Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-79-RC2, 2018

Climate of the Past
Discussions

Interactive comment on "Instability of Northeast Siberian ice sheet during glacials" *by* Zhongshi Zhang et al.

Anonymous Referee #2

Received and published: 6 September 2018

The authors performed simulations of ice sheets under typical glacial conditions using a climate-vegetation-ice sheet model where climate and ice sheets components are coupled asynchronously. The authors found two fundamentally different ice sheet configurations and speculated that spontaneous transitions between these configurations might have happened during glacial cycles. However, I believe that this rather surprising result is an artefact of the flawed methodology employed in this study and therefore it cannot be applied to the real world. This is why bellow I discuss only the methodological aspects of this study.

The authors introduced their model as "state-of-the-art high complexity earth system model" and consider this fact as the "unprecedented advantage" (p. 3) compared to the previous works made with simplified climate models. However, the appropriateness

of the model for the purpose of the study is defined by its most crucial component(s) rather than complexity of the most complex ones. Arguably, for modeling of climate-ice sheet interaction, correct simulation of the surface mass balance (SMB) of ice sheets is of fundamental importance. The authors used a complex but rather coarse-resolution climate model. With its spatial resolution of ca. 400 km, the most important part of ice sheets – the ablation zone - cannot be properly resolved. This is why the accuracy of climate fields simulated over ice sheets depends on the elevation corrections. The authors corrected temperature by using a constant lapse rate and precipitation is assumed to be proportional to the exponent of temperature. Such an approach has been used at least since 90th, mostly for modeling of the Greenland ice sheet response to future climate change. For the centennial timescale and not too strong climate change, this approach can be somehow justified. However, the applicability of this approach to modeling of glacial cycles is questionable at best. In particular, this simple elevation correction does not account for the ice-albedo feedback and therefore cannot be used for modeling ice sheet configurations substantially different from that has been used in climate simulation. Even worse is the situation with precipitation. Precipitation field over ice sheet is strongly topographically controlled and changes in precipitation patterns due to changes in ice sheet extent and elevation cannot be captured by simple temperature correction. Even more in conflict with claimed "high complexity" is the use of the Positive Degree Day method to calculate surface melt. Not only this method is oversimplistic, its inappropriateness even for modeling of the Greenland ice sheet has been shown in numerous studies (e.g. van de Wall, 1996; Bougamont et al., 2007; van de Berg et al., 2011). Equally questionable is using of equilibrium approach for modeling of ice sheets evolution during glacial cycles since the timescales of ice sheets response to orbital forcing are close to periods of orbital forcing.

All these problems significantly affect the reliability of modeling results but, admittedly, they are not unique for the study by Zhang et al. Unfortunately, there is one aspect of this study, namely the way how the asynchronous coupling is implemented, which I believe is fatal for the findings presented in the manuscript. This problem is related to the implementation of asynchronous coupling technique. Asynchronous coupling has a long history in climate modeling (Manabe and Bryan, 1969). This method is based on the assumption that model results are not seriously affected if the fast component of the system (in this case climate component) is run only a fraction of the full integration time of the slow component (ice sheets). This allows one to reduce significantly computational cost. The method is rather flexible but two conditions have to be met: (i) simulation time of the fast component should be sufficient to allow this component to adjust to the evolution of the slow component; (ii) the periodicity of coupling between fast and slow components should be small enough to ensure that changes of the slow component between coupling events are small enough. If these conditions are met, one can expect that the solution obtained with asynchronous coupling is close enough to that would be obtained with synchronous coupling. For modeling of climate-ice sheet interaction, I would guess that the duration of 100 years for each AOGCM run and coupling between climate and ice sheet components every 1000 years would be a reasonable choice. By the way, such choice would require roughly the same amount of computational time to reach an equilibrium state as the authors actually used. However, for the reason the authors did not explain, they used coupling periodicity of 100,000 years instead. As the result, most of the time (see Fig. 3) the ice sheets evolve under the influence of climate forcing which has been computed for the completely different ice sheets configurations and the simple elevation/temperature correction technique cannot handle this problem. The consequence of this problem is seen in Fig. S2 and S3 (why these two crucial for understanding figures are hidden in SI is not clear to me). After several iterations, the model began to "oscillate" between two completely different ice sheet configurations: "Laurentide-Eurasian" and "circum-Arctic". For both states, the ice sheets and climate are inconsistent with each other as clearly seen in Fig. 3. For example, climate computed for the circum-Arctic configuration leads to a rapid (several thousand years) melting of the huge Eastern Asian ice sheet while climate computed for the Laurentide-Eurasian configuration leads to a rather fast (ca 10 ka) buildup of the Eastern Siberian ice sheet. None of these two ice

sheet configurations can emerge in the real world because this result solely originates from the inappropriate coupling technique. This is why I must conclude that the "instability" of the East Siberian ice sheet found by Zhang and co-authors is just a numerical instability which has nothing to do with the instability of real ice sheets. The closest analogue for such sort of instability is the "checkerboard instability" which arises in the ocean or atmosphere models if the time step is inappropriately chosen.

I realize that the authors put considerable efforts in preparation of the manuscript. However, because this study is based on flawed methodology, the manuscript cannot be simply “revised”. If the authors believe that the role of atmospheric stationary waves for the evolution of the Eurasian ice sheet during glacial times has not been yet properly investigated in the previous studies, they should redo all simulations using correct methodology.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-79, 2018.