Geoscientific
Model Development
Discussions

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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Dear Jeremy and readers,

Please find attached (supplementary .pdf) our revised paper with changes from the original version tracked (accompanying updated figures are below this interactive comment).

We found both the reviewers' and the interactive discussion comments on the manuscript to be very helpful and have responded in detail below. In particular, we

have revised the design of the core experiment to include freshwater forcing from ice sheets in line with the reviewers' and Eric Wolff's comments. We have also emphasised the scientific purpose of the experiment; to understand the sequence of last deglaciation events using 'model-geological data' comparison.

We wish to highlight that the Core experiment has been designed following extensive consultation (since January 2014) across the palaeoclimate modelling and reconstruction community. It has been challenging to reconcile everyone's preferences for such a demanding experiment (more than 12 thousand model years); everyone uses different tools and has different (though complementary) scientific aims to each other. However, we are confident that in its new form, the Core experiment design represents the best compromise between any conflicting views and leaves room to accommodate everyone's priorities and resource limitations. For example, this is one reason why we have to be flexible on the ice sheet meltwater forcing protocol, including allowing groups to run without meltwater if they wish (although we recommend that they run with meltwater and provide scenarios that are consistent with the ice sheet histories). Certainly these efforts will be worthwhile and we are excited to begin the experiment and see the first results.

All comments and our responses follow:

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REVIEWER 1

Reviewer's summary: The paper describes the design of the coordinated Core simulation over 21-9 ka with time varying orbital forcing, greenhouse gases, ice sheets and other geographical changes. The choice of two ice sheet reconstructions is given but no meltwater is prescribed. The paper reviews in detail the past experimental designs by EMICs and AOGCMs (ex CCSM) and their results but unfortunately mismatches the experimental design presented this time because of no meltwater. I am afraid the readers are lost in understanding what we can learn from the experiment at the present

form of the paper. I recommend the paper published after revision by (1) [equivalent to point 1. below] presenting a core experimental series with melt-water given at least in a very simple way and (2) [equivalent to point 2. below] explaining what kind of analysis are useful after collecting the non-meltwater experiments from PMIP community. Also (3) [equivalent to point 3. below] clarify the design related to coastline, bathymetry and salinity change due to ice sheet change.

1. Reviewer's comment: '(1) For the first point, the meltwater that is consistent with the ice sheet provided from two schools should be provided so that additional experiment with meltwater can be performed. Where to release could be an option. As in many studies, the regional difference (South vs. North) during the deglaciation is presented and discussed but without the meltwater there is no way expecting the reproduction in proxy as in Clark et al, 2012 or Shakun et al, 2012. Even if there is uncertainty of the location of the meltwater or an uncertainty of timing of abrupt change of melt water, at least the total amount of meltwater can be provided and given by each modelling group. The change of total amount (âĹij sea level change) should be consistent with the ice sheets reconstructed and also constrained fairly well (Clark et al, 2009).'

Authors' response: We have updated the Core experiment design to include freshwater and have revised the manuscript accordingly. This includes uniformly distributed freshwater to conserve water in the simulation (i.e. the total amount of ice melt is applied uniformly to the ocean) and 'routed' freshwater (i.e. fluxes from particular coastal outlets, which can be used to examine more regional responses); both are consistent with the ice sheet reconstructions provided (GLAC-1D and ICE-6G_C). Further focussed simulations will explore these hypotheses (e.g. north vs. south meltwater injection sites) more thoroughly and systematically.

2. Reviewer's comment: '(2) For the second point, if the PMIP4 Core experiment group asks for the non meltwater experiment, then the reason and what is expected should be described clearly. If there is no melt-water, there is no sense in doing a transient experiment, which is very expensive. It is unclear at the moment why the non-melt water

experiment should be done as a Core experiment. PMIP experiments with AOGCMs are expected to do model-data comparision [sic] as well as model-model experiment, but what are the data-model comparison expected? Many studies suggest that the melt water might be important for understanding the "bipolar" ice core signals and various regional signals in proxy. Since the experiment demands substantial computational resource as well as man-power for many groups, the explanation should be convincing. The introduction in the paper is not sufficiently written for the non-melt water transient experiment. Perhaps what is expected scientifically after collecting the results could be written in an independent section in more detail.'

Authors' response: As addressed above (point 1), we have taken on board this comment and revised the manuscript and experiment design to recommend including meltwater (consistent with the ice sheet reconstructions) in the Core. We have also emphasised the aim to carry out model-data comparison with the results (e.g. section 1.4), for which including meltwater forcing in the Core simulation is preferable. We have expanded section 2.5 to discuss this in more detail, including the value of running meltwater-free simulations to accompany meltwater-include simulations.

3. Reviewer's comment: '(3) On the design related to coastline and bathymetry change due to ice sheet change: In table 2, the design of salinity change is unknown. Define what (and how) the modelers do with the total ocean salinity change, which should be consistent with the ice sheet change and melt water.'

Authors' response: We have expanded the row on Freshwater fluxes' in table two to include the addition of freshwater to the Core and to explicitly advise groups to conserve salinity. The freshwater scenarios provided will conserve salinity changes relating to ice sheet evolution (following GLAC-1D and ICE-6G_C). We cannot be more precise about how this will be implemented (technically) because it is model-specific and therefore up to the user, but the transient data will be provided to make this possible.

Reviewer's comment continued: 'There are two options for the "Bathymetry" but what

happens when the ice sheet covers the ocean in the model that keeps the "Preindustrial bathymetry"? What is prescribed for ice sheet and what should be done for ocean boundary condition should be carefully designed and described for the participants.'

Authors' response: The land-sea mask (or 'coastlines') will need to be consistent with the ice sheet, as outlined in the text and tables. This means that land should underlie the grounded ice sheets, as requested by the reviewer, but does not require other bathymetric changes. We have extended the text and table entry to clarify this in the manuscript. Again, how this is technically implemented is model-specific and best decided by the expert user.

4. Reviewer's comment: 'Page 9073 line 9 "many questions and untested hypotheses remain" but the current study should show the perspective, how it answers the questions and the hypotheses are tested.'

Authors' response: A large component of section 1 is devoted to showing the context (or perspective) of the working group, outlining the current state of our knowledge, what hypotheses exist and what questions remain. We have extended the text in this section, as well as later sections, to explain how the multi-model approach aims to narrow down uncertainty in the ice sheet reconstruction and meltwater forcing, for example. We have also discussed focussed simulations that will represent rigorous sensitivity- and hypothesis-driven investigations that are of particular interest to participant groups; including, for example, the regional specificity of climate system response to freshwater inputs, the timing of changes in greenhouse gas records, the influence of the acceleration of northern ice melt on ocean circulation during Heinrich Stadial 1 – these are all discussed in the manuscript and we have extended the text in relevant sections to clarify this. These foci will be further defined on the last deglaciation PMIP Wiki and in subsequent manuscripts as they are investigated.

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REVIEWER 2: SHAWN MARSHALL

Reviewer's summary: 'Summary This manuscript describes the scientific motivation and technical specifications for a community model experiment simulating the deglaciation (26 or 21 ka until 9 ka BP) in climate models of differing complexities. The experiments are designed so that both fully- coupled Earth system models and a variety of reduced models can take part. There is a nice blend of flexibility in the model design – with specified boundary conditions for the main climate forcings and their temporal variability, but some user discretion on implementation. The balance seems appropriate. This is nicely presented and explained, overall. The summary of deglacial climate dynamics and some of the paleoclimatic enigmas during this period makes for a lovely review, and the experiments that are described will certainly be interesting. Most of what one needs from this manuscript is encapsulated in Tables 1 and 2, so at first it seemed unnecessarily long, but the narrative is nicely written and a pleasure to read, offering some helpful insights about the approach to be adopted in the intercomparison.'

5. Reviewer's comment: 'I am a bit surprised that the 'focussed' experiments are not described or prescribed in detail at this point. I understand that perhaps these need to be reactionary to the results of the core experiment. It seems unfortunate though, as it would be helpful to have this information together in a single document. I am sure lots of ideas are already in place for the spinoff or focussed experiments, and it would not have taken too much extra work to have these set out here. But this is not necessary, and it is probably helpful to keep these flexible and as subsets of the main modelling exercise.'

Authors' response: The Core itself is a significant undertaking for modelling groups, it has taken a lot of discussion to agree upon the experiment design within the community. Also, different groups have different preferences and priorities for the focussed simulations. Therefore we agree with the reviewer that it is helpful at this stage (and until the Core is published) to keep the focussed experiment designs flexible, and for clarity it is preferable to keep their full specification separate from the required Core

(described in detail here). As does the reviewer, we also expect some of the focussed simulations to be reactionary to working group results, as they emerge. However because some discussions are already underway and, as the reviewer suggests, we do have some ideas in place, we have extended the text in sections 2.5 and 3 to provide more information on currently planned focussed simulations.

6. Reviewer's comment: 'My only substantive feedback or suggestion involves the meltwater treatment. Several thoughts related to this are made below, in the specific comments. Overall, it seems inconsistent to have specified, time-varying ice sheet volume on the continents but not honour this global water conservation when it comes to the ocean freshwater and salinity budget. I appreciate the desire to control for meltwater runoff, but it makes one wonder if the core experiment, as described, is meaningful since it does not do a physically sensible job of representing the basic ocean state through deglaciation. At least as I understand the model design. Things like preconditioning and ocean mixing surely depend on the mean salinity and its structure. I appreciate that this design is intentional, to eliminate some of the complexity and model dispersion associated with when/where to put the meltwater. And models are dealing with meltwater routing and runoff internally, in some cases. But since the specification is to violate water balance and neglect runoff processes, it would not be unreasonable to honour water balance while neglecting runoff processes. That is, the ice sheet ï ĚŻADV, [sic] as specified through the Peltier or Tarasov reconstructions, can be converted to eustatic water equivalent and restored to the nearest ocean in a specified way for all model experiments. This could be considered for the Core experiment as something a bit more realistic, while saving some of the detailed questions about meltwater runoff and iceberg discharge for the focussed experiments. It would require a bit of extra work to define the timing and location of freshwater runoff, which everyone would follow, but this can be straightforward I think. Just don't inject the water all at once every 1000 years, when the ice geometry changes. Rather than shock the system, one could, for example, take the 1000-year ï ĚŻADV in each major river catchment and divide ĚĞ by 1000 to give the average runoff in m3/yr (or convert to

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Sv), in a way that respects water balance. If one wants to avoid some of the detailed questions concerning paleoriver routing, the appropriate amount of water could just be spread over the large-scale basin (e.g. North Atlantic, Southern Ocean, etc.). I would leave it to the authors to consider what is best here, but I do recommend considering a treatment like this within the core experiment design.'

Reviewer's comment: 'p.9047, ll.12-14, "A choice of two ice sheet reconstructions is given, but no ice sheet or iceberg meltwater should be prescribed in the Core simulation." – this is confusing, are ice sheets to be prescribed or internally modelled? I understood what the authors meant by the end of the manuscript, i.e. don't put any ice sheet meltwater into the oceans, but this seems contradictory to prescribe ice sheets but not put the prescribed change in water volume back into the oceans.'

Reviewer's comment: 'p.9067, Section 2.5, freshwater fluxes during the deglaciation. It does seem odd but also sensible to have controlled experiments that examine non-meltwater forced climate change during the deglaciation. Although given the important role that ocean circulation simply had to have played in the Bolling and YD, this seems limiting. i.e., orbital forcing and CO2 clearly cannot explain these features of the deglaciation. A reference experiment is nonetheless important and useful. I wonder if it is the best reference though, given that the ice sheets did melt away and ocean salinity did decrease through this period. Is it possible to have prescribed changes in mean ocean salinity through the deglaciation and/or prescribed runoff as a second core experiment? The latter could be done based on the 1000-yr ice sheet updates to at least have the correct global water cycle (conservation). I appreciate the arguments and intricacies concerning when and where to put the meltwater. Some hypothesis-driven experiments here seem sensible, as additional experiments.'

Authors' response: We have taken on board this feedback and have adapted the Core experiment design to include transient (i.e. not stepped/shocked) meltwater fluxes in the Core simulation (also see reply to reviewer comment 1 and 2, and the second paragraph of this letter). This will enable more fruitful model-data comparison and the

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possibility to narrow down uncertainty in last deglaciation ice sheet meltwater fluxes. Our focussed simulations (e.g. as briefly outlined in section 3) will continue to address this more fully.

7. Reviewer's comment: 'p.9048, l. 26, "majority of its ice melting" – not really the majority of the Antarctic Ice Sheet melting; rather, much of the excess LGM ice that was out on the shelf, and the thicker ice that covered WAIS; but overall, it was closer to a 20% loss of the ice in Antarctica through this period'

Authors' response: We've changed this sentence in line with this comment to make the meaning clearer.

8. Reviewer's comment: 'p.9050, l.4, the idea of mid-latitude N.Atlantic warming during H1. This is not really compatible with the preservation of Hudson Strait icebergs in a swath at 40-55 N across to Portugal. Is it more of a subtropical warming that has been proposed? Else it is perhaps worthwhile to note this incompatibility.'

Authors' response: We have removed this last clause in the sentence.

9. Reviewer's comment: 'p.9050, l.15, suggest deleting 'older', it conveys a bias against these studies, i.e. a potential lack of objectivity, whereas many of the studies cited below in favour of a northern source are in fact older'

Authors' response: We have removed 'older'.

10. Reviewer's comment: 'p.9062, Section 2.1. I wonder about a prescription for oceanic or surface ocean d18O and dD as well, for those that will explore isotopic cycles through the deglaciation.'

Authors' response: This is beyond the scope of the Core simulation, which is designed as a basic simulation for all models (and most will not run with water isotopes, δ18O and deuterium, due to the computational expense or because they are not implemented), but it would make an appropriate focussed experiment. We will discuss this in the group and if it is popular, we will work in close collaboration with the Isotopes PMIP

working group to design the experiment, including the prescription of water isotopes. It will require careful planning because the isotopes are implemented in the different models in different ways, so we would need to provide the most valuable and important data. For example, many models will get their ocean surface water isotopes through interaction with the atmosphere as the isotopes are implemented throughout the whole hydrological cycle. However, changes in terrestrial ice volume that are not dynamically simulated in the model will complicate this, and that may be where we will need to provide a transient global budget of water isotope data and meltwater signatures.

11. Reviewer's comment: 'p.9062, ll.7-9, discussion of the freshwater budget. Just to be clear here, the experiments should prescribe/force all precipitation to return to the oceans annually then, i.e. equilibrium mass balance conditions on the ice sheets? This is fair for present purposes, but I guess that it will not occur naturally in any of the models, so this sounds tricky. I wonder if more explicit directions here would be helpful, as to how the freshwater routing/flux adjustments should be prescribed. For instance, should an LGM catchment map be prescribed, so that everyone is using the same one, based on the ice sheet configuration? Then everyone forces all precipitation within the catchment to return via a prescribed river outlet/coastal grid cell.'

Authors' response: This is a technical point specifically relating to the equilibrium-type spinup of the LGM to make sure that during the spinup, there are no large salinity drifts in the model, and that water is conserved. It is difficult to provide more detailed or precise directions because it is so model-specific. However, it has been common PMIP practice for several years (e.g. for the LGM experiment) to have to consider this (in the LGM spinup); we have provided the most recent text from the PMIP Wiki, but similar earlier advise was given (e.g. PMIP3 and CMIP5). This should be sufficient information, but if not, individuals can use the working group mailing list and Wiki to ask for help in generating their spinup; although support from their model developers is likely to be more useful in this instance. Similarly, LGM catchment maps will be provided as they become available. However, when these have been offered in the

past, uptake has been very low due to the technical challenges involved (we are not aware of any groups having implemented it). Furthermore, some models calculate their own river routing based on surface topography. For these reasons, we will provide the data, but leave the choice of what to implement for river routing to the expert model-user. However, it is essential to ensure that rivers reach the coast, and this is explained in the manuscript (section 2.6). These details are provided by the LGM PMIP working group.

12. Reviewer's comment: 'p.9064, ll.23, 27. I think with Tarasov as an author, you don't have to list this as 'personal communication' – also on the next page'

Authors' response: We have amended these lines.

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INTERACTIVE DISCUSSION

13. Comment by A. Carlson: 'Hi all, so just looking through the text, I noticed one incorrect statement on page 9055 lines 18-22. The timing of CIS-LIS separation was around the time of MWP-1A according to Dyke (2004) is not right. Dyke (2004) specifically states: "Unfortunately, the initial opening of the ice-free corridor remains only tenuously dated. The initial opening at the south end probably started about 15 ka BP, based on an AMS date of 15.67 ka on wood, mentioned above (Beierle & Smith, 1998) and exposure dates on the Laurentide terminal moraine in the south-western Alberta Foothills (Jackson et al., 1999). By 13.5 ka BP, the southern half of the corridor seems to have opened, because wood (evidently small wood, probably arctic willow; P. Bobrowsky, personal communication, 2002) from basal sediment of glacial Lake Peace yielded an AMS date of 13.97 ka BP (Catto et al., 1996). It is possible that the corridor was entirely open by 13.5 ka BP, because the dated site is located midway within it. However, in the absence of similarly old dates from the northern part of the corridor, and assuming slower ice ablation further north, initial joining of the southern and northern approaches of the corridor may not have occurred until 12.5 ka BP or

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possibly even 12 ka BP. It seems exceedingly unlikely, however, that ice coalescence could have continued until 11.5 ka BP, for the Mackenzie Lobe of Laurentide ice had by that time receded halfway up the Mackenzie Valley (Mackay & Mathews, 1973; Smith, 1992). Furthermore, south -eastern Cordilleran ice had by then receded into the alpine zone (Reasoner at al., 1994), and Cordilleran ice distribution in Alaska was close to the present one. In summary, the known history of the ice-free corridor, although imprecise, does not preclude the possibility of pre-Clovis people using this route and its availability to early Clovis people is almost certain." These are all ages in 14C years, meaning the corridor started opening by âĹij18.8 cal ka and was over half complete by âĹij17 cal ka, thousands of years before MWP-1A. I think this section needs to be clearly redone to reflect this much slower and earlier separation of the CIS-LIS that Dyke (2004) discussed to stop'

Authors' response: As is pointed out in this comment, an adequate discussion of the issues raised would be too lengthy and is only peripheral to this manuscript. We have thus removed this sentence from the manuscript.

14. Summary by EW Wolff: '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.

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

Authors' response: We have made this change.

15. Comment by EW Wolff: 'Page 9050, line 9. It is 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.'

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Authors' response: We have made this change.

16. Comment by EW Wolff: 'Page 9057. Should you add that an important challenge for PMIP is to assemble suitable datasets for model-data comparison. Probably you say that elsewhere.'

Authors' response: Yes, we've added text to this effect at the end of the first paragraph of section 1.3

17. Comment by EW Wolff: 'Page 9062, line 8 [sic – page 9063 line 12?] 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 important as I assume you are telling participants they must use 1365 W/mËĘ2, nit that they can use any number they consider represents the preindustrial?'

Authors' response: In the case of the solar constant, it is deliberate to use 'e.g.' (for example) because the recommended preindustrial value (1365 W m-2) is a widely used and accepted value, it has thus far been the recommended PMIP value, but is not exclusively accepted and others may also be used (see recommendations at http://solarisheppa.geomar.de/cmip5). Some groups already prefer to use other PI values; e.g. 1370 W/mˆ2 (Zhang et al., 2012) and 1360.9 W/mˆ2 (Landrum et al., 2013).

18. Comment by EW Wolff: '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 done 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.'

Authors' response: We have updated the experiment design, manuscript text figures

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and references to use the more recent Bereiter et al. (2015) CO2 data.

19. Comment by EW Wolff: 'Page 9067, last paragraph. "Can abrupt deglacial changes be simulated without icemeltwater?". 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).'

Authors' response: We have added freshwater to the core simulation.

20. Comment by EW Wolff: 'Page 9069. Regarding dust, isn't this another parameter that might be varied in extended simulations?'

Authors' response: Yes, we have extended the text to include this suggestion.

21. Comment by EW Wolff: '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.'

Authors' response: We have removed the comparison of Luthi et al. (2008) to Marcott et al. (2014) since this can be resolved in the records.

22. Comment by EW Wolff: 'Page 9072. "the ...design for later periods..is updated". 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?'

Authors' response: This would be important to avoid (as is pointed out, 'some groups will quite sensibly run straight through the whole period') so we have clarified this point in the text (first paragraph of section 4). Mainly, changes will not compromise the Core, although new data may be used to design focussed spin-off simulations, and to assess the Core results in light of the changes (and additional simulations).

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23. Comment by EW Wolff: '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 an [sic] be made without compromise.'

Authors' response: We have carefully considered this point and have (a) adapted the Core to include meltwater, and (b) emphasised the importance of model-data comparison in the working group's aims (some of the focussed sensitivity experiments will enable model-model comparisons, but our main priority is model-data comparisons). Also see response to Reviewer comment 1, 2 and 6 above.

24. Comment by A. Schmittner: 'In a recent paper (Schmittner et al. 2015) we have shown that changes in tidal energy dissipation between the LGM and the late Holocene may have a large impact on the Atlantic Meridional Overturning Circulation. I think it may be warranted to think about if this could be included as a prescribed forcing over the deglaciation. Schmittner, A., Green, J. A. M., and Wilmes, S.-B. (2015)

Glacial Ocean Overturning Intensified by Tidal Mixing in a Global Circulation Model Geophysical Research Letters, 42 (10), 4014-4022. doi: 10.1002/2015GL063561 http://onlinelibrary.wiley.com/doi/10.1002/2015GL063561/full'

Authors' response: This will be difficult to include in the Core design, but is an interesting component to consider. We will propose it as a theme for a focussed experiment and have amended the text in section 3 accordingly.

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Having carefully and thoroughly addressed all of the reviewers' comments and the interactive discussion, we hope that the revised paper is now acceptable to be published in GMD.

Yours sincerely,

Ruža F. Ivanović

REFERENCES CITED IN RESPONSE:

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 to 600âĂĽkyr before present, Geophys. Res. Lett., 42(2), 2014GL061957, doi:10.1002/2014GL061957, 2015.

Landrum, L., Otto-Bliesner, B. L., Wahl, E. R., Conley, A., Lawrence, P. J., Rosenbloom, N. and Teng, H.: Last Millennium Climate and Its Variability in CCSM4, J. Climate, 26(4), 1085–1111, doi:10.1175/JCLI-D-11-00326.1, 2013.

Zhang, Z. S., Nisancioglu, K., Bentsen, M., Tjiputra, J., Bethke, I., Yan, Q., Risebrobakken, B., Andersson, C. and Jansen, E.: Pre-industrial and mid-Pliocene simulations with NorESM-L, Geosci. Model Dev., 5(2), 523–533, doi:10.5194/gmd-5-523-2012, 2012.
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Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C4275/2016/gmdd-8-C4275-2016-supplement.pdf

Fig. 1.

Fig. 2.

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Fig. 3.

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(a) ICE-6G_C

(b) GLAC-1D

21 ka

12 ka

9 ka

Elevation above sea level (m)

0 500 1000 1500 2000 2500 3000 3500 4000

Fig. 4.

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