Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-30-RC1, 2019

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 #1

Received and published: 12 April 2019

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 ($\sim$ 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 be-

low. 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

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. 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.

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).

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. 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. 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.

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). 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. 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.

Line by line comments

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

Line 17 – what do you mean by persistent?

Line 26 – eustatic is not in caps

Line 38 – replace ‘elevations” with “estimates”

Line 39 – delete “existence of”

Line 40 – delete “factors that control”

Line 41 – delete “additionally”

Line 43 – relative sea level not in caps

Line 45 – “The earth is moving” is imprecise language. This sentence should be written
Line 47 – delete “the”

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

Line 53 – (3) is missing from this list

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 18O

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

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

Line 91 – Confusing sentence

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

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?

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?

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?

Line 134 – need reference!

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

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

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

Line 195-196 – Confusing sentence

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

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

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

Line 207 – delete "evidence of", delete "here"

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.

Line 215 – Confusing discussion

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

Line 236 – Delete "Index"

Line 246 – Replace "indicates. . ." with "suggesting a freshwater environment"

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.

Line 310 – grammar in sentence "It remains possible. . ."

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.

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

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).

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.

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.

References:

Chappell, J. (2002). Sea level changes forced ice breakouts in the Last Glacial cycle: New results from coral terraces. Quaternary Science Reviews, 21(10), 1229–1240.

http://doi.org/10.1016/S0277-3791(01)00141-X

Cann, J. H. (2000). Late Quaternary Paleosealevels and Paleoenvironments Inferred From Foraminifera, Northern Spencer Gulf, South Australia. The Journal of Foraminiferal Research, 30(1), 29–53. http://doi.org/10.2113/0300029

Cabioch, G., & Ayliffe, L. K. (2001). Raised Coral Terraces at Malakula, Vanuatu, Southwest Pacific, Indicate High Sea Level During Marine Isotope Stage 3. Quaternary Research, 56, 357–365. http://doi.org/10.1006

da Silva Salvaterra, A., Cesar, R., Figueira, L., & Mahiques, M. M. De. (2017). Evidence of an Marine Isotope Stage 3 transgression at the Baixada Santista , south-eastern Brazilian coast. Brazilian Journal of Geology, 47(December), 693–702. http://doi.org/10.1590/2317-4889201720170057

Simms, A. R., DeWitt, R., Rodriguez, A. B., Lambeck, K., & Anderson, J. B. (2009). Revisiting marine isotope stage 3 and 5a (MIS3-5a) sea levels within the northwestern Gulf of Mexico. Global and Planetary Change, 66(1–2), 100–111. http://doi.org/10.1016/j.gloplacha.2008.03.014

Hanebuth, T. J. J., Saito, Y., Tanabe, S., Vu, Q. L., & Ngo, Q. T. (2006). Sea levels during late marine isotope stage 3 (or older?) reported from the Red River delta (northern Vietnam) and adjacent regions. Quaternary International, 145–146, 119–134. http://doi.org/10.1016/j.quaint.2005.07.008

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