

Interactive comment on “Large ensemble modeling of last deglacial retreat of the West Antarctic Ice Sheet: comparison of simple and advanced statistical techniques” by D. Pollard et al.

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Overview:

The submitted paper presents results from a large ensemble of ice-sheet model simulations of the West Antarctic Ice Sheet through the last glacial termination and into the future. The ensemble aims to explore a broad envelope of parameter space, and two different techniques are employed to assess the results. As far as I can tell, the primary justification for the paper lies in the intercomparison of so-called 'simple' and

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'advanced' statistical techniques, rather than the presentation of realistic simulations of the deglacial and future states of the ice sheet.

Overall the paper is well-written and clearly laid out, with thorough explanation of the salient aspects of the study and sufficient reference to the preceding studies on which it builds. The figures are clear and effective. As a methodological paper it is clearly well-suited to GMD.

General issues:

I have detailed a few points lower down that I think need further explanation or clarification, but I have two more general issues with the manuscript as it stands.

Firstly, there are numerous (at least 8) instances in the text (p6 lines 22/23; p7 lines 18/19; p13 lines 19/20; p14 lines 8/9; p16 lines 1/2; p16 lines 17-21; p18 lines 6-8; p18 lines 23-25) where the authors refer to 'future work' that will either develop or change some aspects of the study as presented here. Whilst it is of course quite usual that submitted work forms part of a project that is ongoing, I found the repetition of these statements quite off-putting in the sense that they give the reader the impression that the current study is in some way 'incomplete', or worse still, inferior with respect to something similar that is being prepared for another journal (for example, the reference to Pollard et al., 2015b, which is a paper that is only 'in preparation'). I think the paper should be able to stand alone, and if important aspects of the study are either yet to be developed, or modified, then what is the rush to publish seemingly incomplete work here? Will the forthcoming papers build on this one, or undermine it?

The second issue I have with the manuscript as it stands is the inclusion of the 'future' scenario modelling. The title and majority of the paper deal with the deglacial, and since the primary purpose of the paper is to compare results from different statistical methods (for which any results would do) I see no reason to include the additional 5000 year experiments. They are barely discussed in the paper and have no relation to the deglacial experiments. Furthermore, as detailed below the basis for the 6C/2C

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air/ocean warmings is not clear. If they are arbitrary, then what is the justification for adding them to the end of a supposedly 'realistic' deglacial run? And if they are meant to represent a future emissions scenario such as RCP 8.5, then some explanation is needed to clarify why this is used rather than, for example, RCP 6 or any of the others. To my mind it looks like these data have been added to the paper somewhat opportunistically, rather than for any particular purpose. And by the authors own admission these simulations use a climate warming that is 'very simple' (p14, line 7), and the future simulations themselves will be presented in more detail in, once again, the forthcoming Pollard et al 2015b paper currently 'in preparation'. On this basis I think these arbitrary extensions to 5000 CE should be removed and saved for the other pending publications.

Specific points:

p6 - I think the justification for not using the 'drastic ice-retreat mechanisms' of Pollard et al 2015a should be more fully discussed. Either these mechanisms are necessary for realistic simulations (as argued in the EPSL paper), or not. Or do the processes only happen during warm periods and not cold periods? It seems that any complex statistical analysis of results is only useful if it helps reduce uncertainties, but if the largest uncertainty is ignored (ie uncertainty over the inclusion or exclusion of 'drastic' mechanisms) then the results are inherently biased. It would be useful to see how the results change when the 'drastic' mechanisms are included.

p7 - Liu et al 2009 present a transient run that ends at 14 ka BP, so what is used to drive the model from 14 ka to present?

p7 - what is the basis of the 6 and 2 C air / ocean temp increases? RCP 8.5 after 150 yrs equals c. 6 C air temp above present, but CMIP models suggest 6 C air would equate to 1.5 C in the ocean, not 2 C, which presumably could affect the results presented here? Similarly, the extended RCP scenarios define warming trajectories that increase steadily to 2300, and remain constant thereafter, rather than flat-lining at

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2150 as implied here.

p7 - since these "future" simulations are regarded as unrealistic, why include them? Particularly if the 'drastic ice-retreat mechanisms' aren't included.

p15 - 'Macintosh' should be 'Mackintosh'

Fig. 5 - y-axis label is 'sea level rise (m)', which implies that it is showing time-varying rates of change in sea level, but I think it is actually showing the change relative to present? Otherwise the value of c. -6 m from -20 ka to -15 ka could be read as indicating that the sea level was falling constantly by 6 m through that period.

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