1 Response to Anonymous Referee #3

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 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 rec- ommend 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.
We thank the reviewer for the time spent on the review and for his valuable insights, especially from the Physical Oceanography point of view. We agree with the reviewer's assessment of the advantage of flexible, modular, coupled approaches to model various contributors to future sea-level rise and their uncertainties. We also agree that our initial explanation of the STR and DSL terms were not compatible with the definitions in Gregory et al. (2019), and will definitely tighthen the introduction in this respect. All othere referees pointed to the same issue (see in particular referee #1). We below go through all the comments and try and address them, along with modifications to the manuscript that will be carried out if the editor goes forward with requesting a new version.

Specific comments:

- Line 2: paramount to $->$ important for
  Thank you for the suggestion, we will adopt it in the new manuscript
- Line 4: cost and timing $->$ cost, timing, and risk tolerance

Thank you for the suggestion, we will adopt it in the new manuscript

- Line 23: are summed $->$ are modeled separately and summed
Thank you for the suggestion, we will adopt it in the new manuscript

- 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.
We really appreciate the referee checking against Gregory et al. (2019) and seeing this inconsistency. We will definitely remark in the manuscript on the differences in our approach, and refer to our GRD as contemporary GRD. Here is the new paragraph that will be in the manuscript starting at line 37: *Note here that our definition of GRD is not completely in line with Gregory et al. (2019), as GIA is considered as a separate contributor, and the GRD contribution does contribute to global mean sea-level changes. It is rather in line with the definition of contemporary GRD in Gregory et al. (2019).*

- Line 37: local thermosteric $->$ global-mean thermosteric
This was picked up by all referees, and can lead to confusion about the definition of STR and DSL. Thank you for spotting it, we refer to the referee #1 comments on how we addressed this

- Line 53: qualities $->$ quantities
Thank you for the suggestion, we will adopt it in the new manuscript

- Line 61: "stays constant in time" it's unclear what the authors mean by this phrase
We will replace by *is constant through time*

- Line 107: paramount to $->$ important for
Thank you for the suggestion, we will adopt it in the new manuscript

- Lines 124-126: I'm unfamiliar with studies doing this for projection purposes. Do the authors have a reference in mind for this technique?
Referre #1 also requested a reference. There is not one involving a projection, but we provided (eg. Thompson et al., 2016, Fig.3) for a good explanation of the approach that could readily be adapted to a projection.

- Line 127: can drive redistribution $->$ can be coupled to redistribution
Thank you for the suggestion, we will adopt it in the new manuscript

- Line 128: causes $->$ manifests in
Thank you for the suggestion, we will adopt it in the new manuscript

- Line 129: cause a change in the load of $->$ load
Thank you for the suggestion, we will adopt it in the new manuscript

- Line 130: SAL effects. Suggest to reference, e.g., Ray (1998), Stepanov and Hughes (2004), and/or Vinogradova et al. (2015) on these points.
Thank you for the suggestion, we will adopt it in the new manuscript and reference these studies

- Line 131: Please add "made by atmosphere-ocean general circulation models (AOGCMs)" after projections and before the Richter et al. (2013) reference
Thank you for the suggestion, we will adopt it in the new manuscript

- Line 148: local $->$ global-mean
Thank you for catching this typo that was also important to all three other referees. We have corrected the manuscript accordingly.

- 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?
Thank your for the comment. Indeed we remove the global-mean OBP from ocean models, since ocean dynamics don't add or remove any mass from/to the ocean. The CMIP5 and CMIP6 models all (should) have applied the Greatbatch correction, as confirmed by the fact the global-mean value of the 'zos' fields is zero, and we use the 'zostoga' to the models to get the global thermosteric rise. We will add the following paragraph starting at line 155: *"Note also that the global-mean OBP is removed from the ocean models, since ocean dynamics don't add or remove any mass from/to the ocean. In addition, our projections rely on CMIP5 and CMIP6 fields 'zos' (the sea-surface height change above geoid, or DSL term) and 'zostoga' (global average thermosteric sea-level change or STR) where the Greatbatch correction has been applied, resulting in a zero global-mean value of STR."*

- Line 165: The authors should precisely define the ocean function O(theta,phi) for clarity.
Thank you for spotting this issue, we will define the ocean function in the manuscript succinctly as *O=1 for oceans and zero otherwise"*

- Line 176: The former $->$ These
Thank you for the suggestion, we will adopt it in the new manuscript

- Line 193: Please define BAMG on first use
Thank you for the suggestion, we will define BAMG in the manuscript

- Figure 3 caption: thsi $->$ this
  Thank you for spotting the type, we will correct it in the new manuscript

- Line 246: alphas $->$ alpha is
  Thank you for the suggestion, we will adopt it in the new manuscript

- Line 256-257: "We display ... the average" I don't understand this sentence, but maybe it's just me.
  This sentence can indeed be clearer, we replace with *"We display the average thinning rate μ, $\mu + 3\sigma$ and $\mu - 3\sigma$ (for an arbitrary value of the standard deviation $\sigma = 5\%$)."*

- 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.
  We are indeed saying that the approach is equivalent to KOPP14 if we use the same partitioning. The assumptions are not so strong, just that the partitioning be the same. However, as demonstrated by Fig. 11, this is not the case anymore once several basins are introduced. We will try and capture this better in the manuscript, with the following statement: *"Once several partitions are adopted however, the refinement in the fingerprint patterns significantly departs from the KOPP14 approach. "*.

- Line 289: please add spaces between -1.65, sigma, to, 1.65, sigma
  Thank you for the suggestion, we will adopt it in the new manuscript

- 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.
  In this demonstration of the capabilities of ISSM-SLPS, we wanted to approach the geodetic angle. Significant variance in model projections are indeed found in the CMIP5 and CMIP6 benchmarks, which completely occultate any other variance from any other inputs. We wanted to avoid this. We ask the reviewer to allow for this exception, as we believe it leads to a better validation of our capability.

- Line 303: "Bayesian exploration approach" The authors reference such an approach several times, but never explain it or give a reference.
  We agree with the reviewer. The GIA statistics relied upon here are from Caron et al. (2018), but the bayesian framework we refer to is described in Caron et al. (2017) and is based on a bayesian inversion method using Simulated Annealing (Kirkpatrick et al., 1983), a variation of the Monte Carlo with Markov chains (MCMC) method (Metropolis and Ulam, 1949; Metropolis et al., 1953). We will better refine the description in the manuscript and give extended citations. The paragraph will now read *These statistics were evaluated using bayesian inversion method based on Simulated Annealing (Kirkpatrick et al., 1983), a variation of the Monte Carlo with Markov chains (MCMC) method (Metropolis and Ulam, 1949;*

Metropolis et al., 1953). They can be used directly in SLPS, either during a standard probabilistic projection run, or a posteriori as is the case here. These statistics reflect the statistical fitness to a global GIA dataset composed of paleo-RSL indicators and vertical GPS trends.

- 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.
  We agree with the reviewer. The reason for reduced tails is that by multiplying the number of basins, we recompute fingerprints that are more reflective of the true spatial pattern. The example of New York used in Larour et al. (2017) helps in understandig this feature: the entire South-East Greenland contributes zero sea-level change in NY. If a basin is positioned over this entire region, it will contribute zero variance to the PDF distribution for SLR in NY. This leads to a reduction in the tails of the distribution. We will add this explanation in the manuscript too, as suggested. Here is the text we will add starting at line 317: *" This can be visualized better by taking the example of New York, where following Larour et al. (2017) contributions from South Greenland are almost negligible. This implies that all the basins (and corresponding GRD patterns) in South Greenland will contribute zero variance to the PDF for RSL at New York. This will therefore result in smaller tails for projections that rely on more refined basins."*

- Line 311: the "likely" (5-95%) range $->$ the width of the "likely" (5-95%) range
  Thank you for the suggestion, we will adopt it in the new manuscript

- Line 325: siginificantly $->$ significantly
  Thank you for spotting the typo, we will correct the manuscript accordingly

- Line 343: urther $->$ further
  Thank you for spotting the typo, we will correct the manuscript accordingly

References

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