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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.

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

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).

(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?

(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);

(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 δ18O 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 δ18O 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 δ18O record in figure 3 to help the reader.

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)

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.

* 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 ΔR correction you use, nor the calibration curve/programme you use. A supplementary figure with the age-depth relationship would be a good addition.

(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.?

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.

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 $\sim$-85 m (ÇaÄ§atay et 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?

(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 $\pm$ 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.

(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 ice-volume 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 δ18O 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 δ18O 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 δ18O AND sea level, whereas changes in any floating ice would ONLY change the δ18O 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 glacio-isostatic (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 δ18O 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 rheologies). 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.

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 δ18O 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).

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).

TECHNICAL CORRECTIONS

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

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

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