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Climate of the Past
Discussions

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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Anonymous Referee #2

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Yanchilina et al., "Lack of marine entry into Marmara and Black Sea-lakes indicate low relative sea level during MIS 3 in the Northeastern Mediterranean". Summary: This manuscript presents new 87Sr/86Sr data from the Sea of Marmara (the Black Sea Sr/Sr data was published in Yanchilina et al., 2017) that provides constraints on the timing of the reconnection of the Sea of Marmara and the Black Sea with the

Mediterranean. The authors also present seismic/reflection profiles from the region, although only one is new (Chrip profile of Gemlik Bay, figure 2). I have significant concerns about the work presented here (for example; the age model, interpretation of the proxies within the wider context of the region, mechanisms and assumptions: : :) and feel the submission of this manuscript is premature. The work could make a good contribution to the field, however, it requires more thought, a clearer focus and greater attention to detail to do so. As such, I would not recommend the acceptance of the manuscript for publication in Climate of the Past in its current format.

GENERAL COMMENTS

Reviewer: The main issues that must be addressed are: (1) Greater integration of the literature: Currently, the framing of this work within the wider context of the literature on the Black Sea and Sea of Marmara is poor (for example, there is no mention of the work of Aksu et al., 2016, Yaltirak et al 2002 etc.). Additionally, the selection and representation of sea-level data for the interval is inadequate (see section 3.2 below for examples).

Response to reviewer:

In the revised form of the manuscript we will make a bigger effort to integrate more of the prior work and will make sure to add very important references currently missing such as that of Aksu et al. (2016) and Yaltirak et al. (2002).

We thank for the note in regarding to develop a more thorough selection and representation of sea-level data for the interval and in the revised manuscript, will also make every attempt to make it better.

Reviewer: (2) Focus of the manuscript (and title): the data here is not a sea-level record per se, rather a record of marine incursion(s) into the Sea of Marmara and the Black Sea with rising sea level during the deglaciation. A key outstanding question that this paper helps to address, is when these transitions occurred (although your age model needs considerable work, see below). What you have here are very valuable independent constraints for the region on rising deglacial sea level (87Sr/86Sr data). I would suggest trimming (or removing?) most of the introductory sea level discussion (which is far too general), and focus on the new 87Sr/86Sr data and seismic/reflection/Chirp profiles (most of which have been relegated to the supplement). If you wish to make this more of a sea level story, you will need greater consideration the wider sea-level data available in MIS 3 (see below). The focus on MIS 3 is also a little odd, given the data you present. In figure 3, your Cl data suggests increasing salinity but you do not really attempt to untangle the mechanisms driving this (see section 3.3). Similarly, you do not comment on the fluctuations in the Ca (Sea of Marmara) and CaCO3 (Black Sea) records. Is there a connection to the Dansgaard-Oeschger events that are characteristic of the time period?

Response to reviewer:

We want this to be a sea level paper not an incursion of marine water paper. In the revised manuscript and we will answer further below, we will make a better effort to consider the wider sea-level data available during MIS 3.

We do discuss the changes in porewater Cl- and attribute this, as from previously published work, to diffusion of Cl- in the sediments after the marine incursion first into the Sea of Marmara at 12,500 and then into the Black Sea at 9,300 years B.P. The concept is pretty straightforward but in the revised manuscript we will add more discussion on diffusion of porewater Cl- and how this reflects paleo-salinity.

We agree that we need to enhance the discussion on the fluctuations in Ca and CaCO3. The accumulation of CaCO3, reflected in the XRF-measured Ca as well, is a climatic response of lake chemistry. CaCO3 maxima during Bolling/Allerod and Preboreal periods in the Black Sea have been previously interpreted to be mainly composed of authigenically precipitated calcite, which occurs within lakes as a consequence of photosynthetic utilization of CO2 and resultant calcium carbonate super-

saturation in the water column during growing season (Major et al. 2002; Leng and Marshall 2004; Bahr et al. 2005; Soulet et al. 2011a). This happened during warm periods of Bolling/Allerod and the Preboreal. Dansgaard/Oeschger events in the Northern Hemisphere are rapid warming episodes that occurred typically in the manner of decades. Hence, since Dansgaard/Oeschger events are also warming events, the increases in CaCO3 accumulation must indeed been related to their occurrence.

Reviewer: (3) General lack of rigour: The three major aspects that need addressing are; 3.1 There is insufficient information in your methods (in particular, your age model is not described in sufficient detail, see below);

Response to reviewer:

Thank you for the comment, we hope our response below will address this comment.

Reviewer: (3.1.1) Age model. I am unconvinced that your age model is reasonable (or robust) given that methodology and rationale/mechanistic relationships are currently poorly explained and justified. Either more detail is needed (including reporting of the 14C data, see comment below, see *), clearly stating how your age model was constructed (a supplementary figure would be useful), or I suggest adopting a more direct approach using fact that the Black Sea is the dominant source water source for Sofular Cave (you do mention this in lines 167 to 169). In the latter, you could use the well dated (i.e., precise, radiometric dates) of the speleothem to constrain your age model (which you can then check and/or refine with your 14C determinations, especially given your records extends beyond the limits of radiocarbon). Although you do not have a high resolution, continuous 18O record for either the Black Sea or Sea of Marmara to tune to the speleothem record, you do have very nicely resolved Ca (Sea of Marmara) and CaCO3 (Black Sea) records that have good signal correspondence to the Sofular Cave 18O record (and the Dansgaard-Oeschger (D/O) events more generally, e.g., Rasmussen et al., 2014) – warmer – more blooms and higher Ca/CaCO3 production etc. I would also suggest increasing the vertical exaggeration of the Sofular 18O record in figure 3 to help the reader.

Response to reviewer:

We agree that we need to describe the age models for all of the records presented in Figure 3. The δ18O and δ13C record of the Black Sea mollusks was tuned to the Sofular Cave stalagmites and the ages for the 87Sr/86Sr of the Black Sea were interpolated from the calculated calendar ages of the mollusks given that the measurements were done on the same set of cores. The age model for the δ18O of Sea of Marmara was adopted from Vidal et al. (2010). The age model for our measurements for 87Sr/86Sr of the Sea of Marmara is described in lines 163-164. The age model for the CaCO3 was adopted from (Nowaczyk et al. 2012) and the age model for Ca was adopted from (ÇaÄ§atay et al. 2015). Perhaps as the reviewer suggests, one way to put all the models into one would be to tune all the records to the Sofular Cave. We agree with the vertical exaggeration and would make this change in the revised figure. We should also, having looked at figure 3, should add the CaCO3 record for the Black Sea that covers MIS 2 and the Holocene.

Reviewer: Eyeballing your record of Ca (Sea of Marmara) and CaCO3 (Black Sea), I would place the transition from low to higher values (and more square-wave signal) that you currently have at 55 ka at about 48 ka. This would shift most of your records to younger ages (this has the upshot of making your data more consistent with other sea level records – see below)

Response to reviewer:

The authors are confused about what transition and what low and high values the reviewer is referring to.

Reviewer: I would suggest an additional step of incorporating the stratigraphic relationships into you age modelling, and assessment of the age uncertainties of the final age model (e.g., using Bacon, or the deposition model in OxCal). This is not vital but it

would allow you to provide some age uncertainty estimates for the marine incursion(s) into the Sea of Marmara and the Black Sea.

Response to reviewer:

Thank you for the suggestion, this would have been great for our earlier paper that exactly focuses on the marine incursions into the Black and Marmara Seas. Here, the marine incursion is not relevant for the MIS 3 period but perhaps this would be a great idea to still add more certainty to the age model and we will take this into consideration.

Reviewer: It must be an oversight that you do not fulfil the minimum reporting requirements for the 14C dates (e.g., Stuvier and Polach, 1977; Mook and van der Plicht, 1999; Millard, 2014). I know these are not new dates but I would expect as a minimum for you to list the 14C dates you use, the source for any R correction you use, nor the calibration curve/programme you use. A supplementary figure with the age-depth relationship would be a good addition.

Response to reviewer:

We are more than happy to include all of the measurements we present in Figure 3 along with the 14C measured dates. If the manuscript gets accepted as a paper in Climate in the Past, we will submit the measurements as a supplementary spreadsheet.

Reviewer: (3.1.2) Chirp profiles: There is insufficient information here (lines 175 to 182). What was the vertical resolution? What processing did you undertake (and using what software) etc.?

Response to reviewer:

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

Reviewer: 3.2 As mentioned in (1) above, there is poor integration with other available data and literature. For example, there are some cursory attempts to couch this work within the literature but most are related to the proxies and seismic/reflection profiles presented.

Response to reviewer:

In the revised manuscript we will make a stronger effort to integrate more of the available data and prior work, both for global sea level and that relating to the Black Sea and Sea of Marmara research.

Reviewer: Other issues that should be considered include: (3.2.1) What is the impact of glaciation (e.g., the potential outflow of glacially dammed rivers and lakes) and especially the deglacial, e.g., the melting of Northern hemisphere ice sheets filling the Black Sea, e.g., Chepalyga, 2007, Thom, 2010, Vidal et al., 2010, which in turn led to the outflow of brackish water to the Mediterranean via the Marmara Sea? Also, how do your palaeo shorelines compare to the lowstand terrace in Sea of Marmara at -85 m (ÇaÄet al., 2009, Asku 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 to your work?

Response to reviewer:

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 δ18O data of the Sofular Cave stalagmites. We are happy to include this discussion in the text in the revised form of the manuscript.

Our paleoshorelines are at the same level of the lowstand terrace in Sea of Marmara.

Prior to the Sea of Marmara being connected with the Mediterranean at 12500, there was indeed an arid period during which the lake level decreased. This is not exactly super relevant for our work as we make an effort to focus on MIS 3 not deglaciation during MIS 2. The only point of relevance is the increase in CaCO3 accumulation in the Sea of Marmara, what occurs during warm periods in Marmara and Black Sea lakes.

Reviewer: (3.2.2) There are other estimates for the depth of the sills (e.g., Major et al., 2002 – a co-author? - gives the elevation of the Dardanelles sill as -85 5 m, which is consistent with the clinoforms). In addition, you do not discuss (or model) the GIA processes and how these might affect the connection between the various basins.

Response to reviewer:

So the first point is that the elevation of the Dardanelles sill is consistent with the clinoforms so yes, this is the case and thank you for the agreement with us. Second, we did do GIA modeling in previous versions of the paper but we decided not to add them here as we want to focus on reporting RSL and not ESL.

Perhaps it is good to give GIA more consideration as we do mention some conclusions about ESL and more discussion of the location and distribution of Eurasian ice sheets during MIS 3.

Reviewer: (3.2.3) The discussion of MIS 3 sea levels is incomplete and misses some key references. There also seems to be some confusion/conflation of relative- (RSL) and eustatic sea level (ESL), and ice volume equivalent throughout the manuscript. You explicitly state the difference between RSL and ESL (in lines 44 to 45) and yet the discussion of the various means of determining sea level (and what, RSL, ESL or icevolume equivalent) in the introduction is muddled (lines 51 to 68) and omits several well-constrained lines of evidence (as does figure 1). The most obvious are the high resolution, continuous relative sea level records from the Red Sea (e.g., Grant et al., 2012) and the Mediterranean (Rohling et al., 2014) – both of which are publically available. In more detail, in lines 51 to 57, you also have a list of 1 to 4 methods for deriving past changes in sea level, but 3 is missing. These are subsequently returned to in the discussion but only in a very superficial manner. My comments are: Isotopic methods and deconvolution of the 18O signal: The oxygen isotope ratio of marine sediments can be used to infer past sea levels (as a first order approximation) using the relationship between the 18O of the mineral precipitated (e.g., foraminiferal calcite) and the processes governing the hydrological cycle (and thus sea level). The relative contribution of global ice volumes and temperature to foraminiferal oxygen isotopes is complex and subject to substantial uncertainties and several attempts to unravel this are available – e.g., through various assumptions and/or modelling (e.g., Bintanja et al.,2005, Shakun et al., 2015, as mentioned by the authors but see also de Boer et al., 2014, Waelbroeck et al., 2002, Elderfield et al., 2012). However, it should be noted that in all these reconstructions, the global ice volume component is comprised of both a terrestrial component AND any floating ice. Changes in the former would contribute to both 18O AND sea level, whereas changes in any floating ice would ONLY change the 18O record and not sea level. In other words, reconstructed changes in global ice volumes may not be equivalent to changes in sea level (e.g., Rohling et al., 2017). In figure 1, this could easily be fixed by changing the axis labels of (a) and (b) to ice volume. The authors might also consider adding the Elderfield et al. (2012) and/or the de Boer et al. (2014) reconstructions. Coral terraces: these are RSL records, unless they have been corrected for glacioisostatic (GIA) processes, in which case they do provide ESL constraints. In figure 3, the data is incorrectly referenced and there are no details on your(?) GIA corrections to the data. Please clarify. In figure 3, you plot (some) of the coral Barbados (for other Barbados sea-level data within the time period e.g., Bard et al., 1990, Fairbanks et al., 2005), and (some) Huon Peninsula (Yokoyama et al., 2001 but see also Cutler et al., 2003) data along with the 18O record of Shackleton (1987). The latter is an ice volume equivalent sea level not ESL. There are other coral sea-level records available that span some of your time window – e.g., Chappell, 2002, Cutler et al., 2003; or the very recent Yokoyama et al 2018 paper from the Great Barrier Reef. The authors might consider a wider selection of coral data: : :?

Lithofacies, salt marshes etc. where former sea levels are reconstructed using a modern analogue for the relationship between the indicator and sea level at the time of formation (note, the Pico et al., 2017 study is a GIA modelling studies of previously published sea-level reconstructions, assuming different ice models – i.e., variations in the volume and the spatial extent of the former ice sheets – as well as Earth rheolo-

gies). The mention of these in the introduction is a little odd, given that none of this data is plotted nor referred to in the text. The Pico et al. (2016, 2017) studies are returned to in lines 339 to 340 but with very little analysis. Given the above, the introductory section does not sit well with the data you present, and the discussion of MIS 3 sea-level data is poor, in particular how this fits with your data. I would significantly trim this unfocused sea level portion of the introduction and discussion and refocus your manuscript on the timing of the (re)connection of the Sea of Marmara and Black Sea to the Mediterranean.

Response to reviewer:

(1) Response to not including sea level records from Grant et al. (2012) and Rohling et al. (2014). These are both RSL not ESL and we wanted to focus our discussion on ESL.

(2) Listing 4 as opposed to 3 and only discussing three is a typo on our part, it is three methods that we wanted to list.

(3) Going back to discussing prior ESL records in the discussion was our best attempt but we hope to do better in the revised manuscript.

(4) Reply to comment about δ18O signal: 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 δ18O signal.”

Similarly, Shakun et al. (2015) also separately discuss sea level and ice volume contributions to the δ18O 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.

(5) Reply about coral terrace records: 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). We are happy to include this discussion in the revised text.

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.

We wanted to focus on coral terraces that have been corrected for GIA and hence did not include other data.

(6) Reply to comment about Lithofacies. 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.

Reviewer: 3.3 Insufficient/simplistic consideration of mechanisms of change and what is influencing your proxies – there was no real consideration of the: (3.3.1) 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 might impact your proxies. In addition, there seemed to be some confusion of the systematics of 18O in marine, lake and speleothems environments. (3.3.2) impact of the former proximal ice sheets on the glacio-isostatic response of the region; (3.3.3) tectonic setting of the region and the influence of active faults (e.g., fault segments that developed during the Late Pleistocene, for example, see Vardar et al., 2014 and references therein).

Response to reviewer:

We disagree; we discussed this in lines 223-232 of the present manuscript. The δ18O 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 δ18O of the Sofular Cave stays at -12 ‰ 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 δ18O 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.

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: 3.4 Writing: some careless mistakes in the manuscript – for example, poor/incomplete referencing (e.g., line 34) and repetition (line 323 to 324 is immediately repeated as the next sentence).

Response to reviewer:

These are great observations and we will make the requested changes in the revised for of the manuscript.

TECHNICAL CORRECTIONS

Reviewer: References: Greater care with referencing needed. Please check manuscript. For example, line 34: “Members 2006” should be “EPICA Community Members”

Response to reviewer:

We agree with this suggestion, this was a problem with our endnote referencing and we will fic this.

Reviewer: Figures: Figure 1: incorrect axis labelling, poor selection of available data, inaccurate/ incomplete referencing of data in the caption.

Response to reviewer:

We disagree, our axis labeling is correct and we decided to select the data only with ESL records and not all the available RSL as well as its harder to directly compare RSL from our region to all the other RSLs as they are RSL for a reason, impacted by GIA.

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Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2019-30/cp-2019-30-AC4-supplement.pdf

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-30, 2019.