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Geoscientific
Model Development
Discussions

Interactive comment on "ISSM-SLPS: geodetically compliant Sea-Level Projection System for the Ice-sheet and Sea-level System Model v4.17" *by* Eric Larour et al.

Anonymous Referee #3

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A review of "ISSM-SLPS: geodetically compliant Sea-Level Projection System for the Ice-sheet and Sea-level System Model v4.17" by Larour et al. for possible publication in Geoscientific Model Development.

The authors present a new geodetically compliant approach for modeling future sea-level rise due to ocean thermal expansion, ocean circulation changes, water mass redistribution, and glacial isostatic adjustment. This new approach has the relative advantage compared to previous approaches (e.g., Kopp et al. 2014, 2017) that coupling and interaction between contributors are taken into account. The authors highlight the important result that, by modeling Greenland ice mass loss using 18 basins rather than

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Discussion paper

1 basin, uncertainties on future sea-level rise are substantially reduced (Figure 11).

I'll confess that, while I study sea level and was invited to review the paper, I'm a physical oceanographer. I don't have the expertise in modeling, geodesy, or glaciology needed to give a thorough review of this paper. I'd strongly recommend the editor to ensure experts in these topical areas weigh in on this paper.

That being said, I appreciate the paper. I think it's really valuable that the authors are pushing the envelope and developing flexible, modular, coupled approaches to model the various contributors to future sea-level rise and their uncertainties.

I have no major issues with the manuscript (though, again, I strongly recommend more expert reviewers weigh in). I have a couple editorial remarks, detailed below. My only one real complaint regards terminology. The authors' use of gravitational, rotational, and deformational (GRD), sterodynamic, and barystatic sea-level contributions (cf. Equation 1) can be inconsistent with definitions in Gregory et al. (2019). I'd recommend the authors either (1.) adopt the definitions used in Gregory et al. (2019) or (2.) acknowledge where their definitions diverge from Gregory et al. (2019) to avoid confusion.

Specific comments:

Line 2: paramount to -> important for

Line 4: cost and timing -> coast, timing, and risk tolerance

Line 23: are summed -> are modeled separately and summed

Equation 1. Comparing to Figure 3 in Gregory et al. (2019), I'm confused by this equation. The equivalent equation in Gregory et al. would be:

relative sea level = sterodynamic sea level + gravitation, rotation, deformation (GRD) + barystatic - inverted barometer

In Gregory et al. (2019), GRD includes GIA, and GRD makes no contribution to global-mean sea-level changes. What the authors here call "GRD", Gregory et al. (2019) call "contemporary GRD".

Anyway, it's fine that the authors here use slightly different terminology. But they should acknowledge where their definitions diverge from Gregory et al. (2019). Otherwise, readers (i.e., I) will get confused.

Line 37: local thermosteric -> global-mean thermosteric

Line 53: qualities -> quantities

Line 61: "stays constant in time" it's unclear what the authors mean by this phrase

Line 107: paramount to -> important for

Lines 124-126: I'm unfamiliar with studies doing this for projection purposes. Do the authors have a reference in mind for this technique?

Line 127: can drive redistribution -> can be coupled to redistribution

Line 128: causes -> manifests in

Line 129: cause a change in the load of -> load

Line 130: SAL effects. Suggest to reference, e.g., Ray (1998), Stepanov and Hughes (2004), and/or Vinogradova et al. (2015) on these points.

Line 131: Please add "made by atmosphere-ocean general circulation models (AOGCMs)" after projections and before the Richter et al. (2013) reference

Line 148: local -> global-mean

Line 152-153: The authors should clarify whether they remove the global-mean OBP value or not. If not, are the authors making the Greatbatch correction to account for the Boussinesq nature of most CMIP AOGCMs?

Line 165: The authors should precisely define the ocean function O(theta,phi) for clarity.

Line 176: The former -> These

Line 193: Please define BAMG on first use

Figure 3 caption: thsi -> this

Line 246: alphas -> alpha is

Line 256-257: "We display ... the average" I don't understand this sentence, but maybe it's just me.

Line 262: "the KOPP14 ... SLPS framework" Unclear. Are the authors saying that the approach here reduces to and reproduces the Kopp results under certain strong assumptions? Please clarify.

Line 289: please add spaces between -1.65, sigma, to, 1.65, sigma

Line 291-292: "DSL is not ... CMIP5 NorESM-ME runs" Why only one model and why this model? Variance in model projections of DSL changes can be large and important locally.

Line 303: "Bayesian exploration approach" The authors reference such an approach several times, but never explain it or give a reference.

Line 311: "the tails are much larger for the 1 basin scenario" This seems like a very important results, but I don't think the authors have discussed it enough for me to understand physically why this is the case. Suggest to consider adding more of a description.

Line 311: the "likely" (5-95%) range -> the width of the "likely" (5-95%) range

Line 325: siginificantly -> significantly

Line 343: urther -> further

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