

Interactive comment on “Transient climate simulations of the deglaciation 21–9 thousand years before present; PMIP4 Core experiment design and boundary conditions” by R. F. Ivanovic et al.

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This is not mainly a review of the paper but is, as requested by the lead author, a set of comments on the proposed experiment design.

In general of course this is a usefully comprehensive description of what is planned under PMIP for the deglaciation transient. I have a few rather minor comments and then one that is more significant.

Detailed comments:

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Page 9049, line 11. This paper should be referred to as EPICA Community Members, 2004 rather than Augustin et al 2004.

Page 9050, line 9. It vis a little misleading to say that a shift in climate occurred in 1-3 years. A rapid shift occurred in some components (d-xs most notably) but for example the inferred temperature change was slower. I suggest just adding "some components of" climate.

Page 9057. Should you add that an importnat challenge for PMIP is to assemble suitable datasets for model-data comparison. Probably you say that elsewhere.

Page 9062, line 8 and numerous other places, including Table 1. I am sure you mean "i.e.", meaning "that is", and not "e.g." meaning "for example". This is importnat as I assume you are telling participants they must use 1365 W/m², nit that they can use any number they consider reprsents the preindustrial?

Page 9063, line 4, and other places including Tables 1 and 2. You suggest using the Luthi et al 2008 data (which for this part of the core is really the Monnin et al 2001 data) translated to AICC2012. This is an option, but you might want to at least discuss using the dataset presented in Bereiter et al (2015) as supplementary data. Here they have already doen the work of translating to AICC2012, and they include a range of datasets in their composite dataset, including the high resolution WAIS Divide data, with a 4 ppm offset (the offset discussed later on page 9063). To me it would seem smarter to use the fully resolved but consistent dataset.

Page 9067, last paragraph. "Can abrupt deglacial changes be simulated without ice-meltwater?". I think this is a bit disingenuous. We already know that they can't: the north-south phasing of climate is simply wrong if freshwater is excluded as already shown clearly in papers including Shakun et al (2012).

Page 9069. Regarding dust, isn't this another parameter that might be varied in ex- tended simulations?

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Page 9071, line 13. Do you mean "timing" in comparing Luthi to Marcott. I think we can easily fix any timing mismatches, as done in Bereiter et al (2015); it is really resolution that is the issue.

Page 9072. "the ...design for later periods..is upodated". I don't really see how this will work. Some groups will quite sensibly run straight through the whole period. It will be very confusing if you then change some aspect of the design halfway through, just because others have now reached a milestone. Are you really suggesting groups should hold their simulation at the end of each phase until everyone reaches the same point?

My major comment comes back to what the purpose of the experiments is, as always with PMIP. I can see two main classes of justification. One is to test different models against data. The other is to compare the performance of different models against each other. If the aim is the former then it makes sense to allow people some freedom to use different boundary conditions, which you do in allowing two different ice models. If it's the latter it makes no sense to have radically different ice models. However it cannot be the former, because you already know that in the core experiment, you won't get anything like the data (because no bipolar seesaw contrast). Given that, the core experiment (but not the extended ones) MUST be aiming mainly at model-model comparisons and these can only be made if most features of the design are common. I realise you probably had groups who would not compromise on use of their favourite ice model, and I sympathise with the dilemma but not the solution. I think you have to be firm and choose a primary ice model, with no suggestion that it is better and with a strong recommendation that as many groups as possible run both. Those who want to use whichever you choose as the secondary ice model can use it as long as they also use the primary one in a parallel experiment. The aim should be to have a situation where the model-model comparison can be made without compromise.

Bereiter, B., Eggleston, S., Schmitt, J., Nehrbass-Ahles, C., Stocker, T. F., Fischer, H., Kipfstuhl, S., and Chappellaz, J.: Revision of the EPICA Dome C CO₂ record from 800

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to 600 kyr before present, *Geophys. Res. Lett.*, 42, 542-549, 2015.

Interactive comment on *Geosci. Model Dev. Discuss.*, 8, 9045, 2015.

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