Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-158-RC2, 2017

Geoscientific Model Development Discussions

Interactive comment on "A new approach for simulating the paleo evolution of the Northern Hemisphere ice sheets" *by* Rubén Banderas et al.

Anonymous Referee #2

Received and published: 26 November 2017

Banderas et al. study provides a method based on climate index to simulate the evolution of Northern Hemisphere ice sheet reconstructions through the past glacial period (110k - 10k). Instead of using one index, derived from NGRIP ice core record, or insolation changes, as traditionally done by many other previous works using the index approach to simulate NH glaciations, they provide three different indices, varying in the complexity, the most complex one including some millennial scale variability. They then use the 3D ice-sheet model GRISLI to simulate the transient evolution of the Laurentide and the Eurasian ice sheets through the last glacial period. They conclude that the index including the millennial scale variability provides the most satisfying results in terms of extent and volume of the two ice sheets at the LGM and during MIS3.

While the authors present the work as a novel approach, this is, to my opinion, only a

different way of using the index method and this is not particularly novel in the sense that previous studies were able to also get satisfying results with more simple index approach. In particular, if the aim of this study is to show the impact of including the millennial scale within the simulations, then I find that the result analysis is not enough to support the conclusions, especially from statistical and point of view. For example, the simulated LGM ice sheet extent underestimates the reconstructed Laurentide extent and overestimates the Eurasian one. In addition, I wonder why the authors did not derived the entire glacial period until 10k, since from LGM to 10k, the new DATED-1 reconstruction of Eurasian ice sheet extent could also provide a very strong way of validating the present work. Only few simulations with specific ice-sheet model parameters settings are presented here, whereas most of the ice-sheet parameters that have been chosen strongly impact on the final conclusions. To substantially strengthen this study, similar ice sheet simulations varying the key PDD parameters, calving and oceanic melt rates are necessary. In addition, you never mention the hydrology that you use at the base of the ice sheet. In GRISLI this particularly important because according to the criterion used, you can or not trigger the SSA on larger domains. This also could enhance the sensitivity to climate forcing and then show different response than the one you show here. From this point of view, assumptions on which this work relies are mentioned but poorly discussed or missing. Furthermore, the authors try to account for the millennial scale variability in their simulations but they removed the contribution from ocean by imposing 0 melting in the shelf expansion areas and keeping this fixed for the entire simulations. I find that this is a weakness of this approach and should be combined with additional simulations in which the imposed value could be also derived from index (2012), as done by Pollard and De Conto (2012) for example. Based on the results then the authors could strengthen their discussion and conclusions. I detail below my specific comments. In its current state, this study requires further substantial investigations and improvements before publications.

General comments:

Validation of the simulations: the validation work is not enough and requires more elaborations.

1- Why not running the experiment until 10k, this does not cost much more in terms of resources because you run at 40k and one simulation takes at max 12h-18h and you can actually also validate your results with DATED-1 (Hughes et al., 2016) for Eurasia. Because you mentioned many times in the manuscript that you lack of proxies for a proper validation. This is one way of doing it. I would like then to see your deglaciation simulations with the different indices and the match with DATED-1 extent or for example Patton et al. (2016) modelling work.

2- Could plot the extent of ICE-5G for the Laurentide and of DATED-1 at LGM on your Figure 4? So one can appreciate the performance of your index?

3- You can also validate the elevation changes over Greenland, since you have the NGRIP records etc.., which you never show. You could follow the paper by Quiquet et al. You do not use a lot the work by Kleman et al. (2013). They tried to bound the extent with proxies. So I advice you to you their reconstruction to support your simulations.

4- I am not convinced by the comparison between indices and SSTs is uncertain. Because you mentioned it several times, I would suggest to remove those parts. You don't know if air T° and SST always co-vary with a similar amplitude. To me, this part is a bit weak.

5- You never show how precipitations evolves your indices. Actually d18O of NGRIP is more representative of precipitation than temperatures. Please, also show precipitation.You could use speleothems from the Mediterranean and other places to validate your derived precipitations.

6- the index method main weakness is to not account for changes in circulation, therefore the climate-ice-sheet feedbacks are mainly missing. This is the most important weakness of this method. Please also mention it in the method and in the discussion.

Methods: I find the methods not clear enough

1- about the explanations of climate snapshots: please provides more informations about the resolution of the forcing, which matters a lot for downscaling. Provide also informations about your downscaling procedure.

2- I would like to see a Table of the main parameters used in the ice sheet simulations: in particular, PDD parameters, calving threshold, lapse rate, basal drag etc. . .All those parameters matters a lot in your case and you never discuss this.

3- NGRIP is not representative of North America and Eurasian circulation changes. This is one of the assumption you never discussed in the paper. In your Figure 5, for example, Eurasian and North America have the same trend. Which is not the case in proxies and strongly depends on the region. Why not comparing the transient simulated evolution of the CLIMBER experiments over Greenland for sure, but also over Eurasia and North America to see the difference generated by circulation and regional changes. Then insert a justification on the fact that you use Greenland ice core records to derived your indices. From this point of view, I don't see the improvement compared with traditional index methods. A way that would have been perhaps more robust would have been to derived indices from the transient climate simulation itself over several regions, used several indices in your ice sheet simulations and add the part of missing variability to each index as you do with your method M3.

4- You never mention the criterion used to trigger the SSA on continents. GRISLI uses the Shelfy-stream approximation, this has a lot of importance for the simulated velocities and volume. A part about this aspect and discussing your choice is necessary to support your analysis.

5- I find highly necessary that you test your hypothesis against different values of oceanic melt rates and calving values because this also has a lot of influence on the transient evolution. When you do steady-state, it is important to prescribe reasonable value to affect the grounding line in a realistic way. This is even more important in the case of transient simulations. I would thus suggest to add new simulations, in addition to the ones already performed and presented here, varying those values and also test the importance of the calving criterion. Here you set ad-hoc at 0m/yr and larger at depth greater than 450 m. First of all, you really force the answer of your simulations by doing so and you artificially get rid of the calving issue. Second, 0m/yr might be valid for the LGM, but not for the other previous periods. Thus, You should test this assumption and change your method/value for oceanic forcing.

6- You chose to use the PDD method to simulate ablation. Why not using ITM for example, that would also provide a different view of the impact of your index and would be also perhaps more indicated to catch the millennial scale variability embedded in your indices. PDD might strongly dampened the effect of your indices here. In addition to this, you also use a fractionning of precipitation and snow that is very drastic, based on the temperature threshold of 2°C. (Why not using the method from Marsiat (1994) as many of GRISLI studies do, just to test a different criterion of precip/snow

fractionning?). One cannot test everything, but here you are looking at evolution of mass balance, so those aspects matter.

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