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Prof. Marit-Solveig Seidenkrantz
Handling Editor
Climate of the Past

Dear Prof. Seidenkrantz and CP Editors,

Please find enclosed the author response comments for "Glacial Inception on Baffin Island: The Interaction of Ice Flow and Meteorology", which we would like to submit for publication in Climate of the Past. We found the two reviewers' comments most helpful, and plan to address them in a revised manuscript as described in our response below. In particular, our goal in the revised manuscript is to clarify our decision to use a regional climate model rather than a global climate model and to demonstrate the robustness of the anomalous circulation that is part of the negative feedback we identify. We thank the reviewers for their comments and guidance, and we feel that the revised manuscript will be considerably improved as a result.

Sincerely yours,
Leah Birch, Timothy Cronin
and Eli Tziperman

Reviewer 1

- This paper follows a long line of modeling studies on the last glacial inception ~115,000 years before present. Using climate models with the 115 ka Earth orbital configuration (or 116 ka in some studies), sometimes coupled with ice flow models, there is a longstanding problem of not being able to simulate rapid ice-cap grown over Baffin Island and subsequent ice expansion over that region during the ensuing several thousand years, as indicated by the bulk of geologic evidence. These new results continue in the same vein, and find very little ice growth compared to the consensus "observed" view.
  Notable features in this study are the use of a high-resolution regional climate model over Baffin Island (WRF, 20 km), asynchronous coupling with a dynamic ice cap model, elevation binning of surface mass balance, and negative feedback with anticyclonic flow warming air at the ice margins. The introduction contains a helpful and reasonably thorough review of the long line of previous modelling work, and an outline of the observational basis. The paper is well organized and clear throughout. However, I have several major concerns with the methodology, listed below
  Thank you for taking the time to read and for the thorough and constructive comments. In our revised manuscript, we will address your concerns and clarify the motivation for our model formulation and experiment design, in particular regarding the choices involved in the use of a regional climate model.
- Specific comments: (1) The RCM is forced at the lateral boundaries by ECMWF-reanalyzed meteorology for a modern year (1985-1986). External forcings related to 115 ka, i.e., Earth orbit and CO2 level, are only applied in the RCM. The RCMs 100-km outer domain, shown in Figs. 8-11, covers much of North America and Greenland and nearby oceans, but not the entire Arctic or northern Eurasia. Consequently it is missing some of the large-scale forcing on hemispheric and semi-hemispheric scales at 115 ka, including variations in low-order planetary waves, due to the GCM boundary influence.
  True, in an attempt to isolate the role of local processes over North America, we are neglecting circulation changes. This is consistent with Otieno et al. (2012) who used a coupled ocean atmosphere GCM, found a similar amount of cooling over Baffin Island during the summer (around 4C), and noted a lack of change in the Rossby waves. Wells (1983) also looked at Rossby waves during LGM compared to present day, and they found wave numbers of 4-5 are typical today, while at the height of the Laurentide a wave number is 3. This is not to say that there would definitely be no changes in planetary waves

at the beginning, but often in GCM simulations, only large ice sheets cause substantial deviations in the planetary waves. For instance, a large European ice sheet can cause cooling over the Laurentide (Beghin et al., 2014), but a large European Ice Sheet during inception may not be realistic (Bonelli et al., 2009; Ganopolski et al., 2010).

Yet, we agree that one expects changes to planetary waves due to the presence of ice sheets and topography changes. We hope to emphasize in our revised manuscript that, first, there is some debate regarding such changes, and second and more importantly, that our study neglects some feedbacks such as changes to the larger-scale circulation in order to focus on the role of those feedbacks that are included. We will clarify that our negative results may be due to these missing feedbacks, and that our study could therefore be complemented in the future by one in which the boundary conditions are modified accordingly, to test for feedbacks neglected here.

To emphasize our focus on local feedbacks, we will rename our manuscript to **"Role of Local Feedbacks in the Glacial Inception on Baffin Island: The Interaction of Ice Flow and Meteorology".**

- More importantly, ocean surface temperatures and sea ice are prescribed in the RCM from the GCM (I think), and so remain at their modern state; in reality they would be strongly affected by the 115 ka orbital perturbations and influence Baffin Island climate.

  This is an important comment, in the spirit of the previous one, and our response will be again divided into two parts. First, we will point out again in this context that our objective is to isolate local feedbacks and therefore we neglect some others. Second, we will explain that there is still debate on how much and how fast SSTs and sea ice would change during glacial inception. With present day fluxes on the boundaries we decided to keep SSTs at modern values for consistency, and investigate the "warm" interglacial temperature ocean scenario, which does have support in observations (Ruddiman et al., 1980). Cortijo et al. (1994) noted that the northern Atlantic remained close to modern day temperatures through 115 kya until glacial stage 5d around 110 kya. Vimeux et al. (1999) argued that observations support a warm ocean, similar to an interglacial, during inception, though their focus is on the souther hemisphere. Stokes (1955) and Gildor and Tziperman (2000, 2001) also argued that the oceans change slower than the atmosphere, so the oceans may need to remain warmer and ice free at the beginning of inception.

  We will supplement our discussion of this issue using additional references, including studies that show that colder oceans may amplify the inception process

(Khodri et al., 2001), as discussed in the more modern analogue of the Little Ice Age by Lehner et al. (2013). On the other hand, Otieno et al. (2012) found few changes in temperature in the Atlantic, and Meissner and Gerdes (2002) noted with their ocean model that North Atlantic remained warmer. Jochum et al. (2012) also argued that with CCSM4 ocean feedbacks are not necessary for glacial inception, but also did not rule out the role of ocean cooling.

- Also, within North America, the RCM physics contains no snow-masking albedo feedback due to vegetation ecotone shifts. All of these hemispheric-to-continental scale processes and feedbacks have been identified in previous modeling studies (see Introduction) as potentially significant players in cooling over Baffin Island and ice-cap initiation at 115 ka, but are muted or absent in the RCM simulations here.

  We agree that vegetation changes are a potentially important part of the glacial inception process. Related studies often refer to the treeline moving southward (Goñi et al., 2005; Calov et al., 2005), which occurs later in the inception process. Baffin Island vegetation in this model is notably missing large trees (Fréchette et al., 2006), thus vegetation feedbacks may be less important on Baffin Island at the very beginning of the inception process. In any case, we will clarify this issue of vegetation vs. treeline and the relevant literature in our revised manuscript.

- To remedy this, I suggest that a GCM should be used, not modern reanalysis, with the GCM physics including ocean dynamics and sea ice, and with the GCM orbit changed to 115 ka. Preferably both the GCM and RCM would have vegetation feedbacks. Some of this is discussed on pg. 19, but should be implemented in my opinion.

  We agree that using boundary conditions from a glacial simulation of a GCM is a great next step to capture additional feedbacks, like planetary waves and the down wind effect of the Eurasian Ice sheet changes on topography. Additional feedbacks could be discovered with such simulations. Yet we do feel that there is significant value in exploring the role of local feedbacks, especially once we clarify our objectives in a revised manuscript, and emphasize our goal by renaming the manuscript as described above.

- (2) The paper presents results from a "WRF control simulation", described on pg. 5, line 27 and shown in subsequent figure panels. It is not entirely clear from the text, but I think this is really the first step in the asynchronous sequence, and uses 115 ka orbit and reduced CO_2 (pg. 6, line 3). So all the differences from the second iteration in Figs. 4b, 5b, et seq. are due just to

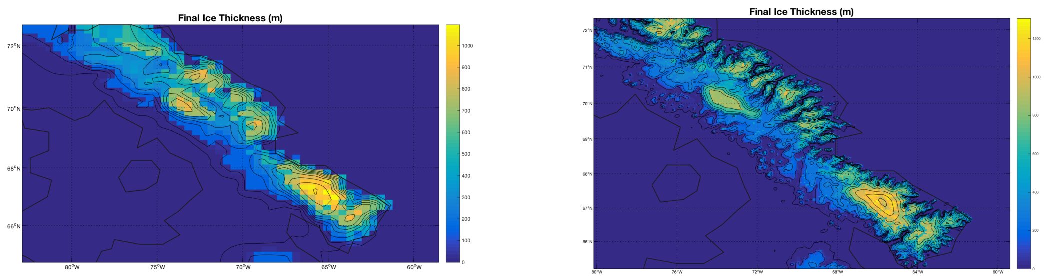
the initial ice cap growth in the first ice model integration. This "control" simulation is not a true modern simulation, with all-modern forcing (orbit and CO2). Such a run is described on pg. 5, lines 17-20, but not used again in the paper. I suggest adding figures showing a basic sensitivity test, comparing that run (a true "modern control" with modern orbit and CO2) with the first WRF iteration run (the "WRF control" here, with 115 ka orbit, reduced CO2, still modern ice cap). And each driven by separate GCM simulations of modern and 115 ka climates, respectively, as suggested in point # 1 above. First, the modern RCM run should be checked to agree roughly with modern observed summer air temperatures, precipitation and surface mass balance (SMB) especially over Baffin Island (as it does according to pg. 5, lines 17-20). Then an important figure should show differences in RCM summer air temperatures between the two runs, both for the whole outer domain (cf. Fig. 9b) and the inner domain (cf. Fig. 4b). The latter would immediately assess the viability of the whole scenario - i.e., qualitatively speaking, in order to produce major ice cap expansion, there needs to be at least a few degrees C of summertime cooling over the Baffin Island region, hopefully accompanied by some increase in annual snowfall. This basic cooling from truly modern conditions can then be contrasted later in the paper with the negative feedback presented here, where initial ice growth produces anticyclonic flow that warms the air around the ice margins.
Thank you for this comment. We realize now that our terminology may have been confusing and we will attempt to emphasize that our first 115kya simulation is Iteration 1.
As for an actual modern control simulation, we will make sure in our revision to refer the readers to our previous paper (Birch et al., 2017), which illustrates the differences in 115 kya and present day insolation. The resolution was higher, at 4km, but we found that using 20 km resolution did not significantly alter those results. The highest peaks of the Penny Ice Cap was colder with 4km, but at the high altitudes this does not differ the mass balance, as melting is already not occurring there. To address this comment, we will also include in the revised manuscript a plot of the temperature differences between model results using the present day insolation and 115 kya insolation in both the 100km outer domain and 20 km inner domains.

- (3) The use of just one modern year of ECMWF reanalysis does not adequately capture the mean (or interannual variability) of climate forcing. The choice of 1985-1986 as an extremely cold and wet year over Baffin Island bears an unknown relationship to the mean SMB forcing on century to millennial timescales that mainly determines ice growth. At a minimum, a GCM should be run for one (or two) decades, and the RCM run also through all those years, to give some idea of the mean SMB over Baffin Island. Choosing just one GCM year (or reanalysis, as here) can seriously skew the centuries-scale ice growth, due to the interannual variations of that single year.

It is true, of course, that a longer averaging of the forcing, as obtained from a climate model, would have advantages. However, given the persistent difficulties in simulating glacial inception using such GCMs, our goal here was to minimize the introduction of biases from global climate simulations, and stick to observations (reanalysis) and to local feedbacks as closely as possible. That has a price, as clearly pointed out by the reviewer, but we feel our approach is at least self-consistent and transparent. We hope to further clarify and elaborate on the motivation for this experiment design in our revision.

- (4) The resolution of the ice model (20 km, same as RCM), combined with the elevation binning of the SMB calculations, may not be sufficient to capture the true overall mass balance and dynamic advance of the ice cap margins. The paper appropriately references van den Berg et al. (2006), who dramatically show that the ice grid needs to be fine enough to resolve the steeply sloping ice-cap surface in the ablation zone, over which SMB varies rapidly due mainly to the atmospheric lapse rate, from ∼zero at the equilibrium line to strongly negative at the ice edge. If the grid only has a few boxes within this zone, and there are large changes in surface ice elevation between neighboring boxes, then subtle changes in climate and the area-integrated SMB may not be captured accurately if at all. The degradation of results depends also on the amplitude of climate forcing, and the method of downscaling SMB to the ice model grid, and has probably occurred to varying degrees in previous inception studies. van den Berg et al.'s test cases are ∼1000-km ice-sheet profiles, for which grid sizes of 5 km or less are needed for roughly accurate results (their Fig. 3). Here, the Baffin Island ice caps are much smaller, and the model's 20-km grid has only a few boxes within their narrow marginal ablation zones (see Fig. 1a, along SW-NE steepest-descent flow lines), which is probably not capturing true ice-cap advance. Judging from van den Berg et al.'s results, a much finer grid for the ice model should be used to ascertain the true behavior, on the order of a few to 1 km, at least until the initial ice caps grow much larger.

  We agree that a higher resolution of the ice model would be optimal and did run the ice model at a 4 km resolution as well (Birch, PhD thesis, 2017). We found the same results, as seen in Figure 1 below. In our revision, we will include a figure of these results and further discuss the issue.



(a) 20km Resolution

(b) 4km Resolution

Figure 1: Ice extent after 2nd Iteration for 20km and 4m Resolutions, using the mass balance from 20km WRF Simulation

- (5) Also, the elevation binning procedure may be contributing to the problem. Although not completely clear, I think the elevation binning (Fig. 1c) is done after each WRF integration, and the "bin line" (as in Fig. 3) is used to specify mass balance as a function of elevation for all points through the next ice model integration. However, the scatter in Fig. 1c shows that SMB is strongly influenced by factors other than elevation. In particular, SMB values around the edges of the ice cap, which are important in allowing or preventing ice advance, may be quite inaccurately represented by the procedure. An alternative method would be to save mean monthly air temperatures and precipitation from the previous RCM integration, and downscale them to the surface elevation of all ice model grid points (by lateral interpolation, and vertical lapse-rate correction), and perform a calculation for annual SMB at each ice grid point, still including refreezing in a simplified way. This could also be used for "hypothetical" ice locations with negative SMB adjacent to the current edge, which are not available directly from WRF (pg. 6, line 5), into which ice can potentially expand.

  Our binning procedure and the resulting SMB forcing recipe, while simple, are consistent with the way most ice sheet models are forced. More importantly, the results of this procedure, showing that ice elevation causes a negative warming feedback, should be robust regardless of how the ice elevation was calculated, as it is a result of the atmospheric model itself. We will further discuss and clarify these issues in our revision. We will also mention the related result that finer bin sizes did not increase the expansion of ice.

- Technical comments:
  pg. 4, line 23: Perhaps basal topography (B) should be listed as an input to the ice model, not surface elevation (H*) or ice thickness (H) which are outputs. Unless H is meant as an initial condition(?).
  H is the present day ice thickness the Ice Bridge Project, while the surface elevation (H^*) is specified as in put in WRF. The basal topography is thus $B = H^* - H$. All are needed as inputs or initial conditions in the ice model and we will make this clearer in the revised Methods Section.

- pg. 22, line 16: For the calculation of $T(z)$ in Appendix A, it is probably adequate to assume a linear conductive $T(z)$ profile from bed to surface, as done here. But it could be augmented using the analytic "Robin" solution that accounts for vertical ice advection given the local SMB (e.g. Cuffey and Patterson, 2010, pg. 217-218, referenced here). Once the basal ice temperatures are calculated, a check can be made that they are below freezing, and so are consistent with the assumption of zero sliding velocities in the ice model (pg. 4, line 8).
  Thank you for this idea. We believe the Robin solution involves the figure on page 411 and the associated equations. We can discuss this additional check in our revised manuscript appendix, but the temperature at the base calculated from this set of equations is still below freezing at $-7°$C.

Reviewer 2

- This is an interesting paper concerning initiation of glaciation on Baffin Island by combining a regional atmospheric simulation with a straightforward ice flow model using a slightly modified contemporary year favorable to inception. The need for high spatial resolution is emphasized.
  Thank you for the most helpful comments; we plan to follow up on your suggestion and make our argument for the anomalous anticyclonic-cyclonic circulation and the negative feedback stronger.
- My comments focus on the key aspect of this paper, namely the anticyclonic-cyclonic couplet that causes the warming that limits the ice growth. Is this real or an artifact of the WRF simulation? The reason I raise this question is that it is well known that regional models can develop anomalous circulations within their domains while matching conditions specified on the lateral boundary (e.g., Glisan et al., 2013: Effects of spectral nudging in WRF on Arctic temperature and precipitation simulations, J. Climate). Even if the couplet is not artificial a somewhat different orientation/intensity could lead to different advection conditions, decreasing or even eliminating the warm air advection.
  Thank you for reminding us of this paper, which we will further discuss in our revised manuscript. We are not employing spectral (internal) nudging, and restrict nudging to the domain boundaries. The goal of spectral nudging is to prevent departures from the GCM used for boundary conditions, while for our objectives, such deviations are of interest and are therefore not constrained. When running a present day simulation, we find that the circulation here is similar to that found in ERA-Interim. The simulation with 115 kya insolation also has circulation similar in magnitude and direction. The differences come in once topography changes are introduced. This indicates that the anticyclonic-cyclonic response is likely not an artifact, and we will further explore and discuss this in the revised manuscript.
- So: 1. When you simulated the present-day climate was there any evidence of the above couplet compared to ERA-interim?
  We did find that June was warmer by ∼1 degree in WRF than ERA-iterim for the present day simulation, but we do not believe it is a pattern inherent in WRF. The circulation in WRF and ERA-Interim are similar in direction and magnitude, and the 1st iteration simulation with 115 kya insolation does not cause the couplet to appear. Our simulations with and without ice-topography changes, robustly indicate that the anomaly appears only once the ice topography on Baffin Island changes. We will show the circulation patterns in present-

day simulations in order to address this issue in our revised manuscript.

- 2. Rather than the differences in Figure 8, what do the full 500-hPa height fields look like for iterations 2 and 10? 3. Can you develop more compelling arguments for the reality of your circulation results?
  The full 500 mbar height fields show the same flow pattern, and we will show these in a supplement added to our revised manuscript. We will also make the case more compellingly that these circulation anomalies do not show up unless the ice topography is changed. The anomalous response reveals that winds are not as strong from the north, which causes the warmer temperatures. We will also include further analysis of our simulations with topography or ice changes alone, particularly the geopotential height, instead of just the temperatures presented in our first submission.

- More generally, the lateral boundary conditions for your model could be very different than what you specified due to climate system feedbacks as a result of reduced summer insolation so nesting a regional simulation in a GCM simulation for inception time might be the best next step in your modeling.
  We agree that one expects changes to horizontal boundary conditions in a glacial world, due to planetary wave response to the developing ice sheets and other factors. We will emphasize in our revised manuscript that, first, there is some debate regarding such changes, and second and more importantly, that our study neglects some feedbacks such as changes to the larger-scale circulation in order to focus on the role of those feedbacks that are included. We will clarify that our negative results may be due to these missing feedbacks.
  To emphasize our focus on local feedbacks, we will rename our manuscript to **"Role of Local Feedbacks in the Glacial Inception on Baffin Island: The Interaction of Ice Flow and Meteorology".**

- Yet another rendition of the altered environment around 115k yr ago is Otieno et al. 2011: Atmospheric circulation anomalies due to 115k yr BP climate forcing are dominated by changes in the North Pacific Ocean. Clim. Dyn.
  Thank you for bringing the Otieno et al. (2012) paper to our attention. We have found it very useful, and we believe it emphasizes that circulation over the Atlantic may not have changed much by the time of inception. They found that the Pacific Ocean has the largest affect over the western part of North America, and causes substantial cooling there. The general circulation could be quite different, but the anti-cyclone is still there as noted by Herrington and Poulsen (2011) and Gregory et al. (2012). Otieno et al. (2012) also noted the formation of an anti-cyclone over Baffin Island.

References

Beghin, P., Charbit, S., Dumas, C., Kageyama, M., Roche, D., and Ritz, C.: Interdependence of the growth of the Northern Hemisphere ice sheets during the last glaciation: the role of atmospheric circulation, Climate of the Past, 10, 345–358, 2014.

Birch, L., Cronin, T., and Tziperman, E.: Glacial Inception on Baffin Island: The Role of Insolation, Meteorology, and Topography, Journal of Climate, 30, 4047–4064, 2017.

Bonelli, S., Charbit, S., Kageyama, M., Woillez, M.-N., Ramstein, G., Dumas, C., and Quiquet, A.: Investigating the evolution of major Northern Hemisphere ice sheets during the last glacial-interglacial cycle, Climate of the Past, 5, 329–345, 2009.

Calov, R., Ganopolski, A., Petoukhov, V., Claussen, M., Brovkin, V., and Kubatzki, C.: Transient simulation of the last glacial inception. Part II: sensitivity and feedback analysis, Climate Dynamics, 24, 563–576, 2005.

Cortijo, E., Duplessy, J., Labeyrie, L., Leclaire, H., Duprat, J., and Van Wearing, T.: Eemian cooling in the Norwegian Sea and North Atlantic ocean preceding continental ice-sheet growth, Nature, 372, 446, 1994.

Fréchette, B., Wolfe, A. P., Miller, G. H., Richard, P. J., and de Vernal, A.: Vegetation and climate of the last interglacial on Baffin Island, Arctic Canada, Palaeogeography, Palaeoclimatology, Palaeoecology, 236, 91–106, 2006.

Ganopolski, A., Calov, R., and Claussen, M.: Simulation of the last glacial cycle with a coupled climate ice-sheet model of intermediate complexity, Climate of the Past, 6, 229–244, 2010.

Gildor, H. and Tziperman, E.: Sea ice as the glacial cycles climate switch: Role of seasonal and orbital forcing, Paleoceanography, 15, 605–615, 2000.

Gildor, H. and Tziperman, E.: A sea ice climate switch mechanism for the 100-kyr glacial cycles, Journal of Geophysical Research: Oceans, 106, 9117–9133, 2001.

Goñi, M. S., Loutre, M., Crucifix, M., Peyron, O., Santos, L., Duprat, J., Malaizé, B., Turon, J.-L., and Peypouquet, J.-P.: Increasing vegetation and climate gradient in Western Europe over the Last Glacial Inception (122–110 ka): data-model comparison, Earth and Planetary Science Letters, 231, 111–130, 2005.

Gregory, J., Browne, O., Payne, A., Ridley, J., and Rutt, I.: Modelling large-scale ice-sheet-climate interactions following glacial inception, Climate of the Past, 8, 1565, 2012.

Herrington, A. R. and Poulsen, C. J.: Terminating the Last Interglacial: The role of ice sheet–climate feedbacks in a GCM asynchronously coupled to an ice sheet model, Journal of Climate, 25, 1871–1882, 2011.

Jochum, M., Jahn, A., Peacock, S., Bailey, D. A., Fasullo, J. T., Kay, J., Levis, S., and Otto-Bliesner, B.: True to Milankovitch: glacial inception in the new Community Climate System Model, Journal of Climate, 25, 2226–2239, 2012.

Khodri, M., Leclainche, Y., Ramstein, G., Braconnot, P., Marti, O., and Cortijo, E.: Simulating the amplification of orbital forcing by ocean feedbacks in the last glaciation, Nature, 410, 570–574, 2001.

Lehner, F., Born, A., Raible, C. C., and Stocker, T. F.: Amplified inception of European Little Ice Age by sea ice–ocean–atmosphere feedbacks, Journal of climate, 26, 7586–7602, 2013.

Meissner, K. and Gerdes, R.: Coupled climate modelling of ocean circulation changes during ice age inception, Climate dynamics, 18, 455–473, 2002.

Otieno, F. O., Bromwich, D. H., and Oglesby, R.: Atmospheric circulation anomalies due to 115 kyr BP climate forcing dominated by changes in the North Pacific Ocean, Climate dynamics, 38, 815–835, 2012.

Ruddiman, W., McIntyre, A., Niebler-Hunt, V., and Durazzi, J.: Oceanic evidence for the mechanism of rapid northern hemisphere glaciation, Quaternary Research, 13, 33–64, 1980.

Stokes, W. L.: Another look at the ice age, Science, 122, 815–821, 1955.

Vimeux, F., Masson, V., Jouzel, J., Stievenard, M., and Petit, J.: Glacial–interglacial changes in ocean surface conditions in the Southern Hemisphere, Nature, 398, 410, 1999.

Wells, G. L.: Late-glacial circulation over central North America revealed by aeolian features, in: Variations in the global water budget, pp. 317–330, Springer, 1983.