derived sediment inputs and not purely terrigenous fluxes. So higher Ti/Ca ratios during the Late Holocene indicate higher terrigenous versus marine-derived sediment input, which was probably related to a re-establishment of the annual Nile floods after a very dry period at ca. 4 ka BP. However, the sediment input linked to monsoonal runoff during the Late Holocene was distinct from the situation during the early Holocene with regard to sediment budgets (sedimentation rates differing by two orders of magnitude – see Fig. 2). So, both periods are dominated by fluvial sediment input but the early Holocene was a period of massive fluvial erosion (and the core site was a depocenter for fluvial sediments).

Line 405. Add “ka BP” after “7.2”.

Will be modified accordingly.

Line 406. “Ti/K” should be “K/Ti”.

Will be modified accordingly.

Line 409 and Fig. 8. Indicate the sapropel interval in Fig. 8.

We will re-design Fig. 8 and add an indication of the sapropel layer.

Lines 436-438 and Fig. 3c. It is not clear that smectite, plagioclase and iron-titanium oxides are more abundant in DL2 relative to the other layers. According to the figure caption, Fe-rich phases are probably pyrite, not iron-titanium oxides.

The presence of Fe-Ti oxides is shown in Fig. 3b and if indeed pyrite represents a large fraction of the Fe-rich phases, this iron sulphide cannot account for the Ti-rich phases. Smectite and plagioclase (and to a smaller extent Fe-oxides) are found in all sediments analysed (albeit sometimes blurred by high CaCO<sub>3</sub> contents), which indicates that detrital sediments mostly originated from volcanic outcrops. This was already shown before (e.g. publications by Eduardo Garzanti). In the text, we explicitly do not say that these layers are enriched in these minerals but only that they are present. We rather suggest that the background detrital mineral assemblage is quite similar between DL and LL2 layers (e.g., lines 459-460) and that dilution by carbonates controls their relative abundances. Obviously, this is still unclear and we will therefore rephrase this part of the text to improve clarity.

Line 459. Add a reference after “interface”.

There is no reference for this sentence, this is a suggestion derived from our observations. (“The internal draping of these layers suggests...”).

Lines 473-475. The authors may cite a recent regional modelling study that simulated surface salinity anomaly distribution in the Mediterranean Sea by increasing Nile river discharge (Vadsaria et al., 2019).

Very interesting paper, thanks for the suggestion!

Lines 476-479. It is unclear whether the authors treated here the mixing of water masses or sedimentation processes. Please clarify.

Thanks. We understand that the text is unclear and we will change it for clarification. The text does actually refer to both water-mass mixing and sedimentation processes. Double-diffusion is a typical process driving sedimentation and water-mass mixing in river deltas where low salinity, sediment-laden plumes enter a more saline water body. Salinity mixing and downward sediment transport is driven by the formation of salt fingers below the freshwater plume and supports the deposition of sediments on the seafloor (Parson et al. 2001; Jazi and Wells, 2016).

Lines 495-496. Add the reference for the bottom water d13C value of -7‰ off the Nile river mouth.

This value comes from our data (so there is no reference) but we will rewrite that part of the text for clarification.

Lines 511-514. About d18O of authigenic carbonates. What does “theoretical d18O” mean? D18O value of equilibrated calcite? If so, how do you estimate temperature and seawater d18O of surface and bottom waters?

Yes, theoretical d18O refers to inorganic calcite in equilibrium with seawater. To calculate the d18O of inorganic calcite, we have used

- seawater temperature profiles at present from Medatlas (MEDAR Group, 2002)
- sea surface temperatures for sapropel S1: median from Alkenone SST estimations (Blanchet et al. 2014, supplementary information)
- seawater d18O profiles from Gat et al. (1996)
- sea surface d18O for sapropel S1: median from d18Osw from Blanchet et al., 2014)

The equations used for calculating the equilibrium d18O of inorganic calcite and biogenic carbonates shown in Fig. 4 were adopted from Bemis et al (1998), Kim and O’Neil (1997) and Cornuault et al. (2018)

Lines 550-564. Delete the description corresponding to K. The results are already shown and K/Ti is not presented in Fig.9a.

Will be modified accordingly.

Lines 638-641. SIW was already defined.

Will be modified accordingly.

Numbering of figures. The authors used (a), (b), (c)...to describe the curves of re-construction instead labelling different panels (ex. Figs. 6, 7 and 8). Sometimes the different curves are combined and a common label is used. The lamination patterns are also labelled. This presentation is confusing. The authors may label the different panels or number all curves. The lamination patterns can be shown without numbering.

Well noted, we will modify the numbering accordingly. The figures will be corrected following Reviewer #2' comments and suggestions (Fig. 3, 5a,b and 8 caption).

Fig. 3. Please indicate which results correspond to which core (core P33 or core P73).

Fig. 5a, b. The x, y- axis and axis titles are too small.

Fig. 8 caption. There are errors of numbering. Please check it.

Fig. 9. A section map showing the site positions with the bathymetry will be helpful.

Good idea, thank you. It will be added to figure 9.

Fig. 10. Use different symbols to distinguish proxies used to evaluate the oxygenation states. Does each panel show the mean state of each period? What is the age range of the interruption? Did the authors recalibrate the age of the different cores using the same calibration?

In accordance with comments from Reviewer #1, this figure will be removed as it blurs the main message.