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Interactive comment on "Pliocene Ice Sheet Modelling Intercomparison Project (PLISMIP) – experimental design" *by* A. M. Dolan et al.

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This submission outlines the PLISMIP experimental design and initialisation conditions for the Greenland and Antarctic ice sheets. The mid-Pliocene Warm Period is chosen to provide a testing ground for ice-sheet stability under a warm climate and document uncertainties associated with different ice-sheet modelling "frameworks/approaches" and perhaps (intent is a bit muddled here) "structural uncertainty in ice sheet models" . While overall I find this a potentially worthwhile endeavor, I have a number of issues with the experimental design and stated intents that I believe warrant attention.

The text starts with an intent of seeking to quantify differences between models but then explicity talks about addressing structural uncertainty:

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Interactive Discussion

Discussion Paper

"means that the model dependency of the results remains unquantified. In response to this, the Pliocene Ice Sheet Modelling Intercomparison"

"However, it is first necessary to understand the inherent differences that caused by structural uncertainty in ice sheet models." (note need to insert "are").

If the focus is simply trying to quantify uncertainty with respect available models, this should be made consistently clear. But there also needs to be a clear statement that this likely falls far short of capturing the structural uncertainty of currently available ice-sheet modelling systems.

A full assessment of ice-sheet model structural uncertainty needs to address a large range of uncertainties associated with missing processes (eg englacial and basal hydrology), representation of poorly resolved processes (eg basal drag, ice calving, pinning points under ice shelves, sub-shelf melt), and implementation of ice dynamics and thermodynamics (eg order of stress representation, numerical implemention of advection diffusion for thermodynamics,...). There are also a number of poorly constrained boundary conditions that can potentially impart large uncertainties (geothermal heat fluxes, parameters associated with basal drag and basal hydrology such bed porosity and till strength,...). Such an assessment is well beyond the scope of the proposed intercomparison. But, as detailed below, it will be important for this intercomparison to at least tabulate how all these issues are addressed by participating models.

I also offer the following Devil's Advocate argument that I would like this paper to address in the rational. It is hard to believe that the mid-Pliocene Warm Period over 3 million years ago is constrained in enough detail and spatial-temporal resolution from proxy data to adequately test ice-sheet and climate models with respect to warm world ice-sheet stability. The 20+ m (absoute) uncertainty in sea-level alone offers a poor constraint. Would it not make more sense to simply carry out experiments with results from CMIP experiments for future global warming? Or to carry out an Eemian experiment which with much closer temporal proximity should provide much stronger

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constraints?

I also have a number of specific comments on the experimental design (among other issues) as listed below.

Specific comments.

##The control simulations are an important component of the experimental design. Good to see them highlighted.

##The current litterature indicates that processes at the marine-icesheet/shelf interface are critical wrt ice stability. These strongly depend on local scale seasonal SSTs and on vertical marine temperature profiles. It's not clear from the submitted description whether the spatial resolution of the SSTs will be small enough to be relevant to accounting for such interface processes in the models.

"Through the comparison of a range of ice sheet models under the same boundary conditions and climatological forcing, PLISMIP will reconstruct the most likely geometry and volume of ice masses on Greenland and Antarctica. In doing so, PLISMIP will also address the issue of ISM dependency."

##How will the "most likely geometry and volume of ice masses" actually be reconstructed/determined? Raw averages? Results conditioned on available proxy data? This is a strong claim that either needs to be explicitly detailed in how it will be achieved and/or toned down.

"Eventually PLISMIP will use all of the data resulting from the PlioMIP experiments to help quantify the uncertainties introduced into mPWP ice sheet simulations when using a single GCM. However, it is first necessary to understand the inherent differences that caused by structural uncertainty in ice sheet models."

##I know from paleo icesheet modelling that the uncertainties in climate far outweight the impact of structural uncertainties in ice sheet models for such modelling. As such, doesn't it make sense to first document the largest sources of uncertainties?

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"sheets, and (ii) any large differences incurred in the equilibrium ice sheet response as a result of using a HadAM3 modelled climatology (rather than observed) may point to potential weaknesses in the ice sheet reconstructions using the same climate model"

I'm not clear on the intended meaning here. Large differences between modelled and observed or between various models? If the former, it is not apriori clear to what extent present icesheets are proximate to an equilibrium state. If the latter, I'm doubtful how you can infer potential weaknesses given the large set of uncertainties arising from variations in model configuration, boundary conditions, numerical implementation,.. that I've listed above.

"Standard bedrock topographies for running the ISMs originate from EISMINT (Huybrechts et al., 1996) for the GrIS and from BEDMAP for the Antarctic ice sheets (Lythe and Vaughan, 2001). These data, along with the PRISM3 ice sheet configurations"

I understand ALBMAP to be a more accurate topographic dataset for Antarctica. Or is this a revised BEDMAP dataset?

"Unlike many previous ISM intercomparison projects (e.g. EISMINT: Huybrechts et al., 1996 and ISMIP-HOM: Pattyn et al., 2008) the different ISMs are set up in standard mode. This methodology was chosen in order to include the uncertainties"

What does "standard mode" mean? Best tuned version for modelling present day Greenland and Antarctica?

"model elevation, and \gamma is the uniform lapse rate correction set to 8 C/km."

Isn't it about time we move beyond this simplistic uniform lapse rate correction. Given that the climate forcing is coming from a single GCM, how about extracting vertical temperature gradients at the appropriate elevations and for each month or season?

"However, the main focus of the analysis of the project will be on the equilibrium end-member ice sheets submitted for each simulation."

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Interactive Comment

Is not transient response of more relevance with respect to concerns about the future evolution of present-day ice sheets? Why the focus on equilibrium response?

##I would also like to see some specification of data formats and initial intercomparison of analyses planned in an appendix. This technical issue can cause many headaches in such model intercomparisons.

"ice-free conditions on Greenland (Fig. 3c) and a modern ice sheet over Antarctica (Fig. 3d)."

These are actually figs 3a and 3c respectively

I applaud the inclusion of the phase 2 experiments, given the strong temperature imprint from the ice sheet boundary conditions used by the GCM but I don't understand why one would use present-day Antarctica combined with ice-free Greenland as this is outside the stated estimated bounds for higher sealevel during the mPWP ("10 to 30+ m"). Would it not make more sense to have phase 1 and 2 roughly match the bounds on mPWP sealevel?

##Having been involved in a number model intercomparison exercises, my largest frustration has been the lack of analysis discerning sources of model differences. This is very difficult, but there are some steps that could be taken to partially address this. First, detailed descriptions of all model numerics and parameterizations needs to be tabulated. ISMIP HEINO partly did this, but there are a number of other poorly documented components in models that can result in major differences (eg pinning point parametrizations for ice-shelves). Second, where possible, consider taking into account participating model results from previous intercomparisons. More thought into this issue by the authors could likely generate other steps. This does make the whole exercise more challenging and onerous but I think it would also offer a much more valuable result to the community.

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