Tokyo, Japan, Friday 25th September

Dear Dr. Didier Roche, Editor of the Geoscience Model Development,

We thank you and the reviewers for the devoted time and efforts for this submitted manuscript, GMD-2015-80.
We responded to all the comments of both reviewers point by point in blue and the change in the text is incorporated as in red for each comment. Also the manuscript with the change is marked to see how the change was incorporated.
We hope that with these changes the manuscript can now be accepted for publication in the Geoscience Model Development.

Best regards,
Ayako Abe-Ouchi
Fuyuki Saito, Masa Kageyama, Pascale Braconnot, Sandy P. Harrison, Kurt Lambeck, Bette L. Otto-Bliesner, Dick Peltier, Lev Tarasov, Jean-Y Peterschmitt and Kunio Takahashi

## Reply to reviewer #1

The manuscript by Abe-Ouchi et al. describes methodology of development of "blended" LGM ice sheets for the PMIP3 intercomparison project. The manuscript also presents results of simulations illustrating influence of differences ice sheets reconstructions on simulated climate. I believe this is useful paper and I would recommend it for publication in the GMD after minor revision.

General comments
Methodological part of the paper which documents methodology of "blended" ice sheets development is clear and needs only some editorial changes. Interpretation of climate impact of three different ice sheet reconstructions performed with the same model, in spite of obvious limitations an AGCM-slab ocean model configuration, is also straightforward.

(1-1) However, I see a problem when the authors compare PMIP2 and PMIP3 modeling results. These are very different models and even if the same ice sheets reconstruction would be used in PMIP2 and PMIP3, significant differences between global SAT anomalies averaged over the different models ensemble are unavoidable.
Even more, because radiative forcing of ice sheet depends not only on prescribed ice sheets but also on models (for PMIP3 this difference is more than 30% across the model ensemble), differences in global mean radiative forcing of ice sheets cannot be solely attributed to differences in ice sheet reconstructions used in PMIP2 and PMIP3. Therefore the authors cannot claim that simulated difference in radiative forcing of ice sheets, elevation and land area between PMIP3 and PMIP 2 of 1 W/m2 is caused by difference in ice sheet reconstruction. Similarly 0.5C extra cooling in PMIP3 ensemble cannot be attributed to different ice sheet reconstructions. Obviously, I do not suggest redoing PMIP3 ensemble with PMIP2 ice sheets reconstruction but such experiment can be perform in principle with the AGCM-slab ocean model. In any case, this potential caveats should be discussed in the manuscript.
-→ We agree that the comparison between PMIP2 and PMIP3 is not only due to the change of ice sheet configuration. In the revised text, we made clear that the difference between PMIP2 and PMIP3 includes the difference due to the

revision or development of the model versions for the CMIP. We would like to clarify that the conclusion is not simply drawn by comparing the ensemble mean between PMIP2 and PMIP3 simulations, but by looking at the individual results from model groups. For each model group the model version is indeed different between PMIP2 and PMIP3. Despite the differences in each modeling group, the estimates of the forcing and temperature change provide larger changes (LGM-PreIndustrial) in PMIP3 than in PMIP2 simulations. This systematic behaviour would not appear if it only results from model versions (some models would show larger and other smaller estimates), which is why we stated with confidence that it is due to the ice sheet. In addition a simple estimate of the impact of the different ice-sheet on the radiative forcing is provided in Braconnot et al 2012, NatureCC and lead to the conclusion that the forcing would be larger with PMIP3 than with PMIP2 ice-sheet. This point is treated in more depth in Braconnot and Kageyama, Phil.transA, in press. In the revised paper we state that there is still a need of further analysis how the difference of temperature and climate in detail between PMIP3 and PMIP2 model results is influenced by the ice sheet. See (1-19) for the revised part added in the end of the paragraph of page 4311 L.7.

(1-2) Some parts of the manuscripts, especially introduction, require improvements of the style. In particular, several sentences (like that on page 4298, lines 2-9) are lengthy and difficult for understanding.
-→ Yes, this is worked out for several parts. For example, the part (page 4298, lines 3-10) is changed as following. "The specification of height of ice sheets has large impacts not only on surface temperature through the lapse rate, but also on the location of storm tracks, precipitation field, planetary scale atmospheric circulation and even ocean circulation. (Justino ……..Ullman et al, 2014)"

(1-3) I would also suggest to use more precise scientific terminology. In particular, sheet reconstructions are not "boundary conditions", at least, in mathematical or physical sense.
-→ Yes I agree that we should use the terminology precisely. The word "boundary condition" was used in the meaning that ice sheet is one of the boundary conditions in climate models used for PMIP and CMIP simulations, which contribute to IPCC reports. We added explanation of "boundary condition" in the second paragraph of the Introduction section. Since the abstract should be written clearly without the help of the introduction, we changed the text in abstract to avoid any misunderstanding as following; "(p.4295, Line2-7) We describe the creation of ice sheet configuration, namely, ice sheet extent and height, ice shelf extent, and …..".

Also the text in the introduction is revised as
(p.4296, L.23~) "The boundary conditions for the climate models must be specified and changed for the LGM experiment compared to the pre-industrial control experiment in PMIP (Braconot et al, 2007a,b, 2012, Masson-Delmotte et al., 2013). They are a (relatively small) change in orbital forcing, reduced ….

(1-4) Another example is "shallow ice-sheet model". This is rather rear and confusing term. I would suggest using the standard term – ice sheet model based on shallow ice approximation.
-→ (2.2 GLAS model) Revised as suggested.

Specific comments
(1-5) p. 4295, l. 2. ": : : the creation of boundary conditions: : :" See my general comments.
-→Page 4295. Line 2-7 is changed as following to avoid the misunderstanding. "We describe the creation of ice sheet configuration, namely ice sheet extent and height, ice shelf extent and the distribution and altitude of ice free land, at the Last ……. (PMIP3)."

(1-6) p. 4295, l. 10. “albedo mask” sounds strange. Do you mean ice sheet mask?
-→Yes. “albedo” → “ice sheet”

(1-7) p. 4295, l. 22. “There are much larger differences in the climate response to the latest reconstructions: : :” The meaning is not clear.
-→ The last two sentences of abstract are revised to clarify the meaning and focus the message.
-→ p.4295 l.18- l.24 “Differences in the climate response between the individual LGM reconstructions and the CMIP5/PMIP3 composite are much smaller than the that between CMIP5/PMIP3 and the previous phases of PMIP (PMIP2).”

(1-8) p. 4296, l 5. I cannot see how PMIP type experiment can help in understanding of the causes of uncertainties in future climate predictions.
→ The text is modified to clarify the role of PMIP, as following; “Modelling of past climate states, and evaluation of the simulations using paleoclimate reconstructions, provide unique opportunities to test and assess the performance of models used for future climate projections (Braconnot et al., 2012, Masson-Delmotte et al., 2013, Schmidt et al., 2014).”

(1-9) P. 4296, l 8. Please remove “change”.
→ It is here meant for changes in natural forcing such as Greenhouse gases and insolation. Therefore it is modified as “The Last Glacial Maximum ……..because the change in natural forcing was of a similar magnitude to that expected by the end of the 21$^{st}$ century (Braconnot et al, 2012).”

(1-10) p. 4297, l. 12. I would suggest to use “ice sheet topography” instead of “overall form of the ice sheet”.
→ Revised as you suggested

(1-11) p. 4297, l. 29. “the change in land-sea geography has impacts on sea level: : :” I suppose that it is other way around, namely, sea level affects land/sea distribution.
-→ clarified as “the change in ice sheets has impacts on sea level and…”

(1-12) p. 4301, l. 12 What is EOFS?
-→ revised as `Empirical Orthogonal Functions (EOFs)'.

(1-13) p. 4301, l. 23. What is “margin forcing”?
→ This terminology “margin forcing” is defined originally in the Tarasov and Peltier papers, even devoted to an independent section 2.4 in the paper of 2004, but here is revised as “margin correction defined as “margin forcing” (section 2.4 of Tarasov and Peltier 2004) ”

(1-14) p. 4307. In the left hand side of the Eq. 10 should be “Mask_1,ave”
→ Right. Corrected accordingly as “Mask_1,ave”.

(1-15) p. 4307, l. 15. The meaning of “masked surface altitude” is not clear to me.

→ Revised as `surface altitude field extended over undefined gridpoints.'

(1-16) p. 4308. L. 10. "The ANU reconstruction consistently shows the largest changes: : :" Do you mean that the ice volume of ALL ice sheets in ANU reconstruction are larger than in the other two?
→ Yes we do. Revised this part to be clear, as `( p. 4308. L. 10- 11) All the ice sheets in ANU reconstruction show larger changes than those in the other two, and those in GLAC-1a reconstruction show smaller changes in NH ice sheet volume'.

(1-17) p. 4310, l. 12. Why "but"? I would say "and" instead
→ Agreed, corrected to "and"

(1-18) p. 4311, l.5. Please remove "change".
→ "change" means the change from modern day to LGM, so this is correct. We should try to make this clear, by modifying "overall change in forcing varies" -→ "overall combined change of radiative forcing (by the specification of LGM condition) varies"

(1-19) p. 4311. L. 5. Please make it clear (see my general comment) that the difference of 1 W/m2 is caused not only by different ice sheet reconstructions but also because of using of different climate models.
-→ Yes, the next sentence is added in the end (related to the comment of (1-1). "Note that this analysis cannot distinguish between the result coming from different ice sheet reconstruction and that from different climate model versions. There are three models which were run with both ice sheet versions PMIP2 and CMIP5/PMIP3, CCSM, IPSL and MIROC. They show a decrease of the combined change of radiative forcing from PMIP2 to CMIP5/PMIP3 by 1.34 W/m2 in average, and each of them is also about the same ~ 1 W/m2 in magnitude. Although there may be different reasons in the change of radiative forcing including those due to model development, it is very likely that the difference of about 1 W/m2 is due to the configuration of ice sheet and land/sea mask specified for the LGM experiment of CMIP5/PMIP3."

(1-20) p. 4312, l. 3. "increase in global mean annual temperature of ca 0.5 C compared to the PMIP2 experiments". In fact, according to the Table 3, global SAT is lower in the PMIP3 compare to PMIP2.
-→ Agreed. This should be revised as "increase in the LGM cooling of global mean annual temperature of ca 0.5 C compared to the PMIP2 experiments"

(1-21) p. 4312, l. 6. What is "ideal world" and how it is related to ice sheet reconstructions?
→ This is to mention that ideally (when we perfectly reconstruct the ice sheet) the ice sheet reconstruction must not be different from one person from another, but in our real world we cannot be perfect and still we have some uncertainty on the ice sheet reconstruction. I would like to revise to clarify the points as "(page 4312 from the beginning of the first paragraph of Section 5, Line 6-10) If our science community had a perfect consensus about the form of the LGM ice sheets, then there was no need to reconstruct a composite set of ice-sheet related boundary conditions. At present the science estimating the ice-sheet thickness is still largely evolving with several constraints from geophysical data, geomorphological data of ice-sheet margin at LGM, lithological constraint of retreat history, glaciological and climatological constraint on ice-sheet shape and so on. It is useful …."

(1-22) p. 4312, l.18. “implied” or prescribed?
-→ Agreed. Revised to “prescribed”

(1-23) p. 4312, l. 23.Because different models participated in PMIP2 and 3, it should be “difference in global mean annual temperature ANOMALIES is 0.5C”
→ Agreed.

(1-24) p. 4313, l. 20-22. This is a questionable argument. Two Antarctic reconstructions used for PMIP3 are so different that at least one of them should be wrong.
-→ It is not possible to argue which one is wrong or correct but at least we try to say that the “composite” ice sheet of CMIP5/PMIP3 should be not so wrong, so we write as following; “While this may reflect model improvements to some extent, it would be unlikely to occur if the composite ice-sheet of CMIP5/PMIP3 was substantially wrong.”

(1-25) p. 4313, l. 23 What is “observation margin”?
→ We revise the text to clarify our point as following; (page 4313, L23-26, combining the first two sentences) “Even after carefully reconstructing the ice sheets by taking into all kinds of observational constraints, the way in which ice-sheet topography and extent are implemented varies between different climate models.”

(1-26) Table 1. The longitudinal range for GLAC-1a (347.25, 479.25) is odd. Please change to (-12.75, 119.25).
-→ Agreed

(1-27) Fig. 6 caption. “difference in radiative forcing and feedbacks” sounds strange to me. How the difference between feedbacks is measured? Please also specify the units.
-→ Agreed. Revised to “difference in radiative forcing (in $W/m^2$)”

(1-28) Fig. 7 Panel (a) does not show “MAT in the simulation with the CMIP5/PMIP3 composite ice sheet“. It definitely shows differences between PMIP2 and PMIP3 but the meaning of “MIROCs” is unclear.
=→ I am really sorry for the mismatch of the caption and the figure. This should be revised
→ Figure 7. Impact of ice sheet choice compared to CMIP5/PMIP3 composite ice sheet on mean annual temperature (degree C) in simulations made with the MIROC slab ocean model, for the ice sheet choice of (a) PMIP2, (b) ICE-6Gv.2, (c) GLAC-1a, and (d) ANU. The land mask (> 50% land) is shown in grey, the ice margin (> 50% ice) is shown in black on all four plots.

(1-29) Fig. 8 “where the temperature is higher than -9C” I suppose it should be “where temperature anomalies is smaller than -9C”.
-→ Should be revised as “where the temperature changes in cooling due to LGM condition is smaller than 9C”.

**Reply to reviewer #2**

I agree with the previous reviewer that the paper by Abe-Ouchi and 10 others on dealing with ice sheet boundary conditions in CMIP5/PMIP3 LGM experiments is useful. Being rather technical in nature, GMD should be a well-suited forum for the paper, and it deserves being published after some minor modifications.

(2-1) My main criticism is that, in some places, the paper reads as if its main intention were to be a guide for dealing with the presence of ice sheets in further CMIP5/PMIP3 experiments. For instance, the very first sentence in the abstract: "We describe the creation of boundary conditions ... for use in LGM experiments ... as part of ... CMIP5 and …PMIP3". However, with the publication of the IPCC AR5 in 2013/2014, CMIP5/PMIP3 is essentially history, and the community is now heading towards CMIP6 (including a further stage of PMIP). So I suggest to be more outspoken on this point, change the manuscript accordingly and, in section 5 (Discussion and conclusions), discuss in some more detail the perspectives for future work.
-→ We agree to the reviewer#2 that the PMIP3 ice sheet was for CMIP5/PMIP3 and that we are heading for CMIP6. We have, however, only just begun to analyze these simulations (as can be seen by the fact that there are only few papers published comparing model responses across the LGM). Moreover, the CMIP6 LGM experimental design is ought to be submitted in an independent paper in this Journal, GMD, in a special issue of CMIP6. There is still a lot of work that should be done by the science community on the CMIP5 LGM simulations using this PMIP3 ice sheet. Through the analysis of the CMIP5/PMIP3 outputs, people need to examine how the climate are influenced by the different ice sheets. This is why it makes particularly important to explain how the ice sheet was constructed. We add a sentence in the discussions emphasizing that there is still ongoing work to analyze the LGM CMIP5 simulations. We add the following explanation.
-→
(page 4298 Line 18) "(… of this choice.) This paper provides the information on the difference between the individual ice sheets and the blended ice sheet as well as ice sheet configuration of previous phases of PMIP. It is important to promote further investigation of the impact of ice sheet configuration and the associated uncertainty in climate change."

Minor issues:
(2-2) "Eurasian Ice Sheet" vs. "Eurasian ice sheet" etc.: Both forms are OK (with a slightly different touch). However, capitalisation or non-capitalisation should be done uniformly.
→ These are unified to "Eurasian ice sheet".

(2-3) I suggest to replace "altitude" by "elevation" throughout the manuscript. Altitude is more commonly used for heights above some reference for points or objects _above_the ground (e.g., airplanes), while elevation is the preferred term for heights above sea level of locations _on_ the ground (e.g., the surface of an ice sheet).
→ Replaced accordingly.

(2-4) Page 4313, line 23: "observation margin" -> "observed margin".
→ We revise the text to clarify our point as following; (page 4313, L23-26, combining the first two sentences) "Even after carefully reconstructing the ice sheets by taking into all kinds of observational constraints, the way in which ice-sheet topography and extent are implemented varies between different climate models."

(2-5) Page 4314, line 21: "Jun'ici" -> "Jun'ichi" (I suppose).
→ Yes, thank you for the correction.

(2-6) Table 1: The notation for the latitude and longitude intervals is strange. Rather add square brackets, e.g., "-89.5, 89.5" -> "[-89.5, 89.5]". Further, the units are missing for all latitudes and longitudes.
-→ Revised accordingly, and the unit (degree) is inserted.

(2-7) Table 2: "Implied changes" -> "Implied changes (LGM - present)".
-→ Agreed. "Implied changes (LGM - present)".

(2-8) p. 4312, l.18. "implied" or prescribed?
-→ revised to "prescribed"

(2-9) Table 3: In all three "change" columns, the units are missing. As for the last column (resulting change in temperature Delta_tas), exactly what temperature is that? BTW, "tas" is a strange symbol for temperature.
→ Units and explanations are added as "Change in radiative forcing (W/m2) associated ….". The symbol tas is replaced by "global annual mean surface air temperature". Since indeed this was not unified throughout the figures, the unified explanation is now given in the figure caption, "The resulting change in global annual mean surface air temperature (in C) is also shown in the last column", and the text in the "change" columns of the table is revised to "Temperature change (C)".

(2-10) Figures 6-9: Units missing.
Units and explanations are added to clarify all figure captions.
Figure6…Estimation of the difference in radiative forcing (W m-2) at the LGM …..
Figure 7. Impact of ice sheet choice compared to CMIP5/PMIP3 composite ice sheet on mean annual temperature (degree C) in simulations made with the MIROC slab ocean model, for the ice sheet choice of (a) PMIP2, (b) ICE-6Gv.2, (c) GLAC-1a, and (d) ANU. The land mask (> 50% land) is shown in grey, the ice margin (> 50% ice) is shown in black on all four plots.
Figure8…Change in mean annual temperature (degree C)
Figure9…Implmented surface elevation (m)