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Climate of the Past
Discussions

Interactive comment on "Glacial Inception on Baffin Island: The Interaction of Ice Flow and Meteorology" *by* Leah Birch et al.

Anonymous Referee #1

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General comments:

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 long-standing 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.

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 RCM's 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. 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. 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.

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.

(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 CO2 (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.

(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.

(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 $\sim$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 $\sim$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.

(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.

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(?).

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).

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