

Interactive comment on “Arctic warming induced by the Laurentide ice sheet topography” by Johan Liakka and Marcus Lofverstrom

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Received and published: 30 April 2018

This manuscript presents a new assessment of the influence of Laurentide ice sheet (LIS) height on atmospheric circulation at the last glacial maximum. The experiment design is simple and thus the results are clear. The study involves adjusting the LIS height as a boundary condition in a medium-high resolution atmospheric model coupled to a slab ocean. Each sensitivity simulation is forced by LGM boundary conditions (orbital parameters, greenhouse gases, etc.), with LIS height scaled in 6 separate experiments from a scaling factor of 0 (LIS albedo effect only) to 1.25 (25% taller than LIS reconstructions). This experiment shows that a taller LIS can drive changes in atmospheric circulation that drive widespread warming in the North Hemisphere Arctic, which could serve to be a self-limiting influence on LIS height, through surface mass

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balance effects. This work is similar to a variety of earlier publications that perturb LIS height (appropriately referenced by the authors). However, this study is novel in its presentation of a method to disentangle the contributions of meridional energy flux and flux convergence due to mean circulation, stationary eddies, and transient eddies. This separation of atmospheric processes allows the authors to demonstrate that the LIS-height-driven surface warming is dominated by the energy flux from stationary wave eddies, which is mostly compensated by transient eddies. This mechanistic description of the various flux and feedback contributions is particularly useful in understanding how large ice sheets influence climate (as opposed to the other way around). The manuscript is well-written and provides a clear description of the mechanisms controlling atmospheric circulation change due to LIS height. The authors also provide a clear explanation of the study's limitations (particularly the lack of a fully dynamic ocean). I appreciate that the authors have included results from a simple surface mass balance model in their discussion of implications for LIS-height-driven temperature change on the mass balance of the LIS itself. I support this manuscript for publication, but I have a few minor comments that should be considered that may help provide some necessary clarification.

Line 11 (in abstract): "These results suggest a positive feedback between continental-scale ice sheets and the Arctic temperatures that may help constrain LIS elevation. . . ." Why is this a "positive" feedback? I tend to consider positive feedbacks to be amplifying feedbacks. But the mass-balance feedback described in this paper counteracts (or "constrains") the initial change. LIS grows → warmer Arctic temps → reduced LIS surface mass balance → LIS shrinks. Isn't this a NEGATIVE feedback? Please consider changing throughout.

Line 30 → model simulations are 60 years in length; 35y for spinup, 25y for analysis. Is this enough? For the spin-up, can the authors demonstrate with some key atmospheric variables that the simulation is no longer demonstrating drift? Similarly, does 25 years provide enough time to appropriately assess a climatology?

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Line 12-17 → The surface mass balance model used in this study is a simple PDD approach. A PDD factor based on observations from modern Greenland might not be completely relevant for the LIS (see Pollard et al., 2000, Global and Planetary Change). It may be worth noting this limitation: that a fully-resolved energy balance model would provide a more complete assessment of surface mass balance. However, Pollard et al. (2000) showed that for paleo applications, conclusions of a PDD approach are generally consistent with an energy balance model. This is to say that I think the general trend of surface mass balance change due to LIS elevation (Fig. 4) is likely robust. However, the observation of positive surface mass balance over Siberia, except in the LIStopo1.25 simulation, might be sensitive to the selection of the PDD factor in the surface mass balance model. Further sensitivity analysis of the PDD factor used in these simulations may be necessary.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-26>, 2018.

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