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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.

Anonymous Referee #3

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General comments. Yanchilina et al. present a compilation of geochemical data that is used to infer periods of connection/disconnection within the Black Sea – Marmara Sea – Mediterranean Sea system during MIS 3. The authors also present seismic profiles in order to determine the level of the Marmara Sea Lake during MIS 3 and hence provide a RSL maximum boundary of 65-70 mbsl during MIS 3, with a possible intrusion of marine waters around 50 kyrs BP.

I have major concerns regarding the study (see below). In its present form (poor use of the existing literature, poor description of the methods, misuse of some set of previously published data, lack of confident chronological constraints for interpretation of

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Discussion paper

the seismic profiles. . .), I would not recommend publication of the study in Climate of the Past. With some more work, I think that such a contribution could be useful to the community.

1. Existing literature:

1.1. It is hard to understand which data are new, and which interpretations are novel compared to the existing literature (for example compared to Çagatay et al. (2009) regarding the level of the Marmara Sea Lake during MIS 1 – MIS 2 – MIS 3, or compared to Aloisi et al (2015) regarding the freshening of the Marmara Sea Lake from MIS 4 to MIS 3 and its link to a potential connection to the Black Sea lake during this time).

1.2. In general, the paper could benefit from a better use of the existing literature. For example, high-resolution qualitative paleosalinity records exist for the Black Sea during MIS 3 (Shumilovskikh et al., 2014; Wegwerth et al., 2016). They actually show changes (freshening and salinity rise) and could be integrated to the study to better document salinity changes in the Marmara Sea and connectivity between both basins during MIS 3. The authors may also integrate the new 87Sr/86Sr data from the Black Sea published in Ankindinova et al. (2019). Also, literature on the Gemlik Lake exists and some cores were dated and may be useful to the study (Gasperini et al., 2011; Taviani et al., 2014; Filikci et al., 2017). A review paper about the connectivity between basins in the Caspian Sea – Black Sea – Marmara Sea through time has been recently published and could be integrated to the study (Krijgsman et al., 2018).

2. Methods are poorly described. In some aspects, a better transparency regarding the methods, which dataset is new and which ones are from other sources would benefit to the paper:

2.1. The authors write they use a GIA model to provide highstand thresholds on RSL in the Black Sea and Marmara Sea (Lines 69-71). In the discussion it seems like they refer to their GIA results (lines 325-327). However, the authors do not provide any kind of information in the method section detailing the GIA model they used, the parameters,

the time period, etc. There is not even a figure showing these results for the Black Sea – Marmara Sea area during MIS 3. The authors simply acknowledge a researcher for generating GIA corrections (line 544). Did the author actually run a GIA model and present these results in the paper?

2.2. In this paper, the only new geochemical dataset (although it is an impressive one) is the 87Sr/86Sr record from ostracods/shells from the Marmara Sea. However, the authors do not mention that few of these data were already previously published in Vidal et al. (2010). Also, the ostracods and mollusks used for strontium analyses come from the sediments of 4 different cores (ITU-C1, ITU-C10, MD01-2430, MD01-2426). However the authors mention the age model for only one of these cores (core MD04-2430). What about ITU-C1, ITU-C10 and MD01-2426 age models?

2.3. MD01-2430 age model is poorly detailed. So it is difficult to evaluate its quality. The authors (lines 164-166) write they composed their own age model from 14C measurements made from pieces of mollusks from MD01-2430 (Vidal et al. 2010) and it seems that they tuned MD01-2430 ostracod δ18O data (from Vidal et al, 2010) to Black Sea mollusk ones (Yanchilina et al., 2017) and Sofular Cave speleothems (Fleitmann et al., 2009; Badertscher et al., 2011). Isn't it a bit circular, as Black Sea data have been already tuned to Sofular Cave speleothem (Yanchilina et al., 2017)? Furthermore, similar patterns between δ18O and 87Sr/86Sr changes in the Marmara Sea and in the Black Sea are used to infer periods of connection between basins later in the paper. If they are tuned to each other, then the inferred periods of connection between basins are not a result per se anymore, it is premise. To get around this problem, the data series shall be kept on independent age models (Blaauw, 2012). Please provide a detailed and transparent description of the age-depth models: 14C dates used (they seem to have been already published in Vidal et al. (2010) and Çagatay et al. (2015)), reservoir correction applied, software used, tuning, uncertainties. . .

2.4. Also, the age model of core MD01-2430 has been already published in Vidal et al. (2010) based on mollusk 14C dates, and later revised/refined in Çagatay et al. (2015)

using tephra markers and tuning to Greenland ice cores. Why did the authors need to redo this reliable age model? Also, as the MD01-2430 Ca% data are from Çagatay et al. (2015), are the MD01-2430 87Sr/86Sr data and Ca% data shown in figure 3 with different age-depth models whereas they come from the exact same core?

3. Misuse and over interpretation of chloride data

3.1. The authors mention pore water chloride data from the Black Sea (Soulet et al., 2010) and Marmara Sea (Aloisi et al., 2015) to say that the Black Sea and Marmara Sea were fresh during MIS 3 and MIS 4 (Lines 112-114). This is not exactly what the original papers state. According to Soulet et al. (2010) the Black Sea was fresh ($\sim$1 psu) during the LGM. There is no mention for earlier periods. According to Aloisi et al. (2015) the Marmara Sea was not fresh but brackish (4psu) during MIS 3 and freshened at the end of MIS-4. The authors should stick to the conclusions of the original papers if they don't provide further material to modify/refine the original conclusions.

3.2. In figure 3, Black Sea and Marmara Sea pore water chloride data are shown as a function of the sediment age which is nonsensical. Diffusion and advection processes continuously alter the geochemical pore water composition, and hence there is no obvious relationship between the age of the sediment and its pore water geochemical composition (for example Manheim and Chan, 1974; Adkins and Schrag, 2003; Soulet et al., 2010; Aloisi et al., 2015). Basically one cannot interpret pore water geochemical data directly as a function of the age of the sediment as unfortunately the authors do at lines 214-219. These data should be dropped from figure 3.

4. The paper is mainly focused on MIS-3 and on the Marmara Sea. However, if one removes the pore water chloride data from figure 3 because of the reasons detailed just above, then only the MD01-2430 Ca% (Çagatay et al., 2015) extends back to MIS-3. The remaining Marmara Sea geochemical data (δ18O [Vidal et al., 2010] and 87Sr/86Sr [Vidal et al., 2010; this study]) only extend back to MIS-2. Can these data be used to support the conclusions for MIS-3?

5. The similarity between the Black Sea CaCO3% (Nowaczyk et al., 2012) and Marmara Sea Ca% (Cagatay et al., 2015) during MIS-3 is interpreted here without clear support as reflecting the connection between the Black Sea and Marmara Sea. The authors do not show on figure 3 that a Black Sea CaCO3% peak (for example Major et al., 2002; Bahr et al., 2005) correlates to the Marmara Sea Ca% during Bølling oscillation (see figure 6 in Çagatay et al., 2015) just before the Marmara Sea reconnection to the Mediterranean. Also Yanchilina et al. (2017) suggest that the Black Sea was isolated from the Marmara Sea during the Bølling-Allerød. So, why would the Black Sea CaCO3% and Marmara Sea Ca% positive correlation suggest the connection of the two basins during MIS-3, but not during the Bølling oscillation? Can this correlation be solely explained by a common climatic conditions as it has been shown that the Black Sea carbonate peaks are driven by surface productivity during warmer oscillations (Major et al., 2002; Bahr et al., 2005; Shumilovskikh et al., 2014; Wegwerth et al., 2016). Conclusive evidence would come from the geochemical measurements of the Black Sea and Marmara Sea carbonates deposited during these events as the authors suggest (L362-364). In the absence of such data, I would suggest the authors to be more balanced in their interpretations or to provide additional and stronger/conclusive support for a Black Sea – Marmara Sea connection during MIS-3.

6. Chronological interpretations of the seismic profiles are not supported by direct dating, and thus if I acknowledge that a sedimentary structure that is below another one should be older, in my view it is very difficult to be conclusive regarding their exact age.

Specific comments:

L22: “connections and disconnections (partial or total)”. It sounds odd, maybe remove “(partial or total)”.

L24: “persistent freshwater lakes”. The Marmara Sea salinity was reconstructed to be brackish (4psu; Aloisi et al., 2015)

L36: Remove “bi-polar”, DO oscillations are North Hemispheric climatic features. The bipolar seesaw concept does not fit the context of the paragraph.

L70: “using a GIA model”: Please describe it in the methods along with the parameters you used. The information is currently missing.

L77: “We show that the two lakes were freshwater”. I am afraid that the authors do not provide original data showing that both lakes were fresh. Instead they are building up on many previous works. Please amend this sentence.

L87: These studies do not provide “accumulation”. They only provide contents.

L91: Vidal et al. (2010) published δ18O from ostracods not from “mollusks”.

L92-103: The whole paragraph lacks support from the literature. Please cite for example Leng and Marshall (2004) for the mechanism and for example Major et al. (2002), Bahr et al. (2005), Shumilovskikh et al. (2014), Wegwerth et al. (2016) for its observation in the Black Sea.

L97: The Black Sea outflowing into the “small” Marmara Sea as an evidence for explaining the temporal synchronicity is unsupported material. Alternatively the regional climatic context could explain the temporal synchronicity.

L104-106: The way it is written is misleading. In 2010, Soulet et al. tested the age of the reconnection between 8500 and 9500 based on the available literature (Major et al., 2006) at this time. So the Yanchilina et al (2017) reference for the age of the reconnection should be dropped in this context. Similarly, Aloisi et al set the Marmara Sea reconnection to 14.7 ka based on Vidal et al. (2010) results. Furthermore the authors speak about pore water measurements in the Black Sea (Soulet et al., 2010) and in the Marmara Sea (Aloisi et al., 2015) without citing the original works.

L107: This statement does not reflect the reality. 1) Pore water chloride content of the sediment does not reflect a paleo-salinity, as advection-diffusion processes alter both the original concentrations and the age-depth relationship with the sediment. The

pore water chloride content profile is modern, not fossil. 2) The advection/diffusion model is actually a quantitative (within the limitations of the model and scenaris tested) reconstruction of the paleo-salinity. The authors state, however the opposite.

L109: “ppt”: Part per trillion? “psu” instead?

L112-114: This is not exactly what the original publications says. Soulet et al. (2010) only tested salinity models not older than 20ka. So it cannot be inferred from this study that the Black Sea was fresh at times older than 20 ka. Instead other studies can be cited. Aloisi et al. (2015) tested salinity models spanning 130 ka, with a salinity decrease at the end of MIS-4, and the salinity value inferred for the Marmara Sea lake during MIS-3 is 4psu (brackish, not fresh).

L128: You may add Shumilovskikh et al. (2014).

L140: You may add Ankindinova et al. (2019).

L155-156: There is something odd. Are you sure you leached Sr to retrieve the Sr fraction?

L163-164: “Although the original 14C age measurements have been misplaced”: Please clarify.

L169-171: Isn’t it the purpose of the paper to infer when the Black Sea was overflowing into the Marmara Sea? From this sentence it seems it was a premise of your work.

L171: Supplementary data 1 is missing so one cannot evaluate the quality of the age model and data.

L175: Literature exits for Gemlik lake: Gasperini et al., 2011; Taviani et al., 2014 ; Filikci et al., 2017

L195: Same conclusions has been reached by Vidal et al. (2010).

L211-218: Naive or even nonsensical interpretations of pore water chloride profiles, as

there is no obvious relationship between the age of the sediment and its pore water composition.

L219: Marmara Sea δ18O and Sr records do not extend back to MIS 3 and the Black Sea record is very scarce for this period so conclusions of this sentence are unsupported for the Marmara Sea and weakly supported for the Black Sea.

L226-227: Direct proxies have been published for this period in the Black Sea (Shumilovskikh et al., 2014; Wegwerth et al., 2016).

L266-267: Chronological data are needed to infer such statement and Vardar et al. (2014) do not provide direct dating of these strata.

L276-277: “It is shown that the MIS-2 period corresponds to a large transgressive period following the MIS-3 low stand (ÇaÄ§atay et al. 2009).” Yet, Çagatay et al. (2009) suggest the opposite: a forced-regression during MIS-2 to -85mbsl that followed a MIS-3 Marmara Sea level at 70mbsl.

L306: These data do not extend back to MIS-3.

L284-286: I don’t follow the authors. They write that core data are unavailable to strengthen the interpretation but "hence. . .".

L326-327: Where are these GIA results? Which results are the author referring to? No method has been described.

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