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Geoscientific Model Development Discussions

Interactive comment on "Simulating the Early Holocene demise of the Laurentide Ice Sheet with BISICLES (public trunk revision 3298)" *by* Ilkka S. O. Matero et al.

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General comments:

In this manuscript, the authors use an advanced marine ice sheet model (BISICLES) to simulate the demise of the Laurentide Ice Sheet during the early Holocene (10-7 ka). In particular, the main goal is to simulate a surface mass balance instability event over the ice sheet (known as 'saddle collapse', .ca 8.2 ka) using an ice sheet model with unprecedented horizontal resolution/representation of ice dynamics and ice-ocean interaction at the ice sheet marine margins. The simulations' initial conditions are based on the ICE-6G_c reconstruction (ice thickness, topography, bathymetry) and on a pre-

vious ice sheet modelling study as concerns the ice temperature. The transient climate forcing is derived interpolating between 500-year intervals climate snapshots simulated with the HadCM3 GCM. The ice sheet sensitivity to different parameters concerning ice dynamics, sub-shelf melting and climate forcing is tested in individual simulations. A simulated deglaciation scenario in agreement with GIA-modelling (ICE_6G_c and GLAC-1d) and empirical reconstructions (Dyke et al. 2004) is presented. The associated meltwater flux magnitude and timing are analysed and compared with estimates based on geological records. Finally, the authors highlight that the ice sheet demise and the associated freshwater fluxes are highly sensitive to the basal traction coefficient and the surface mass balance, with changes in sub-shelf melting and model resolution having a limited effect on timing and duration of the freshwater flux pulse.

The simulations analysed in this manuscript present very advanced modelling features in terms of horizontal resolution (Adaptive Mesh Refinement) and representation of ice dynamics (higher-order approximation of Stokes flow, crevasse calving model). Moreover, this ice sheet model has never been applied before to continental-scale size ice sheets over paleo timescales. For this reason, I think that this manuscript has the potential to provide an essential contribution to the field of paleo-ice sheet modelling.

However, at this point there are some key aspects of this manuscript that need to be reviewed before it can be considered eligible for publication. I think there is need of a deeper analysis to assess the influence of the method used to initialise the simulations on the ice sheet deglaciation and dynamics. Moreover, this manuscript will largely benefit from a more detailed description and analysis of the simulated ice dynamics (evolution of fast-flow areas, calving, grounding-line migration) and the impact of the Adaptive Mesh Refinement on these processes. These aspects represent the main source of innovation of this study and justify the use of a relatively expensive (in terms of computational time) ice sheet model. However, in both results and discussion sections very little/no space is dedicated to this. Therefore, I think this manuscript should be reconsidered after major revisions.

Specific comments:

- All the simulations are initialised starting from (a) ICE-6G_c ice thickness from 10 ka time slice (b) Gregoire et al., 2012 ice temperature from 9 ka time slice, plus 0 °C throughout the ice column outside the ice extent in Gregoire et al., 2012. It is not clear whether the ice velocity is initialised from 0, or a similar approach as for the temperature is used. From the text, it seems that all the simulations are started at 10 ka without previous initialisation of the model thermodynamics. It is remarked in the text that the 2500 simulated years-long 'control' simulation proves that the ice sheet is not in equilibrium with the 10 ka climatic forcing. The choice of not running an initialisation simulation seems to be justified in the text with the sentence "By the early Holocene, the LIS has significantly retreated from its Last Glacial Maximum position and is far from being at equilibrium with the climate". I think that this choice and its implications on the evolution and dynamics of the Laurentide Ice Sheet between 10 and 7 ka deserves a deeper analysis – perhaps to be included in Supplementary Materials. Spin-up simulations are generally 100,000 years-long runs that are used to bring the ice thermodynamics in equilibrium with the climate. Transient simulations of the last deglaciation are generally done starting either from equilibrium-type spin-ups at 21 ka or transient spin-ups starting from the last interglacial (120 ka) and ending at 21 ka (like, for instance, is done in Gregoire et al., 2012). I understand that a similar approach is unfeasible with BISICLES, due to its large computational costs, and this manuscript focus on a shorter time interval (10-7 ka) during which the ice sheet is not in equilibrium with the climate. However, you should still ensure the reader that the method you are using is not producing model artefacts in your simulations due to (1) ice velocity initialisation (2) ice temperatures simulated in Gregoire et al. 2012 had different climate forcing than in this study. It's not fully clear to me whether your climate forcing is the same as in Gregoire et al. 2012 – likely not, as it is PMIP4 – but anyway you take a '9 ka ice temperature' and then you apply a '10 ka' forcing. Also, how areas starting from 0 °C are responding? Do you lose these areas? Perhaps you should include a figure where you show areas with different initialisation for the ice

temperature. I also think you should provide more information on the ice temperature and velocity evolution, both 2D maps and averaged curves. In Figure 3, you show ice velocities at year 50; these velocities does not exhibit clear ice stream patterns, they only seems to be quite high in marine margins - which makes sense, considering how you treat the basal drag coefficient. But is this realistic compared to reconstruction (Margold et al. 2018)? Is this velocity pattern constant or there is a lot of change in fast flow pattern/magnitude? I think that analysing these things in the 'control' simulation might ensure the reader that the deglaciation pattern is 100% caused by changes in your forcing and not because the ice thermodynamics is still in the initialisation phase.

- As stated in the manuscript, using BISICLES allow to simulate the Laurentide Ice Sheet evolution between 10 and 7 ka with high resolution (through the Adaptive Mesh Refinement) and advanced representation of ice dynamics (higher-order approximation of Stokes flow, crevasse calving model). However, I think that the results presented/discussed in this manuscript do not expand enough on this topic, showing mainly ice volume curves and ice thickness maps. I think that aspects of the ice dynamics for which BISICLES is known to be very good (evolution of fast-flow areas, calving, grounding-line migration) should be analysed more and should represent an important (if not central) part of the paper. In the way results are presented, it is hard to understand why it is important/necessary to use BISICLES for this study. The role played by simulated ice dynamics throughout the deglaciation is only assessed indirectly in the sensitivity tests where lower values of the basal traction are considered (with a quite straightforward results). However, there are many ice sheet models less advanced and computationally expensive than BISICLES including a basal drag coefficient – that would likely give the same result in sensitivity tests designed as in this study. Instead, you should try to show why it is really important to use BISICLES for this study, what we are learning from this advanced model. I think for instance that the freshwater flux evolution should be analysed by looking at individual contributions of different processes (sub-shelf melting, calving, runoff). I think you should also include ice velocities maps throughout the deglaciation in the 'standard' simulations (and in the sensitivity tests, perhaps as supplementary material), showing grounding line migration/ice shelves extent. This could be done for instance for specific snapshot close to the 'saddle collapse' event. Also the Adaptive Mesh Refinement should be discussed more: where, in the grid refinement simulations, the resolution is increased? What are the differences in terms of ice dynamics in these grid points? Looking at the overall volume and freshwater fluxes there is apparently no changes, but how about locally and how about individual processes (sub-shelf melting, calving, runoff)? And if there is still no differences, what we can learn from this experiment? Overall, I would like to see discussed in the paper why the BISICLES simulations performed here tell us more than simulation performed with less advanced ice sheet models. Which were the main dynamical processes occurring during the 'saddle collapse'? If the reader would repeat the same study as in this manuscript with a less advanced model, would he obtain the same results? Why?

Technical corrections:

- Figure 1: it is difficult to identify geographic locations in all the panels, I think you should either add some geographical references to the figures or include a map of the study area. Also adding a map outline would be useful to the reader (like in Figure 3, which is really clear). Moreover, it is difficult to compare panels (a), (b) and (c) as the domain/projection is always different. You could (1) show the three panels in the same ice sheet model domain/projection (2) show only panel (c), adding another panel with geographic locations of the study area. It would be also good to have lat/lon tick marks, instead of native distances.

- Figure 2: in panel (a) caption, you say that colours are yellow, dark blue and green. I can only see yellow, purple and a very tiny portion in light blue/purple. In both panels, it is difficult to identify the geographic locations. You could add a map outline (again, like in Figure 3) and maybe the ICE-6G and GLAC1d ice sheet extent at 10 ka.

- Table 2: you could insert one or multiple last rows with some estimates from geological

records, so that the reader can get an idea of whether individual simulations do a good/bad job in reproducing the peak in freshwater fluxes by looking at this Table.

- Line 17 in Section 4.2: I think you should make more clear (not only here, but in the whole manuscript) when you refer to GIA-modelling reconstructions (ICE6G, GLAC1d) or to fully empirical reconstructions (Dyke 2004, Margold 2018). You use sometimes just 'reconstruction'.

- Line 21 in Section 4.4: you say that the basal melting rate from the geothermal heat flux is negligible and you set it to 0 m/a. This is fine, but how about frictional heating? You should distinguish between geothermal and frictional heat fluxes, otherwise the reader could think that both contributions to basal melting are set to 0 m/a.

- Line 25 in Section 4.4: you should also mention what is happening in terms of oceanic circulation (AMOC strength, warm subsurface Atlantic water export in the North Atlantic) and what are the possible implications for the marine-based sectors of the LIS.

- Line 37 in Section 4.4: it is true that 1 degree resolution in ocean models does not allow to resolve coastlines and shelf cavities. However, simple 2eqs. and 3eqs. sub-shelf melting formulations are forced with far-field ocean temperatures (so, away from the shelf cavities and coastlines). I think it is ok to force your simulations with constant values, but it's not necessarily true that it is a better approach then using transient curves or transient ocean properties based on GCM simulations.

- You could include one or two tables with values for the ice sheet extent/volume at different time slices in 'standard', ICE-6G and GLAC1d, Dyke et al. 2004.

- To increase readability, I suggest to use the same unit for simulated years and dates (in ka). It is difficult to read (for instance) year 1400 and then think that corresponds to 8.6 ka.

- To increase readability, I think the English language needs to be improved.

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