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The Cryosphere
Discussions

Interactive comment on "The GRISLI-LSCE contribution to ISMIP6, Part 1: projections of the Greenland ice sheet evolution by the end of the 21st century" *by* Aurélien Quiquet and Christophe Dumas

Anonymous Referee #1

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This paper is clearly written and the figures are good. It describes the results of following the ISMIP6 Greenland experimental protocol with a particular dynamical ice-sheet model. Although this is information of use to assessing uncertainties in projections, the scientific gain is not clear. It would be useful if the authors could emphasise scientific lessons we learn from studying this model in particular, beyond its inclusion in the ISMIP6 comparisons, for example? Looking at the conclusions alone, I think a reader who is familiar with the literature of the last several years would find nothing new or surprising, for instance. However, in the paper there are a few new things which ISMIP6

is helping to clarify, and there are moreover useful things which have or could be done with this model, because it is computationally cheap, to test sensitivities.

A few of my comments relate to the importance of the SMB forcing, which the paper demonstrates. It would be useful to quantify (graphically or in numbers) how much of the spread among GCMs and scenario is due simply to the time-integral of the SMB forcing (as applied to the ice-sheet model), and not affected by the ice-sheet model itself. While it is certainly necessary to use a dynamical ice-sheet model to study large changes in ice-sheets, it would be useful if the authors could present evidence for the need to use one for the 21st century (when not coupled to the atmosphere or ocean), especially as doing so introduces complications of drift and spinup, as described by the paper.

I have some concern about the prescription of the large melting near the edge (o4 line 12) and the retreat masks (p4 line 32). With both of these enforced, is the dynamical behaviour of the model distorted?

p1 line 10-11. I would not jump to such a strong general conclusion (also on p7).

p1 line 17-18. I don't think that this statement (of a most likely contribution of 1 m from ice sheets by 2100) is a correct representation of the current state of scientific knowledge. In the first place, you can't state a likelihood independent of scenario, since there are no probabilities for scenarios. Bamber et al. write "For a +5degC temperature scenario, more consistent with unchecked emissions growth, the [median and 95-percentile] are 51 and 178 cm, respectively." I'm not sure what "most likely" means, but 1 m is twice their median. Also, Bamber et al. report an expert elicitation, whose reliability is debatable since it's opaque. For comparison, the AR5 assessment of the likely range of ice-sheet contributions by 2100 under RCP8.5 is 0.09 to 0.28 m from Greenland and -0.08 to 0.14 m from Antarctica.

p2 line 1. Why "asymptotic"?

p3 line 4. I suppose that strictly you could say an ice-sheet model satisfied momentum conservation, but as far as I know this model and others used for such purposes do not contain terms for acceleration or inertia. That is, momentum is always negligible, and they assume a balance of forces at all times.

p3 Eq 1. I think that BM is a single quantity, isn't it? Typeset like this in a formula it looks exactly like the product of two quantities B and M (like Ubar H is a product). It would be clearer to use a single symbol. Is it just the surface mass balance, or is basal mass balance included too?

p3 line 11. "the total velocity is simply to superposition of the two main approximation". I would suggest "the total velocity is the sum of the velocities predicted in their respective areas by the two main approximations".

p3 line 15. "for which there is infinite, respectively none, friction at the base." I think this should read "for which there is infinite friction at the base or none, respectively." "None" is a pronoun, not an adjective.

p4 line 5. How accurate are the SMB and the surface topography in the control state?

p4 line 11-14. Does this term strongly interfere with, or even overwhelm, the simulated discharge across the grounding line?

p4 line 24. State that these are vertical gradients. I would say that they are vertical gradients of surface quantities in the atmosphere model, rather than in the atmosphere.

p5 line 11. branched to -> branched from.

p4 line 21-22. What do you need the surface temperature for, if you're using SMB as forcing?

p5 line 22, p8 line 3, p11 line 11, Fig 5 caption. Although the reader may sympathise with the authors, it's better to avoid "pessimistic" and "optimistic", which are value-judgements.

p5 last para. I don't understand the reason for these two experiments. Do they start from the same initial state? Since they have the same forcing, they ought to evolve identically.

p5 line 34. alike -> like.

p6 line 22. best -> better.

p6 line 24. "In doing so" means doing what? - absolute or logarithm? I would have assumed logarithm, but the next sentence suggests otherwise. What are the units of 0.55? What are the units of velocity before taking the logarithm? (Strictly you can only take the log of a dimensionless quantity, but the conversion factor between different velocity scales will be a constant offset in the log so doesn't affect its RMSE, I suppose.)

p6 line 30. Why is this "on the contrary"? If I read this correctly, all the errors are in the same direction (too slow in the model). Can you suggest the reason for this systematic bias? What implication does it have for projections?

p7 line 4-5. What implication will this bias in SMB have for projections?

p7 line 9 and 15. Are these large drifts in thickness and velocity related? What effect will they have on projections? It's not obvious that you can simply subtract an unforced drift when it's large compared with the forced response.

p7 line 18. start by -> start with.

p7 line 21-24. Presumably this spread comes mostly from the spread in SMB forcing from the GCMs. Could you also add the ice-sheet area- and time-integral of the SMB perturbation to the graphs?

p7 line 21-24. It seems that these projections imply quite a low sensitivity to climate change compared with the models on which the AR5 was based; their assessment of the Greenland contribution by 2100 under RCP8.5 is 90-280 mm, of which 40-220 mm is from SMB change.

p7 line 25. What sort of "tipping point" do you have in mind, that you might see in the volume evolution? Can you give references to relevant suggestions?

p7 line 26-27. I think we should be more cautious in drawing conclusions. There are only four CMIP6 models considered in this study, out of dozens in total, and two of the four are at the edge of the CMIP5 distribution in your projections. Only two show much greater sensitivity, and those results are within the AR5 range.

p8 line 6-7. It's not the GHG itself which is the driver, but the warming it produces; that is also the reason why the rate of mass loss goes up with time, and the main reason for the spread among models.

p8 line 13-14. Since the point you wish to make is the similarity of the patterns, it would be better to show these maps divided by the integrated change in each case i.e. normalised to the same GMSLR contribution. That would reveal the patterns themselves, so they could be compared, which I agree should be the purpose of this figure.

p9 line 6. It would be interesting to see the time-integral of the applied SMB perturbation here, to compare with the AO experiments (as I also suggested on p7 for Fig 3). Any difference is due to the dynamical response to the SMB forcing.

p9 lines 16-23. The text says "Fig. 8b shows the difference in ice flux convergence in 2100", and the fig caption says "change in the dynamic contribution to ice thickness change in 2100". I don't think either of those is a correct description, if I have understood correctly. You also say, "This can be considered as the dynamical contribution to ice thickness change," which I think is correct. The quantity shown is the difference (change in topography during the experiment) minus (time-integral during the experiment of the local mass balance change with respect to control) - is that right? It would be useful to compare this difference with the change in topography in the same experiment, using the same color scale, in order to see the relative importance of the dynamical change. If it's a small fraction, you might argue that there's no need to use a dynamical ice-sheet model for projections on this timescale. Where it's not small, you

can comment. Part of the dynamical contribution near the coast is a response to the ocean forcing, I presume. Therefore it would also be useful to show the same comparison for the AO experiment. That is, would it be good enough to make the projection without a dynamical model, simply by time-integrating the local SMB perturbation?

p9 line 30. As a guide to the possible magnitude of this underestimate, you could state what the presently observed ice-sheet imbalance would give if it continued as a constant rate to 2100 and compare with your projected changes in response to forcing.

p10 line 4-16. This is useful, but it's not really discussion, I'd say. It's another sensitivity test, and it would go well in sect 3.2.3 about change in ice dynamics.

p10 line 20. Why is it necessarily an overestimate?

p10 line 27-29. Yes, it would! Since your model is particularly computationally inexpensive, please could you do it and tell us the answer? :-)

p10 line 31-32. Could you quantify the elevation-SMB feedback here, or earlier, and compare it with Edwards et al. (Cryosphere, 2014)? You could directly quantify it by running a sensitivity test in which the lapse-rate adjustment is excluded, I suppose.

p11 line 8. is systematically loosing -> systematically loses

Fig 1 caption. Does "respective to" mean "with respect to"? For clarify please state the years of the end of the historical and end of ctrl_proj.

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