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Climate of the Past Discussions

Interactive comment on "Modelling Northern Hemisphere ice sheets distribution during MIS5 and MIS7 glacial inceptions" *by* F. Colleoni et al.

Anonymous Referee #1

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This study aims at examining how different climatic background conditions affect glacial inceptions, taking the last two inceptions (∼Marine Isotope Stages 5 and 7) as case studies. A relatively-low resolution AOGCM forced by the relevant greenhouse gas concentrations and cycles of solar insolation is used to calculate surface temperature and precipitation fields for periods at, and just before, the times of inception. These climate fields are then used as inputs to an offline ice-sheet model, with the resulting ice-sheet evaluated primarily in terms of the potential effect on sea-level. The paper is pretty clear, appropriately detailed and well written, but the methodology, although not uncommon in the field, leaves much to be desired and rather limits what conclusions can be drawn about this interesting topic. Accepting the basic method, I think there are a few places where changes could make the results clearer, so I've recommended revision, overall.

I'm not convinced this study really shows us anything new. It seems that much of the detail you'd ideally like to know about in how the two inceptions differed gets swallowed in the limitations of the modelling setup, so it mainly underlines the first-order importance of ice-sheet–climate feedbacks during inception, and demonstrates that we can't model them terribly realistically given the biases and technical issues in our current models. That said, the results as they are have been analysed well, and the overall attempt to compare and contrast the simulations of the different inceptions could be of use to workers in the field.

As the authors clearly recognise (indeed, it's an obvious conclusion from their results), feedbacks between the climate and a growing ice-sheet are absolutely key to reproducing and understanding glacial inception. There are well-known practical issues in simply coupling climate GCMs and ice-sheet models at the appropriate spatial and temporal scales, which is why the authors, like many before them, resort to a series of rough parameterisations (the annual positive degree day surface mass balance scheme with fixed, global parameters, section 2.2) and adjustments (the method of producing a continuous transient climate forcing from two single steady states, section 2.2.1) to attempt to work around the problem. It's such a general problem that it would be unfair to single out this work for criticism on this count, but the approximation or exclusion of the very effects that they are attempting to investigate is unavoidably going to limit the conclusions that can be drawn, whatever the results look like. Climate models (of equivalent computational cost to the AOGCM being used here) with more sophisticated surface mass balance coupling are currently coming online, so there is hope for something different, if not better, in the near future. But, accepting the method that has been used here for now:

1) Would it be possible to see the sensitivity of the ice-sheet results to the choice of PDD parameters in the paper? If redoing a run or two is too onerous in terms of resources, could a quantitative idea be imported from the sources cited for the values? The exact numbers used, for the temperature lapse-rate particularly, can have a signif-

icant effect on the ice volume you can grow, and it would be good to know what sort of error bar just those parameters could put on the results. On this note, the authors might want to look at Gregory et al, Clim Past 8 1565-1580 (2012), who also looked at feedbacks during inception in a transient AOGCM–ice-sheet simulation - they found a very large sensitivity to the values of the parameters in the PDD scheme.

2) The climate state from CESM is applied to GRISLI without any attempt at removing the known biases of the present day climatology of the model. This choice of forcing is not unreasonable, but I think it is important to know what the effects of those modern biases really is. The relevant temperature and precipitation biases are shown (fig 6), but I would like to see what sort of ice-sheet grows when forced with the raw CTR1850 climate. Although it's not going to be a simple linear term that could be subtracted from the ice-sheets grown for the other states, it would be good to see how the model biases translate into ice-sheet anomalies for one, known state at least.

3) I found the description of the construction of the transient temperature forcing Trec in section 2.2.1 rather unclear. For one thing, equations 1-3 cannot be the exact story, as the units do not add up, and as the variables are described the only variation in time comes from Tindex. I found the role of the Tindex also unclear - I would expect it to modulate the interpolation between Ts1 and Ts2, so that the climate forcing at the beginning of the transient experiment was the same as in steady-state experiment 1, and the forcing at the end of the transient was the same as in steady-state experiment 2. As written, it appears that the Tindex term is simply added in, which means that the forcing at the end of the transient is instead rather different from that in the second steady state experiment. If this were the case, it would be no surprise that the transient runs ended up somewhere rather different to the steady state simulations of 115 and 229kyr - especially for the 115kyr case, where Tlic is large at 115k (fig 3). Whatever the case, could this be explained more clearly in a revision, please? As it is, I'm unsure how to view the results of the transient experiments.

4) For the transient run, two temperature indices are generated, one including only

the feedbacks actually present in the CESM steady state runs (Tgis) and the other attempting to include cryosphere impacts as well (Tlic). The differences between the two sets of runs are then used to denote how elevation and albedo feedbacks help the ice-sheets grow. The situation seems a little more complex than this to me. After all, the PDD scheme used to translate the climate into the SMB forcing already includes an elevation feedback term through the lapse rate adjustment. All of the runs thus include some cryosphere feedback to the climate forcing. Does this mean that part of the Tlic correction is double counting? Should it really be interpreted as showing the effect of large-scale cryosphere impact, rather than everything including local feedbacks? I guess I'd expect the albedo part of the cryosphere adjustment term to be more important anyway to be honest, but I think this could usefully be discussed somewhere in the text.

5) pg6239, line 25 states that the transient runs should, "theoretically", end up in the same place as the steady state runs at 115k and 229k. I don't think I agree with this. If the forcings in the transient run were changing slowly enough compared to the adjustment timescale of the ice-sheet that the transient run represented a series of quasi-equilibrium states then this might be true, assuming there were no hysteresis-like effects in ice-sheet growth (which we know there are). However the whole of the transient run is only 10kyr, with a climate forcing that changes significantly (the 125-115kyr Tlic index alone is 12K in amplitude), and I'd say the icesheet is unlikely to be in quasi-equilibrium under such a forcing. The simulation results bear this out - even the case of T115-GIS, which has a similar ice volume to SS115, has significant differences in ice-sheet topography. On a related note, the first paragraph on pg 6240 isn't very clear - could this be rephrased?

Interactive comment on Clim. Past Discuss., 8, 6221, 2012.