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Interactive comment on "Brief communication: On calculating the sea-level contribution in marine ice-sheet models" *by* Heiko Goelzer et al.

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The paper aims to bring consistency to the approach (and potentially terminology) used in estimating sea level contributions (SLC) from model-based studies of ice sheets. In general it is successful in this, though I would like to see a clearly defined proposal for appropriate terminology. I think this is a useful contribution to the conversation on SLC due to ice sheets, and would recommend it to be published after some improvements.

The paper is in general fairly clear, but would benefit from a sentence or two at the start of each section providing context and motivation for the direction taken. The detail needs to be broken up with occasional text to orient the reader. I am not altogether sure that this article is short enough to be a "Brief communication", but that is for the

editor to decide!

As to the problem of computing SLC from an ice sheet model, it is not clear whether the paper is trying to say "here is the right way to do it, everyone should do it this way" or "here is one way to do it, think carefully about these issues and use consistent terminology when describing your approach". I would argue it should be the latter, as I think alternative approaches to some aspects of the SLC calculation could be justified. In any case, please try to make this a bit clearer, and if it is the former, the authors need to make stronger arguments (mainly regarding external sea level forcing) why this is the right way!

The paper recommends using the initial sea level as a reference level for calculating SLC. This is a reasonable recommendation, but no consideration is given to other options, such as choosing a different reference level (final sea level? time mean sea level?), or calculating SLC at each timestep based on current sea level (the details of this would probably depend on the numerical scheme being used). Can the authors clarify why their suggestion is the best one? Alternatively, can the authors acknowledge that this is one of several possible approaches that may also be valid? The whole paper is about using ice sheet model outputs to estimate a contribution to mean sea level. It would be good to see a paragraph that puts this in the context of the more complex real world picture. Specifically, there is the gravitational effect of redistribution of ice mass on sea levels. This probably causes a local decrease in sea level, and a far field increase. Also, coupled ice sheet – ocean (possibly also atmosphere) models might be able to simulate a spatial distribution of sea level change, in which it may not be trivial to distinguish between the ice sheet contribution and other effects, and the delivered product could be argued to be "superior" to global mean SLC predictions. Even in such a case, where the modelling approach allows a more complete prediction than simply mean sea level, it may be of value to calculate a mean sea level contribution in order to compare with other ice sheet models, and as such this paper can contribute to this situation also.

Specific comments:

Line 12. I suggest “differences” -> “change” because differences implies comparing two different properties but her I think you mean change over time. Equation 1. The way dx, dy and I,j are used seems to imply (thought this isn’t stated) summation over grid cells for a structured rectangular grid. Often in ice sheet modelling unstructured meshes are used, usually either 2D or extruded in the vertical. It should be easy to generalise the notation to all cases except for fully unstructured 3D meshes. Please also clarify in the text that this is an operation over the model grid/mesh.

Equation 4. I don’t see what this adds, I would leave it out. If you include it, you need to introduce V_gr.

Page 3.

Line 11. “Ice at floatation” is an odd expression. When there is “ice above floatation” then the “ice at floatation” is clearly grounded ice. So the expression is not intuitive. When we talk about “ice above floatation” we really mean something like “ice above a fictional surface at which, if it formed the actual upper surface, the ice column would be at floatation”. The fact is that all this ice is grounded, so talking “ice above floatation” and “ice at floatation” can be a bit confusing. Conceptually, we really mean “ice that can contribute to sea level” and “ice that can’t contribute to sea level”. I think I would find this discussion generally more . . .er . . . “natural” if we talked about a floatation thickness, and so the actual thickness can be above of below this floatation thickness. And ice above the floatation thickness can contribute to sea level whereas ice below cannot. And so bedrock uplift, which doesn’t directly impact on ice thickness, does directly impact on the floatation thickness. I am not going to insist on this because the choice of terminology is subjective. But part of the purpose of this paper is to present definitions and terminology with which to discuss ice sheet contribution to sea level rise, so I think some careful thought should be given to this, and my first reaction to “ice at floatation” was that it is somewhat non-intuitive.

Line 12. Perhaps would benefit from a summary sentence ending this paragraph to clarify that an uplift in bedrock will in general lead to SLC being underestimated if only the initial bedrock is used for calculating SLC.

Page 5.

Equation 5. Note that this only calculates ocean volume in the domain of the ice sheet model, which may be a limitation depending on what you want to do with it. I’ll read on and find out. . .

Page 6.

This seems a bit clunky. Equations 6 and 7 are only valid in certain situation depending on where the bedrock was or where it is going to. . . can you not go straight to one more generic equation, albeit slightly more complicated, that captures the change in h_af in general, as a function of change in bedrock (and perhaps also of change in thickness)? I am talking here about change just due to the direct bedrock effects, ignoring ocean volume change for the moment.

Line 17. What is a “sea level component”? Perhaps you mean something like “to convert a change in potential ocean volume to a sea level contribution”?

Lines 12-18. It seems that the plan here is to calculate changes in pov? So this would be a purely bedrock contribution, separate from an ice dynamic component.

Equation 9. Right, so now I can see that the limitation of calculating this on the ice model domain is simply that we assume all bedrock adjustment occurs within the ice model domain. This is probably a reasonable and practical assumption to make, but you should clarify that this assumption is being made, with the implication that any study attempting to include bedrock adjustment in an ice sheet model study aimed at projecting sea level rise should endeavour to consider both the extent of the ice sheet itself and of the region over which bedrock adjustment could occur when defining their model domain.

Lines 22-23. “changes in ice mass occur in reality with a density of freshwater”. This is a strange sentence. Changes initially occur either at the density of ice (calving) or from ice density to fresh water density (melting), so it is not really correct to say that changes occur “with a density of freshwater”. You might want to say something like “Ice loss from the ice sheet ultimately contributes to the ocean with a density of fresh water”.

I would also definitely leave out precipitation because this will usually be in the form of snow, initially with a much lower density than ice, and I don’t (yet) see why you would need to talk about precipitation?

Page 7.

Equation 10. I don’t think V_den has been clearly defined in the text? Also, I am not clear why this is kept separate from the V_af calculation. I suppose I must be missing something, but since we’ve already calculated V_af in equation 1, why don’t we just modify equation 2 like this: SLE_af = V_af/A_ocn * rho_ice/rho_freshwater So now we calculate the volume of freshwater being added to the ocean. This is much simpler than what is suggested in the paper. . . why is this wrong? Probably I missed something...

Line 17. What is “t2i”? Just t2 I guess?

Page 8.

Equation 15. This is not “objectively correct”, but rather is one way of making the calculation. If externally forced sea level at the end of a simulation is not the same as it was at the start, then should the simulated ice sheet contribution to sea level take into account this change or not? It seems to me one could make justification for different ways of doing this. I think a more objective approach in this paper might be to define terminology for different ways of making this calculation rather than to prescribe what appears to be presented as the “correct” way to do it.

Figure 3. The text is a bit on the small side, and I have to zoom in a lot to see the

subscripts in the legend. Can this be made bigger?

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