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Comment on esd-2022-2

Anonymous Referee #3

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.,
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A review of "Process-based Estimate of Global-mean Sea-level Changes in the Common Era" by Nidheesh, Goosse, Parkes, Goelzer, Maussion, and Marzeion

The authors use models to quantify thermosteric effects from ocean heat storage and barystatic effects from land ice changes on long-term global-mean sea-level (GMSL) fluctuations during the preindustrial Common Era (PCE). They compare their results to proxy reconstructions of PCE GMSL changes from Kopp, Kemp, and Walker. One of the authors' main conclusions is that glaciers made dominant contributions to GMSL changes during the PCE.

I'm a sea-level scientist with training in physical oceanography. I don't have expertise in modeling land ice. Thus, I restrict my review mainly to sections on thermosteric effects, and recommend the editor solicits reviews from experts in ice-sheet and glacier modeling.

I really liked this study. Something like it has been needed for a while. The past decade has seen real advances in the community's ability to quantify PCE GMSL changes from proxy reconstructions (from the likes of Kemp, Kopp, Walker, and others). But there's been a total lack of modeling studies to complement those observational studies. This paper starts to fill that gap: it won't be the last word on the subject, but it takes the logical first steps, and therefore deserves to be published after some minor revisions.

Specific comments

References to Walker et al. (2020) should be to Walker et al. (2021)

Section 2.1. The authors describe how they estimate global-mean thermosteric sea level from PMIP3/CMIP5 temperature and salinity. Why not just use the zostoga global-mean thermosteric sea-level diagnostic variable made available by several PMIP3/CMIP5 groups? Also, the authors should identify precisely which PMIP3/CMIP5 model simulations they use (they only identify GISS-ES-R; were other models used?).

The authors consider LOVECLIM, PMIP/CMIP models, and the reconstruction of Zanna et al. (2019). Is that all of the relevant data sources for ocean warming and thermosteric effects during the Common Era? Are there other ocean reconstructions that could also be brought in to corroborate the story they're telling?

Section 2.5.2 Uncertainty on rest of the processes. I find this whole section unclear, ad hoc, and arbitrary. Can the authors please explain more the basic rationale and provide references for their methods when possible? In particular, I'm confused what their uncertainty quantification is supposed to represent. What missing process do they imagine they're accounting for by adding the autocorrelated noise, for example?

Section 3.1

Line 266ff. Can the authors speculate on the high-frequency (decadal) global-mean thermosteric variability apparent in the LOVECLIM solution that is unrelated to volcanism? Is it related to ENSO or another global mode of natural climate variation (e.g., Hamlington et al., 2020, PNAS)?

Lines 283ff. Can the authors speculate on the mechanisms of these changes and when or why upper-ocean and deep-ocean effects may be opposing or reinforcing? More generally, some discussion of the physics involved, rather than just a tabulation of numbers, would be informative.

Line 290. What are the plus/minus values?

Line 302 and elsewhere. Why the italics on Roman Warm Period? Also on the next line it should be Antiquity not Antique.

Line 305ff. Is LOVECLIM model output available to say more about what drove these multicentennial changes? Were they the long-term effects of volcanism? Changes in insolation? Again, some physical insights would be useful.

Line 373ff. The more muted variability in the Walker et al. (2021) results relative to the Kemp et al. (2018) result may be owing to different prior assumptions made in the two studies with regard to dominant timescales of variability (i.e., what time smoothing is implied by the respective versions of the empirical spatiotemporal model).

Line 393ff. Have the authors identified why their model results with respect to global-mean thermosteric versus barystatic contributions during the twentieth century differ so greatly from estimates of Slangen et al. (2017; *Surveys in Geophysics*) and Frederikse et al. (2020; *Nature*)? The authors point out the differences several times, but it would be good to know why these differences exist, and whether they bear on the confidence we have in their simulations of the PCE.