Review of Cuzzone et al., TCD

In this paper Cuzzone et al. present a series of simulations of the Nuup Kangerlua (Godthåbsfjord) region, driven primarily by novel palaeoclimate simulations of temperature and precipitation. Within these, they test high/low temperature/precipitation scenarios and scenarios for each where a level-set calving criterion is turned on/off. Their results show surface mass balance to exert a strong control on rapid retreat (consistent with geological evidence), while calving primarily exerts a strong control on ice dynamics (and therefore the evolution of total domain volume that is arguably more important). While their simulations are not able to recreate the contemporary ice margin (I would have been astounded if they had given the range of potentially confounding factors), the simulations presented the authors provide a very informative exploration of the sensitivity of this system to different climate forcing scenarios. The findings of this paper have implications for others aiming to simulate ice sheet evolution in topographically complex regions, both in terms of palaeo-simulations and contemporary scenarios/projecting future change, though the latter could come through more clearly especially in the conclusion.

On a personal note I thoroughly enjoyed reading this paper, and I have very few substantive comments to make on the science. Having done a lot of fieldwork in this region it's great to see this work done, as I've often wondered how you would even attempt to go about effectively modelling the entire fjord system over these timescales given challenges of resolution, computational cost, boundary conditions, and model physics. In this paper Cuzzone et al. deal with each of these issues in the most robust way that is currently feasible, and are able to provide valuable insights into the controls on Holocene deglaciation of this region.

My only substantive comment on the manuscript is primarily stylistic, in that parts of the results section occasionally stray into discussion (e.g. L322-8; L334-6 and others), while L396-405 does not really fit in this section. What is there is important, and should not be removed from the paper, so I would ask that the authors go through this section and pull out any interpretation of results and reallocate them to the discussion.

As alluded to previously, I think the authors currently undersell the relevance of this work for those working on the deglaciation of other topographically complex regions (in palaeo/contemporary/future contexts), and it would be nice to see this come through a bit more clearly in the introduction, relevant parts of the discussion and conclusion especially.

James Lea

Minor points

L26 – current best practice on place names is to provide Greenlandic name, followed by Danish colonial name in brackets e.g. Nuup Kangerlua (Godthåbsfjord)

L40 – if implementation and resolution of calving is important for robust past simulations, will it not also be important for contemporary simulations/future decadal to centennial projections too?

L59 – interglacial rather than interglaciation?

L225 – 8.3 mm/deg C/day?

L248-9 – why is 40 m/yr chosen?

Section 3.4 – a sentence on how the model domain in the ice sheet interior was defined would be useful. Obviously any upstream impacts on flux will be partially mitigated by the domain boundary condition, though given the model is forced in large part by T and precipitation there are potential downstream impacts on having the domain defined as is (given that the contemporary upstream catchment of KNS extends further to the south). To be clear, I do *not* think this undermines any of the results in the paper – defining palaeo-catchments for ice sheet outlets is tricky to impossible ab initio. However a sentence or two on why this is not a huge issue for the results would be useful for the reader.

Section 3.4 – is GIA accounted for? I know this region is pretty complicated in terms of it's GIA response, though a sentence on the expected range of bedrock change and how this may/may not impact results (particularly impacts on calving) would be worth flagging here.

L264 – missing bracket

L402-405 – this is perhaps a misunderstanding on my part, but I do not think that the assertion that other published simulations that show retreat inboard of the present day ice margin are likely too extreme (L402-405) is fully substantiated by results presented, *unless* the authors are referring to the scale of retreat relative to the present ice margin. There is evidence for terrestrial portions of the ice sheet being inland of the current ice margin along the SW coast during the Holocene thermal maximum (e.g. Larsen et al., 2015 [https://doi.org/10.1130/G36476.1]; and referred to by the authors L429-426), and while I fully acknowledge the differences between terrestrial and marine terminating margins, I still think the assertion made (as written) goes too far.

Figure 11 – can you change the colour map here to something other than red/green as it's a bit challenging for a colour blind person to see!

L633 – fjord rather than ford

L658-675 – The majority of this paragraph reads as a bit of a list currently, and think it would be good to expand a little (half a sentence or so) on each point on their wider implications (both for KNS region and generally across Greenland). This would bring the paper into line with sentences mentioned in the abstract (e.g. L37-40)