Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/esd-2022-2-RC2, 2022

Comment on esd-2022-2

Anonymous Referee #2

Referee comment on "Process-based estimate of global-mean sea-level changes in the Common Era" by Nidheesh Gangadharan et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2022-2-RC2, 2022

General comments

This manuscript produces a new analysis of global-mean sea level change over the common era using process-based modeling with an examination of thermosteric and barystatic (Antarctic, Greenland, and glaciers) contributions through time. The authors compare their modeled GMSL with proxy reconstructions of global sea level and find general agreement, although the model-based estimate underestimates 20th century GMSL. They find that glaciers acted as the dominant source of GMSL changes during the common era; however, the uncertainties were large especially in the last millennium.

The paper is generally clear and well written and while there are some large uncertainties in the results, it is valuable to have new process model-based estimates of GMSL to compare with proxy reconstructions and to further understand the relative contributions of processes driving GMSL changes over longer timescales through the common era.

I would recommend the manuscript to be published in Earth System Dynamics if the following several points could be addressed to improve the discussion of the results and comparison with proxy reconstructions. My comments focus on these aspects of the paper, as I cannot expertly comment on the intricacies of the process modeling methods themselves.

Specific comments

The last paragraph of the introduction mainly refers to analysis during the PCE (except for Ln 71 which says “changes over the CE”) which is inconsistent. However, the results and discussion do cover the entire CE, not just the PCE, so I would suggest altering the text

accordingly.

Because the authors clearly state questions in the introduction that the paper will attempt to answer (Ln 71-73), I would expect clearer answers to each of these questions in the discussion or at the conclusion of the paper. Especially concerning the major sources of uncertainty – while the large uncertainties are referenced throughout the paper, it would be helpful to clearly state the sources of these uncertainties at the conclusion of the paper and suggestions for how to minimize them in future work.

Section 2.4 could be strengthened to explain the proxy-based reconstructions of global sea level – such as the proxy data that was used, the basic methods with spatiotemporal modeling. Specific details like Ln 361-365 describing the different curves could be moved to section 2.4 instead. It would also be helpful to more completely explain the Kopp/Kemp/Walker global reconstruction – that it is an estimate of global sea level via the signal common to all of the sea-level records in the Common Era proxy database. It is therefore the "globally uniform" term among sites from the spatiotemporal model, and not exactly an estimate of GMSL. The Kopp/Kemp/Walker method could give a true estimate of "GMSL" in the presence of spatially complete data.

The descriptions of the proxy-based reconstructions of global sea level need to be corrected. In Ln 361-365 describing the methodological constraint, it is correct that Kemp et al. (2018) used this constraint. However, Walker et al. (2021) also utilized this constraint so this needs to be corrected in Ln 365. The constraint was used for all of the analysis in Walker et al. (2021) and the global curve shown in that paper uses the constraint. A supplemental figure in Walker et al. (2021) shows the global curve without using the constraint – which is the curve that is shown in this paper in comparison to the process model estimate. This needs to be made clear throughout this manuscript and in Figure 4. Alternatively, Walker et al. (2022) could be referenced, which did remove the constraint for the analysis and so the global sea-level results do not include the constraint – this would be the equivalent global curve to what is actually shown in this paper.

Walker, J.S., Kopp, R.E., Little, C.M. et al. Timing of emergence of modern rates of sea-level rise by 1863. Nat Commun 13, 966 (2022). https://doi.org/10.1038/s41467-022-28564-6

In Ln 339-342, could the authors speculate as to what would cause the differing response of the Greenland and Antarctic ice sheets to surface temperature changes? Or provide any references that also support these findings?

In Ln 360 (and throughout the paper) I think it would be more clear and helpful to refer to "reconstructions" as "proxy-based reconstructions" instead.

In Ln 405-409, first a positive contribution is related to GMSL rise in Ln 405, meaning a negative contribution is related to GMSL fall in Ln 406. So how is in Ln 408-409 "All the GMSL components except Antarctic ice sheet have a *positive contribution to net GMSL fall* during 1200-1800 CE" supposed to be interpreted?

I understand the uncertainties and limitations using the process-based model, but I find it difficult to put too much weight on the results for the PCE, when the 20th century global sea-level estimates are inconsistent with reconstructions and observations and are underestimated to a degree that there is not even overlap within the uncertainties. If the model was altered/improved to match the observations/reconstructions in the 20th century, how would this change GMSL and the relative contributions of driving processes (especially glaciers) over the rest of the PCE? Can a more formal list of improvements be recommended to address this discrepancy? How much of this is due to the initial conditions in the model and is there a way that these could be adjusted? I think these questions need to be addressed more completely in the discussion.

Technical corrections

Ln 49, 50, 361, 365: these should reference Walker et al. 2021, not 2020

Ln 424: 'focused' spelled incorrectly

Figure 1: the caption says the Zanna et al., (2019) reconstruction is blue, but it is green on the figure

Figure 4b: would be helpful to show the uncertainties in the rates for the Kemp/Walker/Neukom curves