

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 #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

C1

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 C1, 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:

Reviewer: 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).

Response to reviewer:

The new data that we do our best to clearly show is our $^{87}\text{Sr}/^{86}\text{Sr}$ measurements and the seismic profiles from Gemlik lake. What is specifically novel in our study is our interpretation of both our results and re-interpretation of what was previously published. Neither Çagatay et al. (2009) nor Aloisi et al. (2015) make the conclusion about RSL, they only focus on the interpretation of the lake data. We strongly believe this work should be interesting and relevant to the paleo sea-level community.

Reviewer: 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 $^{87}\text{Sr}/^{86}\text{Sr}$ data from the Black Sea published in Ankindinova et al. (2019).

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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).

Response to reviewer:

We agree, many of these studies should be better integrated into ours. Given that we initially wrote the manuscript in 2016, were focused on revising the 2016 version, when some of these studies were not yet published and hence, we did not know of their existence.

We think the work of Shumilovskikh et al. (2014) should be integrated into our study. Their paper goes back to 63 kyr B.P. and they show a freshening of the Black Sea lake from 63 kyr B.P. to 19 kyr B.P., which agrees with our work.

The work of Wegwerth et al. (2016) also should be integrated and we thank the reviewer greatly for this suggestion. The authors also show and confirm that the Black Sea only freshened from MIS 4 through MIS 3 and into MIS 2 supporting our work.

The work of Ankindinova et al. (2019) is fascinating for the Black and Marmara Sea community but it is not very relevant for our work as we focus on the Black and Marmara connection to the global ocean during MIS 3 not the early Holocene connection of the Black Sea with the Sea of Marmara.

The authors were not aware that there was existing core data for Lake Gemlik, this is very interesting. The MARM05-124 core from the study of Gasperini et al. (2011), cores GE123, GE124 and the corecatcher GE126 from the study of Taviani et al. (2014), and core M13-08 from the work of Filicki et al. (2017) were recovered from approximately 70 mbsf of water depth. Cores MARM05-124, GE123, GE124, and GE126 indicate that below the second erosion surface, there was brown lacustrine mud of

C3

dates 13,950 uncalibrated 14C years and older (Gasperini et al., 2011 and Taviani et al., 2014). The erosion that occurred after that is indicated by the sandy gray in these cores that followed the deposition of lake sediments fed by rivers. All the sediments that are observed to have been below those belonging to the brown mud must then be of age older, belonging to MIS 3 and 2. Hence, the authors would like to thank the reviewer for providing us this information that also we think supports our work and interpretation.

In Vadar et al. (2014), the authors indicate that Gemlik and Bandirma lakes were fed by rivers during MIS 2 and 3 and had lake levels of -50.3 and – 60.5 mbsl (and -53.3 and -63.3 mbsl with the GIA correction). At this period, the lake level in the Sea of Marmara was down to -90 m. Both of the lakes are shown to have discharged into the Marmara during MIS 3 and 2. Gemlik lake is shown to have discharged into the Imraeli Basin, located east of the Imrali Island. The lacustrine unit is indicated to have deposited on the acoustic basement of the lakes, this is supported by the cores from the work of Gasperini et al. (2011), Taviani et al. (2014) and Filicki et al. (2017).

Reviewer: 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:

Response to reviewer:

We tried our best to make the clarification but will do even better for the revised manuscript. We will elaborate specifically on the seismic profile of the Lake Gemlik.

Reviewer: 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 paper 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?

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Response to reviewer:

So this was a mistake on our part. We did use a GIA model for earlier version of the paper, the results of which were heavily criticized so tried to refocus our paper on the RSL results. This part was not altered / removed.

Reviewer: 2.2. In this paper, the only new geochemical dataset (although it is an impressive one) is the $^{87}\text{Sr}/^{86}\text{Sr}$ 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?

Response to reviewer:

The age models for the other cores we tuned to the $^{87}\text{Sr}/^{86}\text{Sr}$ record of the core that was ^{14}C dated with results from Vidal et al. (2010). We acknowledge this is an imperfect method but the best we could do under the given circumstances as the original ^{14}C dates were misplaced.

Reviewer: 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 ^{14}C measurements made from pieces of mollusks from MD01-2430 (Vidal et al. 2010) and it seems that they tuned MD01-2430 ostracod $\delta^{18}\text{O}$ 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 $\delta^{18}\text{O}$ and $^{87}\text{Sr}/^{86}\text{Sr}$ 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.

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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: ^{14}C 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 . . .

Response to reviewer:

We decided to tune the age model of MD01-2430 to Sofular Cave U/Th record for consistency with the Black Sea $\delta^{18}\text{O}$. In Vidal et al. (2010), the authors correct their ^{14}C dates with a water reservoir age of 400 years and then convert to calendar ages using marine04 data set with Calib 5.01 software. Vidal et al. (2010) go on to say that they used the 400 year correction as an assumption. Çagatay et al. (2015) re-calibrate Vidal et al. (2010)'s ^{14}C dates with INTCAL13 software. We believe our method by tuning to a well dated Sofular Cave is a better alternative to calibration to calendar ages. The assumption of 400 years is just an assumption, especially given that it's been clearly shown that the ^{14}C reservoir age changes significantly for lakes. Our method isn't perfect but using a constant 400 ^{14}C reservoir age with 400 years as an assumption isn't either. Our age model also does not affect our interpretation of MIS 3 lake levels.

Reviewer: 2.4. Also, the age model of core MD01-2430 has been already published in Vidal et al. (2010) based on mollusk ^{14}C 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 $^{87}\text{Sr}/^{86}\text{Sr}$ data and Ca% data shown in figure 3 with different age-depth models whereas they come from the exact same core?

Response to reviewer:

The authors see the point and perhaps it is better to leave the MD01-2430 $^{87}\text{Sr}/^{86}\text{Sr}$

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data with the original Vidal et al. (2010) age. We think our method by independently tuning to another reliable record was an upgrade on the previously published age model, that is independent of a variable 14C reservoir age as it's been shown that the 14C reservoir age in the Black Sea, for instance, has varied up to 2000 14C years and is not a constant 400 14C years (Soulet et al., 2010).

Reviewer: 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 (‰ 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.

Response to reviewer:

The authors respectfully disagree. The data is published and it is for other researchers to use in order to build upon earlier work. This is exactly what we are doing. The authors don't have to stick to the original conclusions if we are building up on the original data by compiling other data sets that show, in our interpretation, that both lakes were fresh / brackish with lower salinity.

Aloisi et al. note in 6.1 that, "Since surface waters cannot be significantly saltier than bottom waters our results are consistent with those of ÇAĞATAY et al. (2000), ÇAĞATAY et al. (2009) who provide the lowest salinity estimate (S of about 3 to 7 ‰ for Marmara Sea surface waters during the last glacial from 75 cal kyr BP to the post-glacial reconnection with the Aegean Sea at 14.7 cal B.P."

Aloisi et al. also note in 6.3, contrary to what is suggested by the reviewer, that, "An earlier freshening, such as that modelled in the standard run, can allow for the return

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to marine conditions for a few thousand years after the initial freshening at 75 cal kyr BP (data not shown). Nevertheless, even in this scenario, lacustrine conditions have to be persistent over at least 50 kyr before the 14.7 cal kyr BP marine ingressions in order to model to reproduce the pore water Cl- profile." Hence, 50 kyr is given as the latest age the freshening could have started not the earliest and is consistent with our interpretation of the Marmara Sea porewater Cl-.

Reviewer: 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.

Response to reviewer:

The authors respectfully disagree. While yes, porewater Cl- profile will change over time but this is something that is understood to happen in the future as porewater Cl- is subject to diffusion and advection. But, at this moment, the porewater Cl- at a specific depth that is given a calendar age is porewater Cl- at that age of the current sediment (it would not have changed much between when the porewater Cl- was measured a few years ago to now). We are not making direct interpretations but building up on the work of Aloisi et al. (2015) and Soulet et al. (2010). Perhaps we need to make our interpretations more clear and that porewater Cl- at this age of the sediment is not what was porewater Cl- at the time the sediments accumulated. Instead it reflects the porewater Cl- at this depth and age of the sediment after the processes of advection and diffusion have altered the original porewater Cl-.

Reviewer: 4. The paper is mainly focused on MIS-3 and on the Marmara Sea. How-

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ever, 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 ($\delta^{18}\text{O}$ [Vidal et al., 2010] and $87\text{Sr}/86\text{Sr}$ [Vidal et al., 2010; this study]) only extend back to MIS-2. Can these data be used to support the conclusions for MIS-3? C4 CPD

The similarity between the Black Sea $\text{CaCO}_3\%$ (Nowaczyk et al., 2012) and Marmara Sea Ca% (Çagatay 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 $\text{CaCO}_3\%$ 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 $\text{CaCO}_3\%$ 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; Wegewirth 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.

Response to reviewer:

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If one removes the porewater chloride data, which should not be done because of the reasons above, then in addition to the Marmara Ca% there is also the Black Sea CaCO_3 from Nowaczyk et al. (2012) and also the Sofular Cave $\delta^{18}\text{O}$ from Badertscher et al. (2011) and Fleitmann et al. (2009).

We do think we should include the rest of the Black Sea CaCO_3 record from other published data for completeness.

The reviewer correctly point out that Yanchilina et al. (2017) argue for a disconnection between the Black Sea and the Sea of Marmara during the Bølling Allerød. This actually strengthens our point. While the Marmara Sea Ca record is constant after its connection with the Mediterranean, the Black Sea Ca record (or interchangeably CaCO_3) has a peak during early Preboreal. The two peaks behave differently after the Sea of Marmara has connected with the Mediterranean. We do agree that the reviewer does point out something we do agree with, as CaCO_3 accumulation does correspond to fluctuations in climate, and both the Sea of Marmara and the Black Sea are in the same geographical region, both of the CaCO_3 could be responses to climate and not connectivity. We should add more discussion to this point.

Reviewer: 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.

Response to reviewer:

This is not exactly the case. In Supplementary Figure 4, there is a measured ^{14}C date of the lacustrine sediments of 24.9 kyr ^{14}C years. All the sediments that are outcropping to the left in the figure and below the erosion surface and the marine sediments (in red) are younger than 24.9 ^{14}C years. So the reviewer comment does not give accurate criticism of our work.

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Reviewer: Specific comments:

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

Response to reviewer:

Thank you for the comment; The authors would make an adjustment to this statement to make it more clear and perhaps remove, “partial or total,” as the two lakes are either connected or they are not.

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

Response to reviewer:

We agree with this and should make it clear what we mean by lakes and instead refer to the Sea of Marmara and the Black Sea as brackish lakes with low salinity as by definition, freshwater is that water that has 0 salinity (National Atmospheric and Oceanic Administration 2017).

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

Response to reviewer:

This is a good suggestion and we would make this alteration in the revised manuscript.

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

Response to reviewer:

The authors used a GIA model to draw conclusions about ESL in previous versions of the manuscript but decided it was a better idea to stick to reporting RSL and just make educated conclusions about ESL given the currently understood distribu-

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tion of Eurasian ice sheets during MIS 3. The authors need to remove this from the manuscript.

Reviewer: 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.

Response to reviewer:

This is a good suggestion and in the revised version of the manuscript we will make this change.

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

Response to reviewer:

We agree with this and will make this change in the revised manuscript.

Reviewer: L91: Vidal et al. (2010) published $\delta^{18}\text{O}$ from ostracods not from “mollusks”.

Response to reviewer:

This is correct and we will make the change in the revised manuscript.

Reviewer: 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.

Response to reviewer:

This is a great suggestion and we do indeed need to support this paragraph with the above citations. We will make sure to make these changes in the revised manuscript.

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

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regional climatic context could explain the temporal synchronicity.

Response to reviewer:

The authors point the reviewer to the discussion above regarding CaCO₃ and connection/ disconnection between the Marmara Sea and the Black Sea.

Reviewer: 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 scenarios tested) reconstruction of the paleo-salinity. The authors state, however the opposite.

Response to reviewer:

We agree that we should make sure to cite Soulet et al. (2010) in this and also clarify that this is the porewater Cl- now, not then, given advection and diffusion within porewater of salts. We completely agree with the fossil vs. modern and the wording should more clearly reflect what the authors want to convey.

Reviewer: L109: "ppt": Part per trillion? "psu" instead?

Response to reviewer:

Yes, psu is correct. We will make this adjustment in the revised manuscript.

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Reviewer: 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).

Response to reviewer:

We agree with this comment and should be more careful in the interpretation of pore-water Cl- from the Black Sea. We will adjust our discussion to reflect the work that was actually done.

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

Response to reviewer:

This is a great suggestion and we will add this citation in the revised manuscript.

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

Response to reviewer:

This is a great suggestion, we did not know of this paper before resubmitting the manuscript. We will add this citation in the revised manuscript.

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

Response to reviewer:

Yes, perhaps another word is, "extracted." The mollusks were dissolved in nitric acid following passing the residue through resin that separates out different elements.

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

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Response to reviewer:

The authors hope to clarify this in the revised version of the manuscript. The mollusk samples were 14C dated originally but this data was lost and was not able to be retrieved.

Reviewer: 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.

Response to reviewer:

For the MIS 3 period but perhaps it is a better idea to give the Marmara Sea samples their own age model from either Vidal et al. (2010) or Çataşay et al. (2015) to avoid this somewhat circulatory methodology.

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

Response to reviewer:

Supplementary data 1 should have been uploaded but by mistake, the authors didn't. The authors will make sure to submit the supplementary data in the revised manuscript.

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

Response to reviewer:

Thank you for this information and we will include these data in the revised manuscript. We were not aware of these data and these publications.

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

Response to reviewer:

This is a correct observation and we should adjust our wording to reflect the primary

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contribution of Vidal et al. (2010) to say that the Sea of Marmara must have outflowed to the Mediterranean Sea during this period.

Reviewer: 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.

Response to reviewer:

The authors disagree per same comments as above. The porewater Cl- that were measured, were measured at a specific depth that does have an age to it. The authors should make it more clear that the porewater Cl- profiles are always changing, although more on a centennial / millennial scale.

Reviewer: L219: Marmara Sea $\delta^{18}\text{O}$ 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.

Response to reviewer:

That is why there is the Sofular Cave $\delta^{18}\text{O}$ record to give additional strong support.

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

Response to reviewer:

Including data from these works is a great idea and we will consider in the revised manuscript. The authors wanted to focus on geochemical data sets but all available data sets, geochemical and biological should be given full consideration.

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

Response to reviewer:

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The references that the reviewer suggested regarding Gemlik lake and Israeli ridge will provide the chronological framework for this discussion. We should also adjust our wording to give Vardar et al. (2014) proper citation to reflect what the authors said in the publication.

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

Response to reviewer:

The authors are not sure which part of the Çağatay et al. (2009) study the reviewer refers to but in the abstract, Çağatay et al. (2009) clearly say, "Ancient shorelines are pervasive at -85 m on the northern shelf and in the region of Prince Islands coincident with the elevation of the modern bedrock sill in the Canakkale (Dardanelles) Strait. At times when global (eustatic) sea level dropped below the sill, the surface of the SoM stabilized at its outlet and freshened. Thus this particular shoreline is interpreted as the edge of the most recent SoM lake that existed from about 75 ka bp to 12 ka bp."

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

Response to reviewer:

This is a correct observation and we need to adjust this to instead say, "beginning of MIS 2."

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

Response to reviewer:

This discussion needs to be clarified. Our description of the results from the seismic profile of Lake Gemlik are to show that there haven't been marine deposits during MIS

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2-3-4 and that the lake has only lake deposits during this period.

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

Response to reviewer:

As mentioned previously, the earlier version of the manuscript did have the GIA model and the results but the authors decided to eliminate that and instead give much more focus towards RSL data and interpretations.

Review References:

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