

## ***Interactive comment on “Lack of marine entry into Marmara and Black Sea-lakes indicate low relative sea level during MIS 3 in the northeastern Mediterranean” by Anastasia G. Yanchilina et al.***

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Response to second peer review:

Reviewer: The reviewer comments in 3.2.3 that the introductory section does not sit well with the data that we present and the discussion of MIS 3 sea-level data is poor and in particular how this fits with our data. It is suggested to significantly trim this unfocused sea level portion of the introduction and discussion and refocus your manuscript on the timing of the reconnection of the Black Sea with the Mediterranean.

Response: We respond that we want this to be specifically a regional sea level data,

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not connections of the Black and Marmara Seas with the eastern Mediterranean. Our contribution to writing about the last reconnection of the Black Sea with the Mediterranean is previously published in Yanchilina et al. (2017) and (Yanchilina et al. 2019).

Reviewer: The reviewer comments about the age model that either more detail is needed, including reporting of the  $^{14}\text{C}$  data, clearly stating how the age model was constructed, and/or a more direct approach using the fact that the Black Sea is the dominant source for water vapor feeding the formation of the Sofular Cave stalagmites. The reviewer goes to further suggest that we could use the well dated speleothem  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  records to constrain the age model which we can check or refine with the  $^{14}\text{C}$  determinations, especially for the time period that extends beyond the limits of the radiocarbon dating.

Response: We agree and we should have described the age model for each of the geochemical set of measurements we present in Figure 3. The only new data in the figure is our  $^{87}\text{Sr}/^{86}\text{Sr}$  data from the Sea of Marmara which we describe in lines 163-164. We should have mentioned that we follow the age model for the  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  of the mollusks in the Black Sea previously published in Yanchilina et al. (2017) and Yanchilina et al. (2019), which we did exactly with the method advised here by the reviewer, matching the  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  of the mollusks to the  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  composition of the U/Th dated Sofular Cave stalagmites.

Reviewer: Age model - It must be an oversight that the minimum reporting requirements for the  $^{14}\text{C}$  dates are not confirmed. One expects to list the  $^{14}\text{C}$  dates that are used, any source for the  $^{14}\text{C}$  reservoir correction that you use, and the calibration curve / programme. A supplementary figure with the age-depth relationship would be a good addition.

Response: We are more than happy to include all of the measurements we present in Figure 3 along with the  $^{14}\text{C}$  measured dates. If the manuscript gets accepted as a paper in *Climate in the Past*, we will submit the measurements as a supplementary

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spreadsheet.

Reviewer: Interpretation of the proxies within the wider context of the region - D18O. Confusing sea level with ice volume.

Response: Not exactly. Bintanja et al. (2005) make separate figures for ice volume (Figure 3a) of their paper and global mean sea level (Figure 3b). They specifically state, “the main strength of our method is that it yields long and mutually consistent records of surface air temperature, ice volume, and global sea level by separating the ice-sheet and deep-water parts of the marine  $\delta^{18}\text{O}$  signal.”

Similarly, Shakun et al. (2015) also separately discuss sea level and ice volume contributions to the  $\delta^{18}\text{O}$  records. Their methodology is described in section 3.3 of their paper. Hence, our figures 1a and 1b are correct in referencing ESL and not ice volume.

Having said this, we agree that we should more thoroughly discuss the difference between ice volume and sea level in our introduction sections.

Reviewer: - Coral terraces: these are RSL records. The data is incorrectly referenced and there are no details on GIA corrections. Also see Cutler et al. (2003), Chappell (2002), Yokoyama et al. (2018) from the Great Barrier Reef, Shackleton  $\delta^{18}\text{O}$  record (1987) with the latter being an ice volume equivalent, not ESL

Response: These are not RSL records.

With the exception of the Barbados data, these are ice-equivalent sea-level, and in this case, we should actually have plotted this correctly (Figure 5) of Yokoyama et al. (2001). Yokoyama et al. (2001) corrected the dated coral reefs for GIA in their paper (Section 2.2 Glacio-hydro-isostatic modelling).

Barbados data was shown to be in the region of the world that is minimally affected by GIA effects, unless we understood correctly, and hence can be interpreted as representing ESL. It's a good idea for us to include this discussion in the text.

Reviewer: Insufficient/simplistic consideration of mechanisms of change and what is influencing your proxies—there was no real consideration of the (1) the hydrological balance of the palaeo-lakes (evaporation, precipitation, lake area and riverine inputs; the potential impact of the glacial re-routing of riverine inputs, etc. and how this may impact your proxies; (2) impact of the former proximal ice sheets on the glacio-isostatic response of the region and (3) tectonic setting of the region and the influence of active faults. The latter from the work of Vadar et al. (2014) on fault segments that developed during the Late Pleistocene.

Response: We disagree; we discussed this in lines 223-232 of the present manuscript. The  $\delta^{18}\text{O}$  value of the Black Sea-Lake and Marmara Sea-Lake carbonate is shown to reflect the composite hydrological balance of the basin through the integration of inputs in the form of river and rain water and outputs in the form of evaporative processes. This is discussed before in Major et al. (2006), a manuscript that we referenced. The fact that the  $\delta^{18}\text{O}$  of the Sofular Cave stays at  $-12\text{‰}$  through the entirety of MIS 3 shows that the hydrological balance of the glacial period existed also through the entirety of MIS 3, cold with decreased evaporation but wet from continuous riverine input, leading to a positive hydrological framework. Perhaps we should discuss the hydrological framework more.

We also discussed the impact of the former proximal ice sheets on the glacio-isostatic response of the region in lines 328-329. We are happy to expand our discussion of the GIA effects on the region. Regardless, with or without considering GIA, RSL, would still be at the level we indicate is suggested by both the geochemical evidence and by the chirp profiles.

Discussion of the potential rerouting of riverine inputs is also a great idea but we believe this is irrelevant, because regardless of how the rivers rerouted or did not reroute, the  $\delta^{18}\text{O}$  of the Sofular Cave reflects and shows that the hydrological balance, that was largely controlled by riverine inputs, stayed the same from beginning of MIS 3 to the glacial period in the Black Sea.

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It's a great idea to include Vardar et al. (2014)'s work on the influence of active faults and we hope to include this discussion in the revised manuscript.

Reviewer: Mechanisms and assumptions - Chirp profiles: there is insufficient information here; what was the vertical resolution and what processing did you undertake and using what software, etc?

Response: This is great comment and we will include this information in the revised manuscript.

Reviewer: General integration of the literature. - Currently, the framing of this work within the wider context of the literature on the Black and Marmara Seas is poor. - Does not include Aksu et al. (2016) nor Yaltirak et al. (2002).

Response: Yes we agree and we will include much more of the prior work that was done on the Black Sea, including the works of Aksu et al. (2016) Aksu et al. (2016) and Yaltirak et al. (2002) in the revised manuscript.

Reviewer: Focus of the manuscript. If you wish to make this more of a sea level story, you will need greater consideration of the wider sea-level data available in MIS 3.

Response: We did our best, and we wanted to focus on the available ESL and ice volume equivalent data, not RSL from different regions of the world. If there are any additional ESL records available, we will gladly include them in Figure 1. We do think it may be a good idea to include a threshold line of 40 m from Pico et al. (2016) as another record; we did not originally as we wanted to plot actual physical records from the  $\delta^{18}O$  and uplifted coral terrace data.

Reviewer: What is the focus of the glaciation, referring to the potential outflow of glacially dammed rivers and lakes, and especially the deglacial, e.g. the melting of the Northern hemisphere ice sheets filling the Black Sea, e.g., Chepalyga, 2007; Thom, 2010, Vidal, 2010, which in turn led to the outflow of brackish water to the Mediterranean via the Marmara Sea?

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Response: We didn't include the effects of the glaciation as we didn't think it was necessary, given that we show that the hydrological balance must have stayed the same through MIS 3, both the lake levels in the Marmara and Black Seas which we know from the chirp profiles and also from the  $\delta^{18}\text{O}$  data of the Sofular Cave stalagmites. We are happy to include this discussion in the text in the revised form of the manuscript.

Reviewer: How do your paleoshorelines compare to the lowstand terrace in Sea of Marmara at -85 m (Cagatay et al., 2009 and Aksu et al., 1999). These authors suggest that post 15 ka in Sea of Marmara, evaporation exceeded riverine and Black Sea inputs, how does this compare with your work?

Response: This is not relevant to our work as our work is about the MIS 2 and MIS 3 periods of the Black and Marmara Seas. We are happy to include this discussion as it would be relevant for the hydrology of the region.

Reviewer: There are other estimates for the depth of the sills (e.g. Major et al. 2002), who gives the elevation of the Dardanelles sill as 85 mbsl, which is consistent with the clinofolds.

Response: Exactly, we agree.

Reviewer: Do not discuss the GIA processes and how these might affect the connection between the various basins.

Response: Without a robust GIA model, we can't really discuss this in detail. We did the best we could in lines 328-329. We can give a more detailed and thought out discussion given indications of the location and extent of the Eurasian ice sheet from the work of Chepalyga (2007).

Reviewer: Discussion of the MIS 3 sea levels is incomplete and misses key references. The introduction is muddled (lines 51 to 68) and omits several well-constrained lines of evidence. The most obvious are the high resolution, continuous relative sea level records from the Red Sea from the work of Grant et al. (2012) and the Mediterranean

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(Rohling et al., 2014), both of which are publically available.

Response: We did not originally include these data because they give RSL not ESL and comparing one RSL with another RSL does not make much sense. If the reviewer does think that we should include these data on RSL from the Red Sea, we are happy to include this in the introduction of the revised manuscript.

Reviewer: - Discussion of the lithofacies, marshes (Pico et al., 2017 and 2016 studies). The mention of these in the introduction is a little odd, given that none of this data is plotted or referred to in the text. Response: We discuss Pico et al. (2016) in the introduction and in the discussion. Perhaps it is a good idea to plot the threshold they indicate in our figure 1 as its an additional independent ESL record and also shows how much disagreement there is between all of the ESL and ice volume equivalent data.

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