Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-114-RC2, 2020

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 #2

Received and published: 19 November 2020

Applying multiproxy approach (micro-facies analysis with inorganic and organic geochemistry) 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.

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Discussion paper

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 unpreceded 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?

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.

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 d13C 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 d18O 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)?

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.

I recommend to accept this work after major revision.

Minor / specific comments Line 28 “the entire Levantine Basin”. See my comment 1.

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

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.

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

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.

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

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

Line 218. “trialkyl” or “dialkyl”?

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

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?

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

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

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

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

Line 405. Add “ka BP” after “7.2”.

Line 406. “Ti/K” should be “K/Ti”.

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

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.

Line 459. Add a reference after “interface”.

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).

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

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

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

Lines 638-641. SIW was already defined.

Numbering of figures. The authors used (a), (b), (c)... to describe the curves of reconstruction 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.

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.

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 rage of the interruption? Did the authors recalibrate the age of the different cores using the same calibration?

References Adloff, F., Somot, S., Sevault, F., Jordà, G., Aznar, R., Déqué, M., Herrmann, M., Marcos, M., Dubois, C., Padorno, E., Alvarez-Fanjul, E., and Gomis, D.: Mediterranean Sea response to climate change in an ensemble of twenty first century scenarios, Clim. Dyn., doi: 10.1007/s00382-015-2507-3, 2015. 1-28, 2015.

Pujo-Pay, M., Conan, P., Oriol, L., Cornet-Barthaux, V., Falco, C., Ghiglione, J. F., Goyet, C., Moutin, T., and Prieur, L.: Integrated survey of elemental stoichiometry (C, N, P) from the western to eastern Mediterranean Sea, Biogeosciences, 8, 883-899, 2011.

Vadsaria, T., Ramstein, G., Dutay, J. C., Li, L., Ayache, M., and Richon, C.: Simulating the Occurrence of the Last Sapropel Event (S1): Mediterranean Basin Ocean Dynamics Simulations Using Nd Isotopic Composition Modeling, Paleoceanography and Paleoclimatology, 34, 237-251, 2019.

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