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# *Interactive comment on* "Eastern Mediterranean Sea circulation inferred from the conditions of S1 sapropel deposition" *by* K. Tachikawa et al.

**Anonymous Referee #1**

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The manuscript of Tachikawa et al., about " Eastern Mediterranean Sea circulation inferred from the conditions of S1 sapropel deposition", is well written, references are correct and updated and provides an excellent data set at different resolutions for almost last 20 kyr in the Eastern Mediterranean. This study is mostly based on oxygen isotopes in planktonic foraminifera, major and trace elements composition and benthic foraminifera abundances. Obtained similar results to previous studies pointing a good work at laboratory. The main and more relevant conclusion is a "tentatively" different model for S1 termination compared to re-ventilations events during S1. I am inclined to recommend the MS for publication. However, there are some aspect that trouble me and I have recommended Publish with Major Revisions. I think that the issues below require a re-write and may need a new manuscript focus.

My major comments are focused on 5 points of the manuscript:

1/ The justification for the use of certain geochemical ratio need to be stronger.

The ratio Br/Cl is used for illustrate changes in organic matter content. Nevertheless previously this manuscript authors (e.g., Carpatanis et al., 2011) used only Br counts. Other authors proposed a different way for Br salt correction (see Ziegler et al., 2008 Paragraph [11] Fig. 3c or Bahr et al., 2014 G3). I really don′t recommend use Cl. Tjallingii et al., 2007 demonstrate that "...Cl as a proxy for the seawater content in the sample volume analyzed by the XRF core scanner."... So, in fact Cl is just a proxy for porosity (and core dryness), almost equivalent to lithology and progressive sediment compaction. Porosity profiles often show an exponential decrease from surface to bottom. Element/Cl ratio will include this porosity and compaction signal (see van der Weijden, C. H. (2002), and will make more "attractive" any element but invalid or, at least is not clear for me, his use as organic matter proxy. The same can be applied to the S/Cl ratio.

In addition, through the manuscript, Author′s use different geochemical ratios but I really miss a principal component analysis only for elements (no for the element/Al ratio).

2/ Justification of U/Al and Mo/Al along the interval affected by downward sulphidisation.

Author′s correctly indicate the presense of " downward sulphidisation and impacts the distribution of trace elements such as Ni, Co, As and Cu associated with pyrite (Passier et al., 1996)"... but this kind of front also affect to U and Mo!. Reoxidation will reduce U (Mangini et al. 2001 Mar. Geo.), but downward pore water from sapropel levels will increase U and Mo, in fact described U/Al enrichment from aprox. 148 cms is just the same described for interval affected by downward sulphidisation (darkest gray band at Figure 3)!.

3/ Justification of G. ruber 18O as fresh water proxy.

Author's estimated the G. ruber 18O anomaly combining SST and isotopic record but obtained anomaly are just parallel to non-anomaly isotopic record! (See Fig. 4 dark and light blue lines). I think they obtained just the typical oxygen isotope record dominated by changes in global ice volume (deglaciation) (Rogerson et al., 2004 proposed other ways for resolve this problem).

Chapter "6.2 Conditions of bottom water circulation prior to S1 deposition" need to be re-write. Only Ti/Al shows a clear signal (aridity or not in the continent) but presented proxies do not indicate major changes in the water column.

4/ Multiple re-ventilation in the middle of the S1 period.

Author's propose "possible occurrence of several ventilation events with different amplitude"... but this is not sustained by any data. Author's correctly attribute V/Al, V/Ti, and other proxies depletion to "continuous organic matter oxidation with oxygen penetrated into sediments as well as the subsequent reduction of Mn oxides"... These redox-elements/Al enrichments are usually distanced as a result of differences in thermodynamics and kinetic processes (e.g., Thomson et al., 1995; Rutten and de Lange, 2003). In fact he used Nile River discharge proxy (Revel et al., 2014) only indicate a noticeable decline around 8 cal ka BP. I agree that coolings has been described "in relation to the winter and spring Siberian High at 9.5 to 9.1 and 8.8 to 7.8 ka (Kotthoff et al., 2008; Marino et al., 2009; Mayewski et al., 1997), and for the 8.2 ka event (Pross et al., 2009)" but the only event strong enough for S1 interruption is the 8.2 event and all changes can be explained by "continuous organic matter oxidation with oxygen penetrated into sediments". I did not found a correlation between obtained data and 9.5 to 9.1 and 8.8 to 7.8 dates... In this sense Figure 6 proposed a cooling and decrease in fresh water between 9 to 8 ky (may be just true for 8.2 ky event, despite few authors don't found this signal in the Mediterraean (e.g., Frisia et al., 2006 Quat. Res. ). But 9 to 8 kyr is just the African Humid period climax, with maximum in Nile river input (e.g., Revel et al., 2014). In any case it was a more humid period than Late Holocene. Is hard to accept central sketch in Fig. 6.

5/ Difference between total ventilation recovery at the S1 termination and previous S1 interruption:

Author′s proposed a water depth dependent S1 termination based in carbon date (aprox. 6.9 cal Ka BP) differences with these proposed by previous author′s (Lange et al., 2008). They honestly recognized that the "difference is subtle" and may be is not real. In any case they proposed a valid theorical approach for a water depth dependent model. Nevertheless is unclear the difference between this model and the re-ventilation event during S1 sapropel. Do you propose intermediate water strength (LIW) previously to deep water formation (EMDW) for the S1 final demise?. What is the difference with re-ventilation event?. Please, clarify.

Finally figures need to be clarified. Keep the same colors bars among figures (Fig. 2 and 3 Ok, but Fig. 4 and Fig. 5 is confuse) or include depth and age in X-axis at least one figure will be helpful. Also is rare that U, V, Mo (u/um)content is virtually the same that these element/Al ratio.

Is also rare obtain major elements (Al, Fe, Mn, etc) by ICP-MS.

I don′t include detailed comments because major revisions suggested. References non checked.

I hope these comments will be useful for authors.

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