

Interactive comment on “The land-ice contribution to 21st century dynamic sea-level rise” by T. Howard et al.

Anonymous Referee #2

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Review of “The land-ice contribution to 21st century dynamic sea-level rise”, by T. Howard et al. Ocean Sci. Discuss., 11, 123–169, 2014

General Comments

This manuscript shows how land ice melt influences local dynamic sea-level rise. It presents the results of a climate model that is forced with freshwater fluxes from 2 land ice melt scenarios (medium-range and high-end). This is done in combination with a pre-industrial control run forcing and an adapted A1B scenario for the 21st century. The authors find that the DSL pattern scales according to the freshwater input, but is independent from the warming scenario, and may therefore be linearly added to the DSL from other components.

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It is the first paper that I know of that shows departures in DSL in a climate model as a result of a realistic land ice melt for all land ice sources and for the 21st century. This has so far been one of the unknowns in projection studies that combine multiple sources of regional sea level change, and is therefore an interesting topic. Although the maximum local departure of DSL due to land ice is rather small compared to other contributions to regional sea level change, I think that it is good to show that there is an effect, and to quantify its magnitude. This certainly warrants publication, but I do think that there are some improvements and clarifications needed. Most of these have to do with clarification, but there are also some conclusions that I'd like to see supported more strongly by the results.

Overall, it seems that a lot of words are needed before the authors finally make their point. It wasn't until I reached the conclusions before I had a clear idea of the actual point of the paper. Admittedly, this might be partly on myself, but as I think that as an author you should try to guide the reader through the paper a bit more than you're doing now. I then needed to read back to also understand more of the middle part. On the positive side, this means that the conclusion is well-written. On the other hand, it means that the rest of the paper can be improved. First of all, by writing up some parts of text more efficiently (mainly Intro and sect 2-3), and by clarifying short or vague statements, as specified in the comments below.

Specific Comments

P124,L5: DSL is not the only type of local departure from GMSL due to ice melt, gravitational effects are at least as important. Although they are mentioned in the introduction, in the abstract it sounds as if DSL are the sole cause for local deviations.

P124,L11 evolution in space or time? Or both?

P125, first paragraph: some reference(s) to regional projections paper(s) might be in place here; Milne2009 does not provide projections.

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Please clarify: are the ice sheet scenarios existing (P128,L9-10) or developed specifically for this study (P128,L15)?

In P128L20 the freshwater fluxes are applied to HadCM3, and in P129L2 the fluxes are derived from ECHAM? This requires clarification. Do you mean that the ice melt contributions are determined with ECHAM and then implemented as freshwater fluxes in HadCM3? The subsequent sections (2.1-2.3) do not exactly make matters clearer: while GIS is projected using ECHAM5, this is not specified for G&IC and AIS

P134L4: Why a simplified version of A1B, can you provide a reason for this? Also, the GIS (and presumably G&IC and AIS) are modelled with the full A1B, doesn't this lead to discrepancies?

P135L16: Please define 'three member ensemble'. How is it set up, what are the differences between the three members? I presume they are three different parts of the control run(?), but it should be stated explicitly since a lot of analysis follows from this ensemble.

P135L25: 'the last 100 yr, when the forcing is strong' – the last 100 yr of the control run or of the ice-melt scenario? I presume the latter since the control run is not supposed to have any external forcing, but this should be clarified.

P136L15-16: Do you mean to say that the control run without land ice gives no DSL pattern? Please clarify.

There are a couple of situations in which the authors claim that there is similarity between patterns and conclude that therefore the patterns can be scaled for different ice melt scenarios (P138,L16 – fig 5) or used on top of different climate scenarios (P142,L7 – fig 11 vs fig 3), but the similarity/scaling is not really shown. Yes, maybe the patterns generally look alike, but it is very hard to judge by eye whether they actually scale, as the authors suggest. I would therefore like to see the ratios or differences between these patterns to strengthen these conclusions – especially since these end up in the

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final paragraph & conclusions of the paper (P145L13).

Related to this, the final conclusion of this paper is that all climate models may adopt the DSL pattern to add to their respective DSL patterns, because the linear addition works for this model. However, this relies on the rather strong assumption that all climate models have a similar response in DSL to freshwater forcing. Since the DSL without this additional land ice freshwater forcing is already rather variable (see e.g. Yin2012), and also the response to additional freshwater forcing varies (e.g. Swingedouw2013 shows different sensitivities to freshwater forcing for different models), this seems a very bold conclusion to make, as it might very well not be true. Please discuss this. Or maybe consider to add 'in HadCM3' to the title.

The use of 'case studies' and 'scenarios' throughout the paper seems at times to even confuse the authors. Is it ice-melt scenario or ice-melt case study? Or ice-melt case study scenarios (header sect.2)? Also, scenario can point either to climate or to land ice. It would be very helpful if clearer distinctions were made. Pick a term and stick with it.

Sect7: One thing I'm missing here is how this fits in the bigger picture. Yes it's small (L23) but how does it compare to other regional sea level contributions? How important is it to include this effect (or is it important?)? might also be added to P124,L15: how does DSL compare to the change due to gravitational effects?

P144L23: 'the mean DSL change' due to ice melt or in general? Probably the former, but please specify.

Table1 1; This Table needs MUCH more explanation, because it doesn't make sense at all. What is A1B(m), A1B(s), PI climatology, PI(s)? All these things are barely explained or not even mentioned in the rest of the paper.

Fig 6b; These blue lines appear a little..out of the blue.. I suppose these are the 'independent samples' from the text? Why 8? Also, the point that is made in the

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last sentence of the caption is probably a point you should probably make in the text instead.

Technical Comments

P124 L10: global mean sea P125,L10: Pritchard P125,L29: using -> use a full Stokes model to project P126,L13: take out “,which is” to make the sentence clearer

Fig 1 caption: it would not contribute

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