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Interactive comment on "A mass conserving formalism for ice sheet, solid Earth and sea level interaction" *by* Surendra Adhikari et al.

Anonymous Referee #3

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The paper by Adhikari et al. presents a formalism to calculate the contribution of dynamic ice sheets to mean sea level by considering a changing sea-level, bedrock and land-ice-ocean mask. Compared to standard methods the contribution to mean sea level is computed from ice thickness and not from the height above flotation. The formalism is valid on all timescales, but the full benefit is on longer timescales. As there are currently huge efforts to include dynamic ice sheets in Earth system models the presented paper is a good reference.

Generally, I found the paper well written and structured with illustrative and clear figures. The paper is worth publishing in The Cryosphere but as the focus is rather technical it would fit much better to GMD. The current form of the manuscript lacks a bit in the presentation and clarity. Therefore, I have a few suggestions that should be

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Discussion paper

addressed in a revised version.

1) I found the title a bit misleading to the content of the paper. I am missing the definition of “mass conservation” in the text. I also would like to see (e.g. with an example), if the formalism is mass-conserving (or better mass-conserving than traditional or other methods). Also, the only example in the manuscript was about calculating SLE relevant thickness changes rather than mass-conservation.

2) The Introduction should refer to Goelzer et al. (2020). You do very quick comparisons to Goelzer et al. (2020) on page7,line6 and 23, but I think the Introduction should clearly say what you are doing differently and why. This could be a motivation to release your new formalism. An appropriate discussion to Goelzer et al. (2020) is also missing. Additionally, from the Introduction it was not really clear to me what is actually wrong with the traditional methods, the order of error on SLE they could introduce and what you are now aiming to improve.

3) Not sure if this could be really addressed, but it would be interesting to estimate the errors (i.e. traditional versus your formalism) of current projections of SLE from the two big ice sheets e.g. within the ISMIP6 framework (Antarctica: Seroussi et al., 2020; Greenland: Goelzer et al., 2020b). Or from current remote sensing products like IMBIE (Shepherd et al., 2019). My point here is, that I would get a better feeling for the error on e.g. different timescales, regional settings and how it differs for Greenland and Antarctica. The example based on the Larour et al. (2019) simulation is very helpful (see also my comment to P7,l29ff) but very specific - and, as I understood – not in line with current projections efforts. I do not strictly insist that you show an error for ISMIP6 or IMBIE, but as also commented below, I would like to have a better error estimate and its impact on current research (compare Fig. 3 in Goelzer et al. (2020)).

Minor comments:

P1,l8: I have not found in the text, which computational strategies you have simplified. What do you mean with computational strategies?

P2,l11: Why are “floating ice shelves, ice rises and rumples, and retrograde bedrock slopes” complex features?

P2,l13: What is” traditional theory for ice-bedrock-ocean interface changes“?

P2,l18: “ ... that can be straightforwardly employed in any Earth System model . . .”. I think it would be worth to mention (somewhere), if this new formalism could be adopted to other disciplines (e.q. remote sensing, standalone ice sheet modelling). In the current form, it sounds the formalism in only valid/applicable in ESMs.

P4,l23: I cannot see from Eq. 4 that your new setup diverges from traditional approaches. Eq. 4 is a very common equation to define an ice-mask. On page5,line 25 you give another example of how the traditional setup differs from your setup. Maybe outlining the differences could be gathered together.

P5,l8: I don’t understand this sentence. What is negative?

P5,l9: “. . . hence contribute to sea level inversely.“ Maybe say sea-level drop/fall to avoid confusion.

P5,l10: I am not a native speaker, but are both “evolving” really needed?

P5,l25: Can you add a reference to a Figure after “show”?

P7,l4: “. . . and the elevations”?

P7,l29ff: I would first describe the Larour et al. (2019) setup and then present the results. Can you give an integrated value (e.g. SLE) for both approaches to get a better feeling for the error? The simulations were run over 500 years. Why do you choose to present the results after 350 years?

P9,l6ff: The following paragraphs and Equations appeared very suddenly and without introducing their purpose. According to the title, I would expect Eq. 10 (the mass-conserving field M) is the main point in your paper. But this is not illustrated and somehow contradicts with your statement in the conclusion (p9,l31-32); here you say

DeltaH_s is the main point. This is perhaps personal matter, but I found it a bit brutal to stop the results of the paper with these equations. An illustrative example on "implication of this new geometrical setup for sea level and solid Earth loading studies" (your comment on p2,l20) would make more sense to me.

Fig.3: What is the grey line? And in the caption: Is "conventional"==" traditional"? I guess yes. Please use the same wording in the whole text. Eq. 3 and 4: consider rewriting with "latex-cases"

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