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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.

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Anonymous Referee #1

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Yanchilina et al. present a compilation of data that speak to lake levels and paleosalinity in the Sea of Marmara and the Black Sea during MIS 3. The authors argue that these datasets indicate low regional sea level (80 m below present day) over most or all of MIS 3. As the manuscript stands, it is not entirely clear what data is new, or which interpretations are novel as opposed to drawn from the existing literature. This

interpretation of regional sea level near -80 m over much of MIS 3 is based on paleosalinity proxies and seismic profiles of lacustrine deltaic topsets, which are not usually considered proxies for sea level. In my view, the authors do not sufficiently explain how this data can be used to reconstruct regional sea level, and therefore I am not sure these conclusions can be drawn based on the observations presented, as detailed below. Overall, I would recommend that the authors include more discussion of how the data presented can be used to understand sea level, in addition to ample references to literature which might support the proposed interpretation about paleosalinity and lake levels. I would caution the authors to be careful in drawing inferences about sea level from lake levels/salinity, and in doing so, it is necessary to include more references that would support the conclusions.

Specific comments

Reviewer: Salinity proxies are not considered a robust proxy for sea level. A variety of other processes are captured in these measurements including local climate effects such as precipitation patterns and temperature. In the methods section, it is not clear that the authors considered the extent to which these other variables may have dominated the signal measured in these geochemical proxies for salinity.

Response to reviewer:

Changes in salinity of marginal basins have been used to give an idea for regional and eustatic sea level in the past. We think that including more of this discussion will help explain how our method will additionally contribute to reconstruction of paleo sea level.

The first example is from the work of Van Daele et al. (2011). Here, the authors look at infills in the Gulf of Cariaco, a marginal basin, that is connected to the Caribbean Sea via a shallow 58-m-deep sill. The authors use a similar idea to ours, changes in regional sea level, to infer when the regional sea level was higher than the sill versus lower. When the regional sea level is higher, saline water intrudes into the Gulf of Cariaco, creating sedimentary infills. When the regional sea level is lower, saline water does not intrude and sedimentary infills related to saline water intrusions do not occur.

The second example is from the work of Pico et al. (2016). The authors use sedimentary core analyses from a Yellow River Delta in the Bohai Sea of China to make inferences about a migrating paleoshoreline. Pico et al. (2016) use information from cores taken from the delta, to observe changes in inundation of the delta associated with first changes in regional sea level and second, make further to make conclusions about eustatic sea level after applying glacio-isostatic corrections.

The third example is using changes in the δ18O record of the eastern Mediterranean Sea (Grant et al. 2012) and the Red Sea (Siddall et al. 2003) using a basin isolation concept to infer changes in regional sea level. The basin isolation concept features reduction of intrusion of saline water into a basin that is marginally connected to the global ocean; in the Red Sea from the description of Siddall et al. (2003), "Reduction of the strait profile by sea-level lowering decreases the exchange transport of water masses through the strait. This results in increased residence times of the water within the Red Sea, enhancing the effect of the high rate of evaporation (2.06 m yr-1) on properties in the Red Sea. The basin though amplifies the signal of sea level change, which are recorded in δ18O values of foraminifera in Red Sea sediment cores."

For our manuscript, we want to make conclusions from a simple concept. The Black and Marmara Seas are connected to the Mediterranean by two shallow sills, both approximately shown to have been 80 meters below sea level during MIS 2 and 3 (the sill level connecting Marmara and Black Seas, Bosporus, is now 30 mbsl but during MIS 2 and 3 it was lower, at 80 mbsl). Now, marine water enters through these sills and these seas are saline because the regional sea level (from the Mediterranean side) is higher than the sills, hence, saline water has no option than to flow into both of these basins and make them seas as opposed to freshwater lakes. A good visual schematic is filling water in a bathtub. The water flows and if its higher than the level of the bathtub, it will overflow over the edges of the bath tub onto the floor. Same concept almost for the water inflowing into the Marmara and Black Seas, when the level of the Mediterranean

is lower, no salt water flows in and when the level of the Mediterranean is higher, salt water flows in. This is what we are trying to conclude from the data presented. The paper is not about making conclusions about lake/sea level in the Black Sea and the Marmara Seas, although it alludes to them, the paper is about the regional sea on the Mediterranean Side of the Marmara and Black Seas.

We agree that it would be great to have detailed regional and global sea level data from uplifted coral terraces but such information is largely absent and is mostly available for the glacial and postglacial and the Eemian. One of the reasons we are making and drawing these conclusions is because of the lack of regional and global sea level data that currently exists during MIS 3. We believe that knowing a constraint for regional sea level and an overall understanding that it was low, is a very important contribution for the paleo sea level community.

If there was any marine intrusion into the Marmara and Black Seas, there would have been a rise in salinity, during Marine Isotope Stage 3. There is not recorded a change in paleosalinity from all proxies available to date, most strongly the lack of change in the δ18O of the Sofular Cave in the Black Sea, shown to reflect δ18O of surface Black Sea water.

Reviewer: Furthermore, I found that the explanation regarding each method did not include sufficient information (or citations) about what values would be considered significant in indicating saline or freshwater conditions at present day or in the past. I think that the majority of the sea-level community would agree that salinity proxies are not as robust as geological markers such as dated evidence of a shoreline.

Response to reviewer:

Please see above. We would love to have an opportunity to more thoroughly discuss how proxies such as δ18O and porewater Cl-, among others, indicate past changes in salinity.

One of the points we make in the manuscript is that there is limited information from uplifted coral terraces from the time periods of MIS 3 and hence, we offer an independent method towards providing what regional sea level must have been in the eastern Mediterranean. If current disagreement is giving indicators of eustatic sea level being as high as 40 mbsl to as low as 80 mbsl, there is sufficient disagreement regarding what sea level must have been. 40 m of global ocean is a very large piece of missing water volume. We argue and give sufficient evidence that it was on the lower side in the eastern Mediterranean.

Reviewer: Inspection of Figure 3 shows that a number of these salinity proxies do not include measurements during MIS 3 (or in both basins), so it seems difficult to draw a conclusion based on these. it seems plausible that a rise in sea level may not have lasted long enough to substantially change the saltwater content of this region for the duration of time required to be recorded in sedimentary deposits. It would strengthen the paper to include a discussion of what kind of timescales of marine inundation would be required to record an increase in salinity using these geochemical proxies. In fact, previous studies have suggested rapid episodes of sea level rise of 10-15 m in just 1-2 kyr during MIS 3 (Chappell, QSR, 2002).

Response to reviewer:

This is a great idea and we would be happy to include this discussion in the revised manuscript. It is a correct observation that not all of the proxies go into MIS 3. The strongest piece of evidence comes from the δ18O of the Sofular Cave stalagmites with temporal resolution of 24 years (line 119 in the present manuscript). Hence, if salt water did enter, the saline intrusions would have had to be less than 24 years, which geologically speaking on the time scale of MIS 3, are practically insignificant.

Reviewer: Seismic reflection data constitutes the other form of evidence used by the authors to argue for continuously low sea level near -70 m. I found the figures showing the seismic reflection profiles to be confusing. Importantly, an erosional unconformity

exists above the MIS 3 lacustrine ages (which are not reported directly) and this would indicate it is possible that water levels were much higher later during MIS 3 and that any deposition during this time period was subsequently eroded during a major base-level fall leading into MIS 2. Besides the obvious unconformity, which calls into question whether these deltaic deposits represent the highest sea level over the MIS 3 period, I think a more substantial argument is needed for using deltaic topsets as a kind of sea-level record.

Response to reviewer:

While it is true there is a seismic unconformity related to sea level fall during MIS 2, and prior sediment, if it was present, would have been removed. Our point is that no such prior sediment existed. This is largely seen from the Figure 5 and Supplementary Material Figures 1-5. The chirp profiles show that MIS 3 and MIS 4 are comformable.

Reviewer: If the Black & Marmara Seas were not connected to the ocean, as argued in this study, then lake levels will not necessarily reflect sea level. My understanding is that lake levels may be largely controlled by precipitation and evaporation, and would not reflect local regional sea level.

Response to reviewer:

Lake levels are controlled by evaporation and precipitation unless they are seas which is exactly the case for the modern day Marmara Sea and Black Sea. The two basins are not lakes but are seas because of the saltwater intrusion into the two basins as a consequence of the two sills that connect them being shallower than the regional sea level from the eastern Mediterranean. Because the level of the sills is shallower, saltwater flows in. In the period before the two bodies of water connected with the Mediterranean Sea at 12500 and 9000 years ago (Sea of Marmara at 12500 and Black Sea at 9000), the two bodies of water were lakes. We use this observation to conclude that the level on the eastern Mediterranean side must have been shallower than the level of the sills, hence leading the two lakes to be controlled by precipitation and evaporation.

Reviewer: A further issue is the treatment of ages used in this study. The ages within MIS 3 deposits (and associated uncertainties) are alluded to, but are actually not reported within the main text. Obviously, this information is crucial to interpreting water depths from this data at a particular instance of time. It would be very helpful to include the ages and location of cores on the figures showing the seismic reflection profiles.

Response to reviewer:

This is a great suggestion and we are happy to supply the 14C dates as a supplementary file.

Reviewer: As I have mentioned above, these proxies may not reliably record local relative sea level, however if we were to assume they do, I am still not convinced that these proxies would represent relative sea-level across the entire MIS 3 time interval (60-26 ka). The authors argue based on observations of deltaic deposits and freshwater conditions inferred from geochemical proxies that the sea level in this region was -80 m for most of MIS 3. However, in this manuscript, the authors also note that there may have been a marine incursion from 55-44 ka, although they later dismiss the likelihood of this possibility. This time period, from 50-40 ka, happens to the be the time period during which sea-level high stands are observed globally during MIS 3 (Cann, 2000, Caybioch & Aycliffe, 2001, Hanebuth et al., 2006, Simms et al., 2009, Pico et al., 2016, DaSilva et al., 2017).

Response to reviewer:

This is an intriguing suggestion and documentation. The authors strongly believe that the relative sea level must have remained below 80 mbsl during the period 55 to 44 kyr B.P. If the regional sea level on the side of the eastern Mediterranean was sufficiently high such as to overflow into the Sea of Marmara, there would be both indications in the seismic profiles, as there is of the documentation of the last intrusion by a uniform

layer and indications of a sapropel layer, that also formed with the most recent marine incursion into the Marmara Sea 12,500 years ago. There is indeed a disagreement of the two CaCO3 curves between the Black Sea and the Marmara Seas which most likely instead documents a disconnection of the two basins with the inflow from the Black Sea decreasing / coming to a stop and with the Sea of Marmara behaving independently for a few thousand years.

Reviewer: The authors seem tied to a conclusion about low sea level during MIS 3. I think this manuscript would be improved by presenting the data and multiple hypotheses, rather than focusing on a single interpretation of this dataset.

Response to reviewer:

That is a great idea but there aren't any other reasonable conclusions that the authors can reach given the data that we have. We are open to suggestions of what lack of saline entry into the Marmara and Black Seas could mean for regional sea level on the eastern Mediterranean side but really strongly see that all the data that we have points to a low regional sea level. We believe it is good to focus on one main message in our study as opposed to making several unfocused conclusions.

Reviewer: The important contribution of this study is to bring together a variety of observations about the nature of the connection between the Marmara and Black Sea. In my view, this dataset may or may not shed light on the relative sea level history in this region given the uncertainties associated with the methods used (paleosalinity proxies and deltaic topsets are not considered robust geologic sea level indicators). However, this dataset may be an important contribution to understanding the salinity history of these basins, and I think the authors should focus on this (and presenting possible hypotheses) rather than drawing much wider conclusions.

Response to reviewer:

This manuscript is not about the salinity history of the basins but is about regional sea level on the eastern Mediterranean during MIS 3. See discussion above. The methods we use are not nontraditional as several other studies also use saline entry in and out of marginal basins to make conclusions about regional (and some go further) towards sea level through this time period. One of the main reasons to use, as the reviewer characterizes, “nontraditional” methods is because alternative methods using uplifted coral terraces do not give robust sea level indications through this time period. We use our data to argue for a low sea level during this period which we think is very important, both for development of GIA models and to understand fluctuations of past climate change.

Line by line comments

Reviewer: Line 17 – Delete “the” in front of MIS 3

Response to reviewer:

This is a great suggestion and we would remove “the” in the future version of the revised manuscript.

Reviewer: Line 17 – what do you mean by persistent?

Response to reviewer:

We meant that the climate was variable; we agree that this word is confusing and is better removed and/or changed to variable climate change.

Reviewer: Line 26 – eustatic is not in caps

Response to reviewer:

We agree and in the revised manuscript we would have eustatic with a lower case.

Reviewer: Line 38 – replace ‘elevations” with “estimates”

Response to reviewer:

We agree with this suggestion and in the revised manuscript would replace ‘elevations’

with estimates.

Reviewer: Line 39 – delete “existence of”

Response to reviewer:

We agree with this suggestion and in the revised manuscript would delete “existence of,:

Reviewer: Line 40 – delete “factors that control”

Response to reviewer:

We disagree with this and think that it makes sense to leave “factors that control” as we discuss what controls the changes in ice sheet growth and collapse.

Reviewer: Line 41 – delete “additionally”

Response to reviewer:

We disagree with deleting “additionally” as this is the second reason why its important to understand changes in ice volume.

Reviewer: Line 43 – relative sea level not in caps

Response to reviewer:

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

Reviewer: Line 45 – “The earth is moving” is imprecise language. This sentence should be written

Response to reviewer:

We agree that this sentence is not well written and agree to rewrite it to, “The relative sea level changes because of . . .”

Reviewer: Line 47 – delete “the”

Response to reviewer:

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

Reviewer: Line 50 - I think you mean methods... while there are some theoretical differences in approaches it is largely the tools that you are referring to

Response to reviewer:

Yes, we agree to change this to methods in the revised manuscript.

Reviewer: Line 53 – (3) is missing from this list

Response to reviewer:

Thank you for this note, we agree that the (4) should be (3).

Reviewer: Line 56 – grammar/wording in sentence "Isolating.." - you are not isolating ice volume from these records, you are estimating the contribution of global ice volume to changes in seawater delta δ18O.

Response to reviewer:

We agree with this suggestion and in the revised manuscript will change from "isolating" to "estimating the contribution of global ice volume."

Reviewer: Line 65 - This is not exactly what the study showed - it used estimates from the Pico et al., 2016 paper to infer the source of ice loading in North America

Response to reviewer:

The Pico et al. (2016) study reading directly from their introduction reevaluates previous geological records from the Albemarle Embayment using a new set of GIA calculations to show that the peak equivalent global mean sea level (GMSL) during MIS 3 reached a peak of -40 m which is what we wrote.

Reviewer: Line 69 – This may be misleading because the authors do not run a GIA

model to look at this question specifically. Rather authors should say they use previous studies of GIA in this region to estimate the effect of GIA-related SL changes

Response to reviewer:

This is true and we will remove the discussion of using a GIA model from the introduction. We previously did run a GIA model but decided not to include the discussion because that made the paper more complicated.

Reviewer: Line 91 – Confusing sentence

Response to reviewer:

Yes, we agree and will change this sentence to, "CaCO3 is a proxy that can be used to infer connectivity between water bodies." This sentence is more clear and direct.

Reviewer: Line 97 – need citation for volume of Marmara and river inflow

Response to reviewer:

We agree with these additions. The Sea of Marmara is 3378 km3 (Ünlüata et al. 1990) in volume and the Black Sea is 544,000 km3 in volume (Lane-Serff et al. 1997). The modern river flow into the Black Sea is 350 km3 yr-1 (Ünlüata et al. 1990) and the net modern flow of water from the Black Sea into the Sea of Marmara is 300 km3 yr-1 (Latif et al. 1991, Oszoy et al. 1995, Polat and Tugrul 1996). The modern river inflow into the Sea of Marmara is 5.80 km3yr-1 (EIE 1993). We will include these citations

Reviewer: Line 100 - confusing wording. Do you mean the lakes must have been alkaline in order to explain these accumulations? And why would you get this kind of accumulation during warming? Could you explain this concept more clearly?

Response to reviewer:

We agree and this should be better clarified. 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 CO_2 and resultant calcium carbonate supersaturation in the water column during growing season (Major et al. 2002, Leng and Marshall 2004, Bahr et al. 2005, Soulet et al. 2011a).

Reviewer: Line 108 – Can you cite references for what values would be considered freshwater or saline? What is the range of values you might find globally? What values are considered significant?

Response to reviewer:

The amount of the chloride ion in the water directly reflects salinity through the equation: salinity (ppt) = 0.0018066 * Cl^- with Cl^- concentration being in mg/L (Foundriest Environmental, 2014). NOAA defines freshwater salinity is near 0 ppt (parts per thousand) while those that are considered brackish, mixture of fresh and marine, will range between 0.5 and 35 ppt (Foundriest Environmental, Inc. 2014, National Oceanic and Atmospheric Administration 2017). Based on this range, fresh and marine waters are identified. For a lake to be fresh, its salinity must then be near 0. Given this information, it makes sense to instead refer to our lakes as brackish not freshwater and in the revised manuscript we will make this alteration.

Reviewer: Line 130 - Is this new data in this study? Authors should make that very clear, here, and in introduction. What kind of new data are the authors presenting?

Response to reviewer:

Great idea and in the revised text we would make it clear what $^{87}Sr/^{86}Sr$ data we are presenting is new and which was published before.

Reviewer: Line 134 – need reference!

Response to reviewer:

We agree with this suggestion. $^{87}Sr/^{86}Sr$ ratio of seawater has evolved over geologic

time, but value has remained constant during the period that is focused on in this study (Henderson et al. 1994). Sr isotope ratios of continental water bodies are known to reflect the geographical origin as different geological formations will have their own unique strontium isotope composition (e.g. Stein et al. 1997, Krom et al. 1999). This is used to differentiate sources of water into basins. Biological and inorganic precipitates that form in the basin of interest record the changes, which result from input and mixing of different water sources, provided the different water sources have sufficiently different isotope ratios. Seawater has currently $^{87}Sr/^{86}Sr$ value of 0.709155 for the global ocean (Henderson et al. 1994) and 0.709157 for the Aegean Sea and 0.709150 for the Sea of Marmara (Major et al. 2006). The average $^{87}Sr/^{86}Sr$ of the water feeding the Black Sea is 0.7088. The marine water also has a concentration of 30 times higher of dissolved Sr (Broecker and Peng 1982, Palmer and Edmond 1989). The modern salinity of the Black Sea surface water is 18 ppt and it represents roughly 1 to 1 mixture between freshwater and marine water. We will include the above explanation into the revised manuscript.

Reviewer: Line 137 - Can you cite something? And what is the change you would expect per volume % change of marine water?

Response to reviewer:

The citations we will plan to include are in the above description.

Reviewer: Line 140- How would you expect this value to change through time?

Response to reviewer:

Great suggestion; we would include how this proxy was previously used by both Yanchilina et al. (2017) and by Major et al. (2006) to indicate entry of marine water into the Black Sea in the early Holocene transforming it from a freshwater lake to a sea.

Reviewer: Line 165 – Is this standard practice? Can you cite a study that has used a similar method?

Response to reviewer:

Tuning geochemical records of one series of measurements to another well dated series of measurements is indeed standard practice and has been used by many. A good review of tuning paleoclimate proxies is given by Blaauw (2012). This method is not perfect but is the best known alternative for constructing age models for records that do not have their own perfectly known independent age model, which given the uncertainties associated with 14C dating and changing reservoir ages in lake basins, is used for the work of this manuscript.

Reviewer: Line 195-196 – Confusing sentence

Response to reviewer:

We agree. In the revised manuscript we would change it to, “The Sea of Marmara, given its much smaller volume relative to the Black Sea, must have outflowed to the Mediterranean Sea.”

Reviewer: Line 196 – “contest” means to disagree, I think you mean we present data to support the hypothesis that the two lakes were freshwater.

Response to reviewer:

Yes, that is correct. Instead of contest, we would change it to “show.”

Reviewer: Line 204- How exactly is this signal changing over this time period? Can you reference a figure that would show this?

Response to reviewer:

Great point. We need to add a sentence to show the difference and refer to the respective figure. The figure is figure 3. The Ca concentration of the Sea of Marmara is high and the $CaCO_3$ concentration of the Black Sea is low. The two data series bifurcate here for this time period.

Reviewer: Line 207 - There are no references for this. How do you know that every time there is marine incursion there will be a sapropel layer? How long would you expect that a marine incursion should last in order to develop this layer? Also probably should briefly define what a sapropel is for readership

Response to reviewer:

Great point. We do need to make references that note the formation of the sapropel layer in the Sea of Marmara and the Black Sea. The sapropel in the Black Sea is recorded to have formed both during the Eemian (Shumilovskikh et al. 2013), when the global eustatic sea level was close to that of the modern global eustatic sea level and during the early Holocene (Ross and Degens 1974, Lamy et al. 2006). The sapropel in the Marmara Sea is recorded to have formed during the Bolling Allerod period (Vidal et al. 2010), because the global sea level was rising at this period as the planet was coming out of its last glacial period.

Sapropel is sediment with high organic content and is understood to have formed in the Black and Marmara Seas as a consequence of changes in surface nutrients and anoxia (Emerson and Hedges 1988).

We also would welcome to add a discussion section specifying what would be the expected temporal resolution for there to be a formation of the sapropel layer. I would argue that the marine intrusion would have to be for 50-100 years, this being resolution of accumulated sediments, and of significant enough marine volume for a sapropel to have developed.

Reviewer: Line 207 – delete “evidence of”, delete “here”

Response to reviewer:

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

Reviewer: Line 211 - How do you know this is the reason? You need a little more explanation connecting the observations with the mechanism for producing these.

Response to reviewer:

I think we here explain the mechanism succinctly and sufficiently. "...as a result of the earlier connection of the Marmara Sea with the global ocean and higher post-connection salinity, leading to an earlier diffusion of marine water into the previously lacustrine sediments." Basically because the marine intrusion occurred earlier for the Marmara Sea, the saline water had more time to diffuse into the sediments relative to the Black Sea. We could add however that because the salinity in the Sea of Marmara is higher than in the Black Sea, the diffused trend also has higher porewater Cl- for the Sea of Marmara relative to the Black Sea.

Reviewer: Line 215 – Confusing discussion

Response to reviewer:

We are not sure exactly what is confusing here as what we mean to say is that if there was marine entry into Marmara and Black Seas during MIS 3, this should be reflected in the porewater Cl- measurements of this age. The porewater Cl- values would not be trending towards freshwater for both of the seas.

Reviewer: Line 218 – Replace "with the light" with "in light of"

Response to reviewer:

We think we should delete this phrase and finish the sentence after "...and we discuss this in this paper."

Reviewer: Line 236 – Delete "Index"

Response to reviewer:

We will think about this suggestion and take it into consideration upon revising the manuscript.

Reviewer: Line 246 – Replace "indicates: : :" with "suggesting a freshwater environ-

ment"

Response to reviewer:

We agree with this alteration and will incorporate it into the revised manuscript.

Reviewer: Line 284 – However there is a large SL fall after MIS 3, so you might expect a lot of erosion during this time. No deposition during MIS 3/2 is simply absence of evidence rather than evidence in itself.

Response to reviewer: We agree with this point and need to update our discussion to clarify our interpretation of the figure.

Reviewer: Line 310 – grammar in sentence "It remains possible: : :"

Response to reviewer:

Yes, we agree and we would change the wording to, "It remains possible that strong freshwater flux out of the Sea of Marmara through the Dardanelles Strait would keep out marine water by a few meters. In such a scenario, RSL would still have to be low, maybe a few meters above the paleodepth of the Dardanelles Strait at most."

Reviewer: Line 321 – RSL index is not a conventional term, you might mean RSL indicator. Essentially you want to say that this set of data suggests RSL ranged from X-X mbsl. But, do you mean the seismic data suggest this? These are not really ocean shorelines; the elevation of lake shorelines cannot be used to represent a sea level shoreline. I would be very skeptical of interpreting sea level from lake levels because there are a number of other factors that control lake elevation including precipitation, for example. If you are inferring this from the geochemical data can you explain exactly where those numbers (70-90 m) come from (depth of sill?). This needs to be much clearer, as this is a main conclusion of the paper. However, as the paper stands I am not sure what data this sea level is inferred from, and cannot determine how robust this conclusion is.

Response to reviewer:

We are using three indicators to draw our main conclusion with the illustration below: (1) geochemical data; (2) seismic lake shorelines; (4) depth of the paleosills; (3) seismic imaging of Gemlik lake. The geochemical data suggests no marine entry occurred. The seismic shorelines suggest that the lake was consistently at 80 mbsl with a positive hydrological balance leading to outflow from the Black Sea and Sea of Marmara to the Mediterranean Sea. If the Mediterranean Sea was connected, then it would have flowed into the Sea of Marmara and the Black Sea and the level of the paleoshorelines would have been higher and the water chemistry would also have changed. None of this is observed suggesting saltwater did not enter into the Sea of Marmara and into the Black Sea. With this we draw the conclusion that the RSL on the eastern Mediterranean side of the Dardanelles strait, had to remain below 80 mbsl. Illustration below:

Also, sill depths are approximate here, just to indicate our argument and our point.

Reviewer: Line 374 – This is uncited and it is not clear how the authors estimated this.

Response to reviewer:

This is part of the conclusions, perhaps we should leave the 10-20 m estimations out of this discussion and just say lower. We ran a GIA model before and this was the result given by the model.

Reviewer: Line 583 – Figure 3 Based on this figure I feel confused about the conclusion. Which proxies are supposed to be the same in both water bodies if there is freshwater? Also, there is not a record during most of MIS 3 for all proxies in both water bodies, so it is not clear that it makes sense to compare these proxies (for example Sea of Marmara does not have Cl measurements during MIS 3 and near does Sr for either water bodies).

Response to reviewer:

In the revised manuscript we will clarify which of the values on the figure indicate fresh-

ness and which marine.

While not all proxies go into MIS 3, the porewater Cl- (b), left side certainly does and goes into MIS 4 as well. It is the red diamonds in the figure.

Reviewer: Line 605 – Figure 4. What is the point of this figure? Is this an area that matters or are we interpreting a paleo shoreline? I am not sure I understand the significance of a shoreline in Gemlik Bay as this is far from the connection between the Marmara and Black Sea.

Response to reviewer:

This is a perched lake. It is a lake that is located in the Sea of Marmara (Figure 4). The point of this figure is to show that there was no accumulation of marine sediments into this lake during MIS 4 and 3 and this lake was disconnected from the Sea of Marmara. This lake is separated from the deeper parts of the Sea of Marmara by a ridge. This is just an additional piece of evidence indicating the level of the lake remained at its paleoshoreline of 50-60 m while the local lake level in the Sea of Marmara was 80 mbsl. The lake was fed by river water and discharged into the Sea of Marmara before it connected with the Sea of Marmara at 12,000 years (Vardar et al. 2014). After that, sediments that accumulated were marine and were also dated by a series of cores (Gasperini et al. 2011, Taviani et al. 2014, Filicki et al. 2017). We need to include the latter to strengthen our argument.

Reviewer: Line 641 – Are there cores taken in the profile in Fig 5a? What are the dates? It would be great to include the dates next to the location of these. Are these figures from another study? I think that this could be said at the beginning of the figure caption. It is confusing that these might be from two separate papers so the terms are not the same. For example, an erosional surface is shown in Fig 5b, but these aren't highlighted in Fig 5a. It would be great if the terminology in the captions were consistent across both figures.

Response to reviewer:

There were no cores taken from profile 5a. Both 5a and 5b are from another study and this is said in the figure caption, “(a) Succession of superimposed prograding clinoforms adopted from a previously published illustration (Fig. 11c) (Aksu et al. 2002)…” and “adopted from an earlier published XIX profile retrieved from R/V Hydrograph in 1998 (Genov 2015).”

We will update the terms in the revised paper to make the two figures more easy to follow and complement each other.

Review references:

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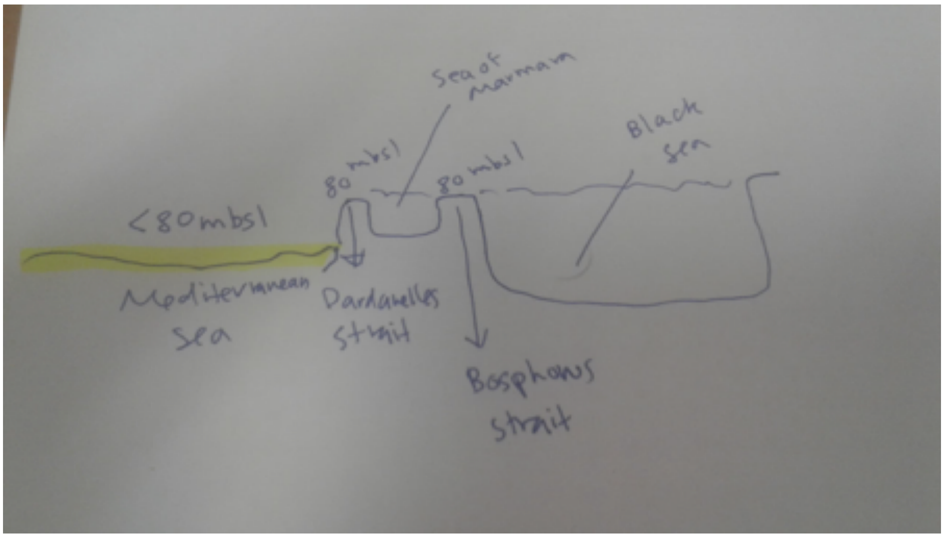

Fig. 1.