Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-1-RC2, 2020


Climate of the Past
Discussions



# *Interactive comment on* "The phase space of last glacial inception for the Northern Hemisphere from coupled ice and climate modelling" *by* Taimaz Bahadory et al.

**Andrey Ganopolski (Referee)**

andrey@pik-potsdam.de

Received and published: 31 March 2020

The manuscript by Bahadory et al. presents a large ensemble of transient simulations of the last glacial inception performed with a coupled climate-ice sheet model of intermediate complexity. This is an interesting paper which provides a new insight into the mechanisms of glacial inception. However, to be suitable for publication in CP, the manuscript requires a number of clarifications and more critical discussions of potential caveats. Below I describe my major concerns and suggestions.

1. Phase space of last glacial inception

The meaning of the term "phase space of last glacial inception" which authors put in the title and mentioned several times in the text, is unclear to me. Since "phase space" is space, their dimensions (axis) should be properly defined. For example, for mechanical systems, phase space is defined by coordinates and momentum. For the climate system, Fig 3b in Ganopolski et al. (2016) gives an example of another phase space. Here the position of glacial inceptions (bifurcation point) is shown in the insolation–CO2 phase space. The authors should either clearly define what they understand under "phase space" in their manuscript or abandon this term. A similar situation is with the term "bifurcation" which authors used several times (p. 16 and 22) but the meaning of this term remains unclear.

2. Introduction

The authors devoted less than one page for discussing previous modelling works related to the last glacial inception. Apart from several own papers, they only cited my publications (Calov et al. (2005); Calov et al. (2009 ) and Ganopolski et al. ( 2010)) and the only information Bahadory et al. provide about our works is the spatial resolution of the CLIMBER-2 model: "The model used in that study employed very low resolution (51°longitude by 10°latitude for atmosphere and approximately 100 km for the ice sheet model)" (page 2, line 47). Amazingly, just 20 lines below (page 3, line 66) the authors again describe the spatial resolution of CLIMBER-2: "A further limitation in this latter study is that the CLIMBER EMIC employed uses a 2.5D statistical-dynamical atmospheric model with very limited longitudinal resolution (51.4o) and a 3 basin 2D ocean model". Do the authors realize that they describe the resolution of the same model twice? In any case, I believe that the manuscript by Bahadory et al. will benefit a lot if, apart from the resolution of CLIMBER-2, the authors will discuss also other relevant publications. Already in Calov et al. (2005), we cited in the introduction more than 25 modeling papers and since then the number of relevant publications increased significantly.

3. Temperature biases and realistic simulations of ice sheets extent

The authors stress in the manuscript that they do not use any climate bias corrections and I fully agree that bias correction represents a trade-off between internal consistency and the realism of past climate simulations. All climate models have biases and for simulations of quasi-linear response of the system (like CO2 increase), climate biases are likely not very important. However, for simulation of glacial inception, which is a fundamentally nonlinear process, temperature biases can be much more important because their magnitude can be comparable with the climate response to orbital forcing. In their previous paper (Bahadory and Tarasov, 2018), the authors wrote a lot about temperature biases but provided no information about spatial patterns and magnitude of temperature biases. Table 3 only indicates that average temperature over the box covering most of Canada is close to reality. The real problem is, however, not average but strong (5 to 10oC depending on the season) zonal temperature gradient over northern North America related to the atmospheric circulation and explained by quasi-stationary planetary waves. Due to the coarse spatial resolution of CLIMBER-2, this effect is not resolved and this leads to a strong, dipole-like temperature bias (Ganopolski et al., 2010; Fig 2a). This is why it is noteworthy that the North American ice sheets simulated without temperature bias correction in Calov et al. (2005a) (Fig. 6) and in Bahadory et al. (Fig. 4 and 5) are very similar with the thickest ice located over Alaska. Introducing of temperature bias correction in Ganopolski et al. (2010) led to a very different ice sheet evolution which we believe is more realistic. The similarity between Bahadory et al. and our old results (Calov et al. , 2005) can be caused by the fact that the LOVECLIM model, in spite of a higher spatial resolution, has a rather simplistic atmospheric model which results in similar to CLIMBER-2 temperature biases. At least, this is what one can conclude from Fig. 1b in Heinemann et al. (2014), another paper based on the LOVECLIM model. By the way, in this paper temperature bias correction has been used. Bias correction has been used also in a number of GCMs studies such as Vizcaino et al. (2008); Herrington and Poulsen (2012). This is why, it would be useful to show present-day (preindustrial) summer temperature biases simulated by LOVECLIM model used by Bahadory et al. This can be, for example,





Printer-friendly version

Discussion paper

the average value over the 50 ensemble members or a single representative one. Of course, it is up to the authors to decide which technique to employ but they should inform their readers about potential serious drawbacks.

4. Present-day constraints on model parameters

On page 10 the authors wrote that “despite having different start times (due to different calendar start years between 122 ka and 119 ka ...), all simulations start growing ice in the first 100 years of simulation”. It is not clear from the paper which runs started at which time, as well as why start time was chosen differently for different runs. However, the fact that according to Fig. 1 the model simulates between 10 to 20 meters sea level drop already at 119 ka is worrisome. Indeed, since climate before 120 ka was similar to preindustrial one or even warmer, such rapid ice sheet growth at the beginning of model runs indicates that at least some model realizations would simulate glacial inception already during the late Holocene which, of course, is in odd with observational data. In Section 5, Bahadory and Tarasov (2018) wrote that they used “a trial criteria based on ice volume changes (between 1700 and 1980 CE)” to reject model versions which grow “too much” ice during this interval. But this interval is much too short for such a test. For example, Fig.3 in Bahadory et al. clearly shows how much ice is formed after year 200 since the beginning of the runs. Since summer insolation and GGHs concentrations remained practically constant at least since 1000 BCE till ca. 1900 CE, testing of whether or not selected model versions simulate glacial inception in the late Holocene would require at least 10 times longer runs than have been performed by the authors. To be able to judge their realism, it is crucially important to know how much “present-day” ice is simulated by different model versions.

5. Spatial patterns of simulated North American ice sheet

When discussing spatial patterns of simulated Laurentide ice sheet, the authors wrote “to our knowledge, there is no community-based geologically-inferred MIS 5 ice margin reconstruction for NA. Aside from the issue of Alaska (and certain adjacent parts of the

Yukon), our results are, within (large) age uncertainties, consistent with the till stratigraphy presented in Clark et al. (1993)" (page 22). However, Clark et al (1993) explicitly stated that "the Laurentide Ice Sheet first developed during Stage 5 over Keewatin, Quebec and Baffin Island" (page 79) which is inconsistent with the results presented by the authors. It is also noteworthy that the recent reconstruction of NH ice sheet for the MIS 5d presented in Batchelor et al. (2018), also places MIS 5d Laurentide ice sheet over northern-eastern Canada and implies very little glaciation over Alaska and in Western Canada. Since I am not an expert in the history of glaciation of North America during the last glacial inception, I wonder what the authors think about these apparent inconsistencies? And if they really believe that there are no reliable reconstructions for the ice sheets during MIS 5d (I do not understand the meaning of "community-based"), then what is the motivation for performing a large ensemble of transient last glacial inception simulations?

6. Using simulations of glacial inceptions to constrain transient climate response

The authors devoted only one paragraph in the text to the description of how they used their model ensemble to constrain transient climate response (TCR). However, they highlighted this result in the abstract where they suggested that their results can be used "to constrain future climate change". Since future climate change is a very hot issue, this small part of the manuscript deserves serious attention, especially, because the authors put a very tight constraint on TCR (0.7-1.4 C). If their estimate of TCR is correct, then only five of ca. 30 different GCMs participating in CMIP5 have the right TCR while all other overestimate it. Obviously, such a claim has very serious implications for future climate change projections. Below I argue why simulations of glacial inception cannot constrain future climate change.

Although a number of attempts have been made to use paleoclimate data and results of paleoclimate modelling to constrain equilibrium climate sensitivity (ECS), these studies cannot directly constrain TCR. Indeed, although there is some correlation between TCR and ECS, this correlation is not very tight and TCR of different models with similar





Printer-friendly version

Discussion paper

ECS can differ by factor two. The reason is that TCR strongly depends on the rate of ocean heat uptake which differs significantly between climate models. Obviously, simulations of glacial inception provide no constraints on ocean heat uptake. This is why below I only discuss whether simulations of glacial inception can constrain ECS.

i) Climate sensitivity to CO2 doubling (ECS) and the response of climate to seasonal and latitudinal redistribution of insolation are caused by completely different forcings an, therefore, numerous processes and feedbacks play a completely different role. I am not aware of any study about the relationship between regional and seasonal climate response to insolation change and global climate response to CO2 change (ECS), but I doubt whether there is a strong correlation between these very different climate changes.

ii) As far as the simulated rate of ice sheet growth is concerned, the situation is even more complex because ice sheet growth is controlled not only by simulated climate change but by many other factors. The first one is model biases in modern climatology. If these biases are comparable with climate response to orbital forcing, then there is a big question of whether ice sheets growth can constrain future climate change. Second, ice sheet response to orbital forcing strongly depends on surface mass balance parameterization. The authors used the PDD scheme which does not even explicitly account for the direct impact of orbital forcing on the surface mass balance of ice sheets and a number of studies (e.g. van de Berg et al., 2011; Bauer and Ganopolski, 2017) questioned the applicability of this simplistic scheme to the modelling of ice sheet response to orbital forcing.

In short, I do not believe that simulations of glacial inception can really constrain ECS, let alone TCR.

References (not cited in the manuscript by Bahadory et al.)

Batchelor, C.L., Margold, M., Krapp, M., Murton, D.K., Dalton, A.S., Gibbard, P.L., Stokes, C.R., Murton, J.B. and Manica, A., 2019. The configuration of Northern Hemisphere ice sheets through the Quaternary. Nature communications, 10, 1-10, 2019. 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, 2017. Ganopolski, A., Winkelmann, R., Schellnhuber, H.J. Critical insolation-CO2 relation for diagnosing past and future glacial inception. Nature, 529, 200-203, 2016. Heinemann, M., A. Timmermann, O. Elison Timm, F. Saito, and A. Abe-Ouchi. Deglacial ice sheet meltdown: orbital pacemaking and CO2 effects, Clim. Past, 10, 1567–1579, 2014. van de Berg, W. J., van den Broeke, M., Ettema, J., van Meijgaard, E., and Kaspar, F.: Significant contribition of insolation to Eemian melting of the Greenland ice sheet, Nat. Geosci., 4, 679–683, 2011.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-1, 2020.