The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-23-RC2, 2020

The Cryosphere
Discussions
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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 #2

Received and published: 14 March 2020

Summary

This paper presents a framework for describing the geometry of an evolving ice sheet margin in Earth system models, in which the geometry of the bedrock and mean sea level also can be dynamic. The authors define relevant terms and give a mathematical description of the way different quantities are related. In particular, they quantify the portion of ice thickness change that contributes to changes in ocean mass and global mean sea level. As Earth system modelers work to integrate dynamic ice sheet models with solid-Earth and sea-level models, this paper will be a useful reference.

In general, the paper is well written, and the figures are very helpful. Sometimes, however, technical terms related to sea level are used without precision, or are introduced without giving enough background information. Some of the equations contain terms

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that are not clearly defined or explained physically. In the comments below, I suggest where the exposition could be improved, especially for readers approaching these concepts for the first time.

Also, the text refers to “traditional” or “customary” approaches of estimating sea-level contributions from ice sheets based on the change in height above flotation, in contrast to the approach described here. It would be helpful to see some specific examples of customary approaches from published papers, with estimates of the magnitude and sign of the associated errors. This would enable readers to better assess the value of the proposed formalism.

Major comments

p. 1, Title: The title includes the words “mass conserving”, but in the text I did not find an operational definition of what this means in an ESM with evolving ice sheets. Please provide such a definition, and perhaps an example of how mass conservation would be violated.

Also, the title implies that there will be a detailed analysis of ice-sheet interactions with solid Earth and sea level, but the actual scope seems narrower: to accurately compute the contribution of dynamic ice sheets to barystatic sea level rise (i.e., the sea-level component associated with a redistribution of mass between land-based ice and the ocean) in ESMs. I suggest a revised title that better reflects the scope.

p. 1, Abstract: The abstract is very general and does not provide a clear sense of the scope of the paper. If a central goal is to describe how to accurately compute the ice sheet contribution to barystatic sea level rise, then this goal should be clearly stated.

p. 1, l. 1: Although I am fully in favor of including dynamic ice sheets in ESMs, I would not go so far as to suggest that “any Earth System model” already includes them. For some ESMs, ice sheets are still on the back burner.

p. 2, l. 2: “Defining geometry”. I am not sure what this means–something like defining geometric concepts that are relevant to models with dynamic ice sheets?

p. 2, l. 4: “future debate and reconciliation.” Ideally, the concepts are set forth clearly in a way that leads to greater mutual understanding and less debate.

p. 2, l. 5: “basic configuration setup”. I am not sure what this means. It seems an odd way to describe what seem to be theoretical or analytical frameworks.

p. 2, l. 8: I am not clear why these previous analyses are referred to as “traditional configurations.” The word “traditional” suggests a contrast with something novel and untraditional to be introduced here. However, I don’t see this work as heading off in a different direction from the cited papers, but rather as clarifying concepts that are particularly relevant for ESMs. As above, “configuration” doesn’t seem to be the right word.

p. 2, l. 13: Similarly, what is meant by “traditional theory for ice-bedrock-ocean interface changes”?

p. 2, l. 25: “sea surface elevation”. This is an ambiguous term; it can refer either to the mean (on some appropriate time scale) or to a quantity that varies on short time and spatial scales. There is some explanation below, but it is better to be as clear as possible when introducing the quantity S(omega,t). I think that what is meant here is what Gregory et al. (2019) call “mean sea level”, a term they recommend in place of the deprecated term “mean sea surface.” If S is actually meant to represent the geoid, which is not quite the same as mean sea level, then this should be stated clearly.

In general, Gregory et al. (2019) is a comprehensive, carefully written reference. I suggest that the authors adopt similar terminology, paraphrasing and referring to that paper as appropriate.

p. 2, l. 26: What is the International Terrestrial Reference Frame, and how is it defined? Is it similar to what Gregory et al. (2019) call the reference ellipsoid?

p. 2, l. 32: It is stated first that S is highly variable in space and time, and then it is stated

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Discussion paper

that S is quasi-static and does not, in fact, include short-term dynamic processes. Please use S only to refer to quasi-static mean sea level, and use a different term when discussing short-term dynamics.

p. 3, l. 5: “Sea level” is another ambiguous term, as discussed by Gregory et al. I suggest “mean sea level.”

p. 3, l. 5: “represents an equipotential surface whose spatial pattern mimics the geoid.” This is confusing. First, how is the geoid defined? Gregory et al. define it as the geopotential surface chosen so that the volume between the geoid and the sea floor is equal to the time-mean volume of sea water (including the liquid-water equivalent of floating ice) in the ocean. Second, what is meant by “mimics” the geoid? Does “mimics” mean “is equivalent to”, or “is similar to”? If the latter, in what way does S(omega,t) differ? If S is mean sea level, then it is not an equipotential surface; for instance, mean sea level has a higher geopotential on one side of the Gulf Stream or ACC than the other. (Though it could be convenient to define S as an equipotential surface in areas not covered by ocean.)

p. 3, l. 11: Since ocean and ice have variable density, it would be clearer to refer to rho_o and rho_i as reference densities.

p. 3, l. 12: “sea surface relative to the seafloor”. I suggest “local mean sea level relative to the seafloor”.

p. 4, Eq. (3): Why are the ocean and land functions undefined at coastlines and grounding lines? Is it problematic not to include them in one domain or the other?

p. 4, l. 7: Please say precisely what is meant by “connected to the open ocean”. I would guess that the connected regions include marginal seas (e.g., the Mediterranean) but not inland lakes (e.g., the Great Lakes). Also, one needs an operational definition of the open ocean before defining a connection to the open ocean.

p. 4, l. 17: “is connected to” is more precise than “is in direct contact with”

p. 4, l. 23: Are there published examples of frameworks (i.e., "traditional theory") that cannot handle pinning points? It seems natural to define the ice domain as in Eq. (4), and I'm not aware of frameworks with a different or less natural definition.

p. 4, l. 24: Which "employed assumption" is being referenced here? Maybe the assumption that only ice that is part of the ocean domain is included within the floating ice mask?

p. 4, l. 27: "the first generation of Earth system models". I'm not sure we are still in the first generation, since ESMs have been around for about a decade. Maybe "current Earth system models".

p. 5, Eq. (6): Why is the grounded mask needed in this expression, if it is true that H = H_0 for floating ice shelves?

p. 5, l. 8: What is the referent in "it can be negative"?

p. 5, l. 21: What is meant by "directly affects"? Is this just the (fairly trivial) statement that some, but not necessarily all, of the net change in ice mass results in a change in ocean mass?

p. 5, ll. 21-22: "Quantifying the fraction of ice mass change that contributes to sea level. . ." I thought that this was the main point of the section. In what sense is it analytically unapproachable and beyond the scope of the study?

p. 5, l. 23: "Despite the assumption. . ." Which assumption? If the reference is to the assumption that "the net change in ice sheet mass directly affects the ocean mass", I'm not sure there is a contradiction, but I'm not clear on the precise meaning of the assumption.

p. 5, l. 27: "...yields some error." After reading this statement, I was expecting to see quantitative error estimates later in the text. There is an illustration in Fig. 3, but is it possible to state the typical order of magnitude of the error? For instance, is it closer to 1% or 10%?

p. 5, l. 28: Please say precisely what is meant by “contributes to sea level”. For example, if an ice sheet loses mass, the geoid will change because of gravitational and rotational effects, but these changes aren’t part of DeltaH_S. What is meant, I think, is the part of the ice loss that adds to the mass of the ocean, i.e. the barystatic sea-level component. If so, then barystatic SLR and related terms should be defined here or earlier.

p. 5, l. 30: Since this is a central equation in the paper, I would like to see a clearer description of the physical meaning of each term, and when appropriate a derivation. I convinced myself that the first two terms on the RHS are correct, but I was not able to derive the third term or understand the physical motivation. In the text, the closest thing to an explanation is on p. 7, l. 20: “The last term in the equation accounts for the fact that fresh water density evolves during the accretion and ablation of ice, whereas the average ocean water density in the vicinity of the grounding line acts to determine the ablation height.” This is confusing, in part because freshwater density rho_w is a physical constant that does not evolve. Please provide a clearer explanation and, if possible, a supporting figure.

p. 6, Figure 2: The figure and caption are helpful, especially panels a and b with the four different regimes.

p. 7, ll. 3ff: The text refers to three distinct “regimes”, whereas Fig. 2 refers to four regimes that are defined differently from the regimes in the text. Please use the term “regimes” consistently. In the text, paragraph 1 corresponds to the first term on the RHS of Eq. (7), and paragraph 2 to the second and third terms. But as stated above, the explanation of the third term is not clear. Perhaps revise so that paragraph 2 addresses the second term and paragraph 3 the third term. Then the current paragraph 3 would become a short paragraph 4.

p. 7, l. 19: Could you give an example of when the magnitude of the change in H_F would be equal to the magnitude of the change in H, and when it would be less?

p. 7, ll. 29ff: I am confused about the difference between events in regimes 1 and 2. My understanding is that regime 1 consists of regions that are grounded at both the start and end of the simulation, whereas regime 2 consists of regions that transition from grounded to floating during the simulation. If so, this should be stated clearly. âĂć For regime 1, it is stated that DeltaH_S is different from DeltaH_F because of “evolving bedrock and sea level”. Are bedrock and sea level not evolving in regime 2? Or is the point rather that in region 1, the ice remains grounded throughout the simulation, and therefore the entire DeltaH contributes to DeltaH_S, whereas DeltaH differs from deltaH_F because of bedrock changes? âĂć For regime 2, it is stated that the discrepancy is due to the “missing fraction of newly grounded or newly floating ice.” I am not sure what this means. I understand why DeltaH_S differs from DeltaH in this region, but not why DeltaH_F differs from DeltaH_S.

p. 7, l. 30: Please say more precisely what is meant by the “customary approach of using DeltaH_F.” Can you cite specific examples in the literature in which the ice-sheet contribution to SLR was derived from DeltaH_F, yielding a significant error? In the literature (beyond this specific example from Larour et al. (2019)), do the errors have a systematic sign? Are these errors prevalent in ice sheet models that include isostatic adjustment (i.e., where DeltaH_S could have been computed accurately, but DeltaH_F was reported instead)? Or is the problem that most ice sheet models ignore isostatic adjustment, so that they are missing a key term needed to compute deltaH_S?

Also, can you state the magnitude of the systematic error? That is, what is the magnitude of the integrated error in Fig. 3 panel c, relative to the integrated value of deltaH_F?

p. 8, Fig. 3: In addition to the 2D fields, it would be useful to show a graph consisting of time series of the area-integrated values of H_S and H_F. This graph could show not only the total values, but also the values computed separately for regions 1, 2, and 3. Also, please cite Larour et al. (2019) in the caption.

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p. 8, l. 6: Here, barystatic sea level change is finally defined. I suggest introducing and defining this concept earlier in the paper. Also "ocean mass-related" is a bit vague; I suggest phrasing similar to that of N19 in Gregory et al. (2019): e.g. "the part of global-mean sea-level rise which is due to the addition to the ocean of water mass that formerly resided within the land area as land water storage or land ice." Then DeltaRˆI, introduced below, would be the land-ice contribution, and DeltaRˆL would be the contribution from other land terms.

p. 8, Eq. (8): The term on the LHS includes a subscript, a superscript, and an overbar, without immediately saying what these things mean. I suggest a more gradual and systematic introduction to the notation. Also, could you explain why the denominator contains rho_w instead of rho_o? At first, I assumed that the denominator represents the mass of the ocean, but I think the reason for rho_w is that we are converting a mass of fresh ice into an equivalent ocean volume, ignoring halosteric effects. Again, a more detailed physical explanation would be helpful.

p. 8, Eq. (9): This equation introduces several more terms without preamble, and the reader has to study the following paragraph carefully to translate each term. Please rewrite in a way that is gentler for the reader.

p. 9, ll. 7-9. I am not clear on the meaning of the third and fourth ("past") terms, and how these terms change the ocean mass. I understand that past ice-sheet changes affect sea level through ongoing glacial isostatic adjustment, but isn't GIA included in term 6, the vertical-land-motion term?

A more general comment: It could be easier for the reader if the text were organized from general to specific, instead of specific to general. That is, first define the various kinds of sea-level rise, introduce notation, and state the various source terms. Then state that this paper is focused on R_IˆC, as computed in equation (8) based on DeltaH_S. Finally, show how to compute DeltaH_S. This would be a fairly major rewrite, and I don't want to be too prescriptive, but it is challenging for readers to introduce basic concepts just a page or two before the conclusions.

p. 9, l. 16: When deltaM is described as a mass-conserving field, is this equivalent to saying that its global integral is zero? Also, is it strictly true that rho_i*deltaH_S is equal to the change in ice mass at each location? Here, I'm wondering about the third term in Eq. (7); is that term associated with a change in the local mass per unit area?

p. 9, Conclusions: As mentioned above, it would be helpful to quantify the benefits of the new methods, e.g. by estimating the errors associated with the older methods.

Minor corrections

p. 1, l. 6: "and include the ice shelves and adjacent ocean mass". The phrasing is awkward. Please use a parallel grammatical construction.

p.1, l. 7: "is" -> "can be"?

p. 1, l. 15: "grounding line" -> "grounding lines"

p. 1, l. 17: Delete "involved"

p. 2, l. 2: "first order" -> "first-order"

p. 2, l. 28: Delete "for"

p. 3, l. 5: "refer" -> "refer to"

p. 4, l. 30:"ice sheet driven" -> "ice-sheet-driven"

p. 4, l. 32: "far field" -> "far-field"

p. 5, l. 2: Add "the" after "estimate"

p. 5, l. 20: "farfield" -> "far-field"

p. 5, l. 26: Add a comma after "below"

p. 6, l. 7: "predict" -> "predicts"

p. 9, l. 15: Insert “the” before “ice sheet”

p. 10, l. 6: “analyses” -> “analysis”

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