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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 #1

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* Summary

The paper presents a formalism to geometrically interpret changes in ice sheets and underlying bedrock and their combined effect on the ocean and sea level. The approach defines two distinct domains (land and ocean) that can both intersect areas of ice cover. It then traces ice and bedrock changes and transitions between different domains to determine the sea-level contribution of the ice sheet.

* General comments

The paper is well written, clearly structured and deals with the important question of how to calculate the sea-level contribution of a marine ice sheet, among others. I believe it would make an interesting contribution to The Cryosphere given that the

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points raised below are addressed adequately.

One of the main conclusions of this paper reads very similar to the one in Goelzer et al. (2020), both papers proposing an alternative to conventionally calculating the sea-level contribution of marine ice sheets based on volume above flotation. it seems important to clarify what the similarities and differences are. The results presented by Goelzer et al. (2020) imply that ice and bedrock changes have to be considered together at least in any place where ice could ground over the course of the experiment. This is the direct consequence of the claim that the sea-level contribution calculated from one point in time to another should be independent of what happens in between. Since the example that is put forward (see their Fig 2a and related text) matches with regime 3 here, there seems to be a direct disagreement between the two approaches: bedrock changes are taken into account in their case, but not here. This may point to a flaw in the approach that should be clarified and discussed. If bedrock changes are not considered as part of the ice sheet change in regime 3, what other component is taking it into account (if any) and how does the domain separation between those components work? If both approaches are not compatible, why and under what circumstances do they differ? What would needs to be changed to make the two approaches compatible? Are the two approaches addressing a different modelling framework, which explains the differences?

While the first few words in the abstract seems to say that this paper is about representing ice sheets in models, the mention of geodesy later may suggest that observations of ice sheets are equally addressed. If the aim is indeed modelling, I think this should be made clear and a clearer distinction be made from observations. If both should be addressed at the same time, I suggest to make sure that the presented formulations make sense in both realms.

While I appreciate the formal description of the case, I miss better guidance of the reader through what is a difficult problem to understand and visualise. Recurrent redefinitions of variables (see example S(w,t) below) should be avoided, individual terms

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in the equations should be better explained and examples should be given where possible. This particularly applies to cases where the formalism uses familiar concepts and applies them to something else (e.g. floatation condition for ice applied to define the ice free coastline).

For understanding and reproducing the results it would be useful to provide access to the data and tools used to produce the results and plots in Figure 3. Please consider making the geometry and scripts available.

*Specific comments

P1.l1 Not all ESMs include ice sheet components. Reformulate.

P1.l2 The connection between ESMs and geodesy is not clear to me. E.g. observation don't exist for ESM paleo simulation where the formalism should also hold.

P1.l5 "grounded and floating masks" suggests a modelling perspective, but "as viewed from space" relates to an observational dataset. What is the perspective of this paper?

P1.l5 "Here we present ...". The subject in this sentence is not clear. Reformulate.

P1.L13-15 This is clearly true for simulations of the Antarctic ice sheet, but not really for the Greenland ice sheet, which is dominated by surface mass balance changes. Similar cases may also exist during other climates with ice sheets that were mainly land based. Suggest to reformulate.

P2.l2 "Defining geometry". Do you mean "Defining the bedrock"? How could the geometry be defined upfront for an intercomparison when the models are supposed to produce an evolving geometry?

P2.l5 What do you mean with "basic configuration setup" and "Similar setups"? Could you describe this in more detail?

P2.l12 Why are "floating ice shelves" and "retrograde bedrock slopes" complex features? They occur in the very simple flowline model setups you may be referring to

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above. Clarify.

P2.l13 What is the "traditional theory for ice-bedrock-ocean interface changes"? Clarification needed.

p2.l23 "setup" -> maybe "interpretation"?

p2.l24 What is the difference between "glaciers" and "ice sheets" in your description?Clarify if the two terms are interchangeable or distinct. If the latter, what sets them apart in your formalism?

p2.l24 I found the upfront separation between land and ocean confusing for your context, because it is not intuitive where that separation is to be made for a marine-based ice sheets. The definition what is to be considered land and ocean comes too late. I suggest to make that clearer much earlier.

P2.l25 Same for S(w,t), defined here first as the sea-surface elevation. How should we think of S for an ice shelf? Why not start with defining S as the geoid as you do later.

P2.l26 I admit, I had to look up what the ITRF is. For other readers not familiar with it, you may want to add a sentence or two to say what the ITRF provides. In practice, if I use Bedmachine data, is it registered on the ITRF or are you suggesting this is something the user would have to take care of herself?

P2.l31 So far S(w,t) is defined as sea-level. As such, any case B > S is not well defined. The interpretation of S as the geoid must come earlier for this to make sense.

P2.l32 You say here that S(w,t) includes high-frequency noise and variability, but on the next page you want S(w.t) to refer only to the quasi-static component of the sea-surface. Why not introduce S directly as the quasi-static component of the geoid, rather than going through three redefinitions along the way (sea surface -> quasi-static sea-surface -> sea-level -> geoid).

P3.l1 Remove "changing" before interactions?

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p3.l7 What is the "interior of marine ice sheets"? Clarify

P3.l9 Say and explain what F(w,t) means. Traditionally it determines if the ice is floating. But you seem to extend it to locations with H = 0? Maybe it would be worth it to mention that.

p3.l13 Please define what "open ocean" means and what "contact with the open ocean" means. This definition comes too late in the manuscript. Does w have to be part of O? Maybe we need the definition of O already before this part not on page 4?

p3.l14 A more obvious definition of a generalised coastline for me, that also exists in presence of a marine-based ice sheet would be the point where the bedrock and the geoid intersect (1|2 in Fig2). That doesn't help for your formalism, but it goes to show that it is not immediately obvious to think the coastline at he grounding line. Better guidance needed.

P3.l8-15 I found this paragraph difficult to follow. You start with reference to Fig. 2a, where our focus is on the left hand side and with F(w,t) which suggests it is about ice. But then you describe R(w,t) and the coastline, which are difficult to visualise in a place with an ice shelf. It may help to guide the reader by being explicit about the two 'generalisations' that take place here: floatation criterion (for ice) –> definition of the coastline (everywhere). grounding line (for ice) –> coast line (everywhere).

P4.l1 I don't see why there could not be a grid point in a model with B=S and H>0. A glacier terminating on land or on a sill exactly at sea-level? Please clarify.

P4.l6 The fact that neither O nor L are defined at the grounding line seems problematic. How can your formalism be mass conserving when grounding line grid points in an ice sheet model are not part of these masks? How do you track the grid cells that fulfil this condition, do they form a separate category? Why would it not matter to consider them?

P4.l9 Remove "deep" and "well". I suppose the condition could also be true for shallow

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troughs with bathymetry moderately below sea-level.

P4.l10 While I understand the use of this connectivity concept in your formalism, I find it problematic in practice. It means that small changes in ice or bedrock can lead to very large changes in O and L. In an unfavourable configuration, the short term grounding and ungrounding of a critical point could e.g. switch an entire system of connected fjords on and off.

p4.l18 With the above, combining the grounded and floating ice masks leaves a hole at the grounding line. Is this desired?

P5.l13 The geoid typically changes first, then the bedrock. Maybe re-order in the sentence.

p5.l22-23 I am confused about this sentence. Isn't "quantifying the fraction of ice mass change that contributes to sea level" exactly what you are doing by defining dH_{S} below? Reformulate?

p5.23 "the assumption" appears three sentence back, maybe refer to it more specifically.

p5.23 Remove "all the time".

p5.26 "As we show below". This has been shown before by others (see references). Reformulate to avoid confusion.

p5.l30 Can you please explain what the three terms mean physically. E.g. the first term accounts for thickness changes of ice that is and remains grounded ...

p5.l30 Could you explain why H0 appears as an *absolute* contribution in the third term compared to considering *changes* in H_{S} and H_{F} in term one and two?

p6.l6 Not clear what "holding in the areas of on-land ice margin migration" means. Reformulate.

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p7.l15 I suppose you mean that the nonzero ΔH_{F} is compensated by other terms in Eq. 7. Which ones? This is important to understand.

p7.l22-23 This sentence may be confusing because Goelzer et al. (2020) consider not only transitions between grounded and floating ice, but all three regimes. Reformulate.

p7.l26 Important to note that in regime 3 bedrock changes can contribute to sea-level change, even if you are not considering them as part of the ice sheet change. Also important to realise that ice floating at t and t+Δt may have been grounded at some point in between. I think this makes your solution dependent on the time stepping. See Fig 2a in Goelzer et al. (2020) for an example of such a case.

p8.l7 The change in ΔH_{S} and ΔRCI(Δt) is not only due to ice mass changes, but also due to bedrock changes under the ice. It may be good to mention that here.

p8.l8 For clarity, could you mention why potential changes in ocean area from O(w,t) to O(t+Δt) do not matter for ΔRCI(Δt) in Eq 8? I suppose the underlying assumption is that we should be interested in sea level at time t+Δt?

p8.l12 G(w,t) is not explicit in Eq. 7 only implicitly by evoking Eq. 6. This could be mentioned.

p8.l14. Maybe remind us what R(w,t) is here, as it is only introduced inline and back at p3.

p8.l15 To make this equation more digestible, maybe start with combined symbols. E.g. by combining all the barystatic components like you do in the text.

p9.l1-24 My experience with the paragraph including Eq. 9 is that a lot of new concepts are suddenly thrown in here without much preparation. Especially the idea to separate the effect of the past from the contemporary would profit from some more introduction. Maybe a new section with a few introductory sentences could be started p8.l6 to prepare the ground for this discussion.

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p9.l3 Since ΔRV (ω,Δt) may be the component that takes into account bedrock changes in regime 3 and is complementary to the bedrock changes happening under the ice, I would be interested to see how it is calculated and how the masking works in that case.

p9.l28 By your definition, the grounding lines are neither part of the grounded ice nor part of the floating ice domain. What is this category called and how are you accounting for it?

p10.l4-5 How does your approach compare to/ differ from that proposed by Goelzer et al. (2020).

Figures

Figure 1. It seems confusing to introduce lakes, subglacial lakes and proglacial lakes and than not consider them at all. It would make the figure much clearer to remove them.

Figure 2. The geoid (and S) is also defined over land. Please add in both panels.

Figure 3. Please include a panel with ΔH_{F}. This is important as it is discussed as the conventional method and appears in the difference in panel c. Please also add contour lines to delineate the regions 1-3. Mention that regime 4 does not exist in this region if that is true. Otherwise, delineate regime 4. If different from results in Goelzer et al (2020) it would also be interesting to add a comparison as figure here.

References Goelzer, H., Coulon, V., Pattyn, F., de Boer, B., and van de Wal, R.: Brief communication: On calculating the sea-level contribution in marine ice-sheet models , The Cryosphere, 14, 833–840, https://doi.org/10.5194/tc-14-833-2020, 2020.

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