Review of Banderas et al, 2017, GMDD

This study proposes and tests a new approach for creating a transient climate forcing from a small set of equilibrium climate model simulations and a proxy record index (e.g. Greenland ice core data). In the traditional "index" method only two simulated climate states are used, essentially describing all temporal changes with one climate anomaly pattern. In the here newly proposed method, long-term variability ("orbital") is separated from short-term variability ("millennial"), with each connected to a different simulated climate pattern/anomaly. This modification improves one of the weakest features of the "index" method, namely that it assumes that all climate variability over the assessed period is spatially constant (as they all follow one pattern).
The new approach is certainly interesting and could be published in GMD. However, the manuscript lacks some important discussion, and is in places not very clear. I therefore suggest a large number of general and more specific comments that I think should be discussed before publication in GMD.

GENERAL COMMENTS

1. The novelty of this new approach is not made very clear in the manuscript. Adding an extra spatial pattern for a certain frequency range will largely improve the "index" method. Please emphasize this. Also, the explanation of the traditional method (M1) is not very clear (e.g. Page 2, line 32 to page 3 line 3). Please rewrite.

2. These methods ("index" and updated "index") assume that temperature variations reconstructed over Greenland are representative for the entire Northern Hemisphere. Is this valid? Please discuss.

3. Even though having an offline climate forcing can be very useful, lately efforts have been made in coupling climate models of intermediate complexity (even CLIMBER specifically) to ice sheet models (see for example Bauer and Ganopolski, 2017). The impact of the lack of climate-ice sheet feedbacks in your approach needs to be discussed.

4. What is the resolution of the CLIMBER-3a model, and how does this (low) resolution impact the (40x40km) ice-sheet model results?

5. Which factors are used in the PDD scheme? And are they kept constant over the domain and different simulations? Bauer and Ganopolski (2017) suggest that when using fixed PDD factors one cannot realistically simulate the glacial evolution of the Northern Hemisphere ice sheets. Please discuss.

6. The study will largely improve when more paleo data is included. For example, the Eurasian data set covering 25-10ka of Hughes et al. (2016) could be used to evaluate which method captures the FIS transition from glacial to Holocene best. Also, is there any data indicating a different behaviour of the LIS vs FIS over this

period (see page 8, lines 23-31)? And are you sure there is no other independent (proxy) temperature data for the last 120ka?

SPECIFIC AND TECHNICAL COMMENTS

The following list of suggestions is intended to improve the readability of the text:

Page 1, lines 4-5: Rewrite to: "The impact of the climatologies on the paleo evolution of the NG ice sheets is evaluated."

Page 1, line 5: change usual approach to "index approach"

Page 1, lines 5-7: please rephrase. Maybe first mention the climate anomaly field and ice-core index, and then add this to PD climate?

Page 1, line 9: "corrected to provide a perfect agreement", this is called tuning.

Page 1, line 10: "recent" is confusing, because it could mean recently measured, or recently published.

Page 1, line 11: change "usual" to "index"

Page 1, line 13: rephrase to "results in a too small ice volume"

Page 1, line 18: change to "variability and improves the transient …"

Page 1, line 21: change to "need to be invoked to explain millennial-scale ice volume fluctuations."

Abstract and Discussion: would be helpful for the reader if the approaches/methods are numbered as in the rest of the text. For example: Our new method (M3)…

Page 2, line 2: move references to end of sentence, as these papers use proxy data.

Page 2, lines 9-10: These LIG values are not precise, but estimates. Add "roughly", "approximately" or similar.

Page 2, lines20-25; another paper that should be cited here is: Goelzer et al, 2016. Also, another approach to simulate paleo ice sheet evolution is by using ice sheet models with reduced complexity, forced by simple climate forcing (e.g. Langebroek et al., 2009).

Page 2, line 30: change to "lack of continuous spatially well distributed proxy data"

Page 2, line 31: “even based on simple assumptions” is very vague. Maybe leave out?

Page 2 line 32-page 3, line 3: please rephrase. Remember also that climatology is not the same as temperature.

Page 3, line 5: “latter” is not clear, do you mean “index” approach?

Page 3, line 7: change to “orbital climatic variations, while it is scaled following the characteristic time evolution of the index, which includes orbital and millennial-scale climate variability”

Page 3, line 9: change “two modes” to “orbital and millennial”

Page 3, lines 10-11: “As a result, this method can be expected to lead to a misrepresentation of millennial scale climate variability…”

Page 4, lines 12-13: Basal melting is no “surface” boundary condition. Please rewrite.

Page 4, line 16: What do you mean with linear atmospheric profile? Do you just adjust the temperatures with a temperature lapse rate depending on elevation? What is the lapse rate value (degC/km)?

Page 4, line 19: which PDD factors are used?

Page 4, line 20: Is basal melting not also depending on the ice thickness?

Page 4, line 21: change to “in regions where the ocean floor is below 450m …”

Page 4, line 25: definition of PD should be stated first time present-day is used

Page 4, lines 25-26: Change to: “PD climatology obtained from observational data, with simulated climate snapshots of the last glacial cycle and a time dependent index derived from proxy records.”

Page 4, line 34: “using the ICE-5G topography”

Page 5, line 1: How do the ocean temperatures impact the ice sheet model results, if the basal melting is fixed?

Page 5, line 5: “index” instead of “usual”

Page 5, equations: Maybe I miss something, but wouldn’t it be easier to use something like T(t) = T0 + gamma(t) * dTorb; with gamma = 1 for LGM, and gamma = 0 for PD ?

Page 5, line 11: delete “previous”

Page 5, line 11: preindustrial or rather PD?

Page 5, lines 13-14: delete “time” before index. “Thus, the index dictates the timing of both orbital and millennial-scale variability.”

Page 5, equation (5): Maybe you have to emphasise that “gamma = alpha + beta”

Page 6, lines 22-23: Rewrite to: “… NGRIP ice-core location. This tuning to the NGRIP KV reconstruction also introduces a scaling of the synthetic…”

Page 7, line 12: Change “indirect measurements” to “reconstructions”

Page 7, line 13: Change “ As an initial proof of consistency” to “We first compare”

Page 7, line 26: Change “climatologies” to “temperatures”

Page 7, line 32: change to: “… suggesting an too low amplitude …”

Page 8, line 1: It is very difficult to see the amplitude of the M2007 reconstruction. Maybe it would help if this is also shown as a (dashed?) line? What is the sample resolution of this core? Is it high enough to capture the high variability of the simulated temperature evolution?

Page 8, lines 15-16: Several geological time periods are mentioned here (Eemian, Holocene, MIS2 and MIS4). Their ages need to be stated. It would also be very helpful if these periods are indicated in the figures.

Page 8, lines 20-22: Change to: “… only at the millennial scale set by the difference between the PD and interstadial temperature fields used in…”

Page 8, line 24: change “reflecting the fact” into “meaning”

Page 8, line 33: “forcings to” instead of “methods in”

Page 8, line 34: Figures S2 and S3 do not really show any orbital climate variability

Page 9, line 5: change to: “… reconstruction, the climates of M2 and M2 are identical at orbital timescales, and only differ at …”

Page 9, line 5-7: Unclear, please rephrase. Do you mean that the orbital patterns are used to explain the millennial changes, and because the orbital changes are large, the response is also (too) large?

Page 9. Line 11: change “ its larger orbital amplitude” to “tuning to the lower NGRIP temperature”

Page 9, 12-13: change to: “The temperature fluctuations in M3 incorporate both the larger orbital and the smaller millennial amplitude fluctuations compared to M1.”

Page 9, line 15: SLE difference is maybe on average 20 m, but not “constant”. Also it is not clear from the figures that this difference is larger for LIS than FIS. Please quantify by taking some mean.

Page 9, lines 15-16: “The intermediate case M2 follows more closely M1 in the LIS, but M3 in the FIS.” What does this mean? Please elaborate.

Page 9, line 16: There is no clear drop at 55 ka, in Figure 6. Maybe it is rather around 58 ka, or 48ka? Please update, and indicate in Fig 6 which drop is meant.

Page 9, lines 22-28: What is the timing of these D/O events? Please indicate in Fig 5&6.

Page 9, line 23: Which positive feedbacks? Please discuss.

Page 9, line 33: I think Figure 5 should be cited here instead of Fig. 4

Page 9, line 34: “our view” is very vague. And there is not much data to support it. Maybe best to rewrite to say that M3 is the most advanced method or so?

Page 10, line 3: Change to “Figure 4b”

Page 10, line 5: the wording of “divides” is confusing here. Do you mean ice sheet divides or continental water divides?

Page 10, lines 4-7: The LGM distribution from M3 is very different from M1 and M2 (as shown as Supp figures). Maybe this could be more quantitatively compared to a dataset (Peltier?), and used as argumentation that M3 is the best method?

Page 10, line 11: add “Hughes et al., 2016”

Page 10, line 14: The Supplements do not really show climate variability.

Page 10, line 16: This is actually “a new method”, not “2 methods”.
Page 10, lines 22-23: Change to: “Depending on the frequency either the glacial-interglacial climate anomaly field (orbital variability) or the stadial-interstadial field (millennial) is varied.”

Page 10, line 24: change to: “… and millennial-scale variation are tuned to fit the Greenland ice-core record.”

Page 10, line 32: change to “The different climatologies have a large impact on the development of NH ice sheets…”

Page 11, line 9: change “these sites” to “this region”.

Page 11, line 21: Change “Improving its representation” to “Including millennial-scale patterns”

Page 11, line 31: Hughes et al. (2016) suggests ~23m. Again, it would be helpful if the values are also indicated in the figures, as well as the timing of the LGM.

Page 11, line 34: Would be useful to add the sea-level curve from Fig. 1a in Fig. 6 in order to see the difference in reconstructed and simulated variability.

Page 12, line 1-2: Please also discuss here the missing feedbacks between climate and ice sheet in this offline method (e.g. albedo effect).

Page 12, line 13: Change “therefore” to “apply that to”

Figure 1: a) The sea-level curve is not used as forcing, or? Then please delete “forcing”

Figure 1: Is the VK index only derived from NGRIP? If not, please rewrite figure caption.

Figure 1: The shading is difficult to see, could you make it less transparent?

Figure 2: Can you add the locations of the analysed sediment cores?

Figure 3: “obtained by”; is it Martrat et al., 2014 or 2004?

Figure 4 is not mentioned in the text until after Fig 5&6. Maybe change the order?

Figure 6: What are the initial conditions, how much ice, and where? Maybe easier to make this graph relative to today? (is probably very similar)

Please make sure that the website storing the results is available.

REFERENCES

Bauer, E. and Ganopolski, A.: Comparison of surface mass balance of ice sheets simulated by positive-degree-day method and energy balance approach, Clim. Past, 13, 819-832, https://doi.org/10.5194/cp-13-819-2017, 2017.

Goelzer, H., Huybrechts, P., Loutre, M.-F., and Fichefet, T.: Last Interglacial climate and sea-level evolution from a coupled ice sheet–climate model, Clim. Past, 12, 2195-2213, https://doi.org/10.5194/cp-12-2195-2016, 2016.

Hughes, Anna L C**;** Gyllencreutz, Richard**;** Lohne, Øystein S**;** Mangerud, Jan**;** Svendsen, John Inge (2015): The last Eurasian ice sheets - a chronological database and time-slice reconstruction, DATED-1. *Boreas*, 45(1)**,** 1-45.

Langebroek, P. M., Paul, A., and Schulz, M.: Antarctic ice-sheet response to atmospheric CO_2 and insolation in the Middle Miocene, Clim. Past, 5, 633-646, https://doi.org/10.5194/cp-5-633-2009, 2009.