The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-116-RC2, 2018

Interactive comment on "Marine Ice Sheet Instability and Ice Shelf Buttressing Influenced Deglaciation of the Minch Ice Stream, Northwest Scotland" *by* Niall Gandy et al.

Anonymous Referee #2

Received and published: 7 September 2018

This manuscript describes a series of simulations of the deglaciation of the Minch Ice Stream (MnIS) region within the British-Irish Ice Sheet. Using BISCICLES, the authors determine that a region of reverse-sloping bed causes rapid deglaciation of the MnIS through the marine ice sheet instability. They also determine that MnIS begins in an unconfined state during the LGM wherein the development of an ice shelf has little influence on the ice stream, but then transitions to a confined state as the ice stream retreats into a region of pinning.

Overall, the paper is well written, besides a few awkward and confusing explanations (pointed out below). The simulations that are conducted are clever and effectively show that the marine ice sheet instability and (maybe) buttressing may play a role in deglaciation of MnIS. I think the most general issues that need to be addressed in this study are: (1) To what extent are these simulations supposed to represent actual deglaciation vs. numerical experiments on the role of some ice dynamical feedbacks in this region? (2) Can you be sure that some of the other feedbacks which have been omitted purposefully (to focus on MISI/buttressing alone) do not play a role in modifying the importance of MISI/buttressing? (3) How important is buttressing, actually?

I think that what is needed to address these issues is either: (a) providing a stronger argument why these processes can be excluded and the conclusions of the study remain intact, (b) additional simulations which explore the role of these other feedbacks and buttressing more carefully. I think that is these issues are addressed (in addition to the more minor issues listed below), this paper would be a valuable contribution to understanding the deglaciation of the British-Irish Ice Sheet and ice sheet dynamics more broadly.

Major points: 1. I think it needs to be made clear that the uncertainties associated with the climate and glaciology of the MnIS during deglaciations are large enough that the simulations presented here are not necessarily representative of the actual time-dependent deglaciation, but rather investigate mechanisms that may have played a role during a generic deglaciation of MnIS. That is, you make the following assumptions in the RETREAT simulation (that I suppose is the most like actual deglaciation): climate forcing was step-like, there was an ice shelf (i.e. calving and basal melt were low enough to allow for ice shelf formation), basal friction was fixed in time, and SMB does not evolve with changing elevation. You say that you make these simplifications so that you can isolate internal instabilities. This point is important to make more clearly and upfront as the purpose of this study, since making these simplifications takes you away somewhat from reality.

2. You have wisely decided to perform some of your numerical simulations while omitting certain feedbacks, in order to test whether MISI and buttressing may occur given

certain bed topography and climate forcing. This does not answer the question of whether MISI/buttressing are the most important feedbacks, or whether they might be significantly amplified or reduced by cooperating feedbacks. In particular: rebound of the bed and the elevation-SMB feedback may also play an important role in the deglaciation of ice streams. Gomez et al (Nature Geo, 2010) have shown that local sea level changes may have an influence on grounding line stability during deglaciation. Robel & Tziperman (JGR, 2016) have shown that curvature of the elevation-SMB feedback may cause acceleration of ice stream during the initial stages of deglaciation. It may be worth addressing these issues by making an argument for why these other feedbacks are not important, and also potentially performing additional simulations that test the influence of these feedbacks to some extent.

3. Page 4, Line 22 (and page 5, line 12 and page 10, line 7-9): The elevation-SMB interaction may be considered to be an “internal instability” considering that it mostly has to do with the climate at the ice sheet surface. Another way to think about this, why do we only care about “internal” instabilities? Are these the only instabilities that are important? To what extent can we disentangle feedbacks having to do with the ice sheet from those having to do with the ice sheet surface climate?

4. Page 5, Line 26: I can see that “the purpose of this study is to test for a retreat instability of the MnIS with a given topography” However, why is this the correct purpose for the study to have? What does this tell us about the actual deglaciation or about the process of MISI? I think you need to go a bit further than stating that the purpose is to do some clever simulations to the purpose is to answer some particular science question.

5. How important is buttressing, actually? (Page 7, Line 18-20) You’ve said that the influence of the ice shelf on the grounding line position is very small, so how much influence does the ice shelf actually have on grounding line stability? If you include floating ice in your calculation of ice volume, then it is not clear to me how much of your 10% volume difference is just the ice shelf itself. I think you need to be a little more

careful in order to make a strong argument that there is any buttressing actually happening here. Especially since this is a much wider ice stream than what you typically find in Greenland (see argument p.7, line 32), it is not obvious that the buttressing will be significant.

Minor Points: page 1 Line 11: "A valuable case to examine these processes is - awkward phrasing Line 13: what is well constrained? the ice stream or a measurement about the ice stream? Line 15: continental shelf Line 16: sub-ice shelf melt Line 21: We conclude that geological data. . .the future of contemporary ice sheets. Line 29: Consequently, any change in ice thickness at the grounding line can cause an irreversible grounding line migration with no change in external forcing. [The point is that the term "instability in grounding line migration" is unclear.]

page 2 Line 1: which can stabilize Line 3: Higher-order models have more success in accurately simulation the grounding line. . . Line 5: Cite Tsai et al. 2015, JGlac Line 8: episodic retreat Line 8-9: how is retreat controlled by retreat history? Line 12: proxy observations (no hyphen)

page 3 Line 17: with L1L2 physics retained from the full Stokes flow equations (Schoof and Hindmarsh 2010). Line 18: BISCICLES uses adaptive. . . Line 26: Is there reasons to think that a linear Weertman exponent represents ice stream dynamics effectively? Is there a good citation? Line 28-30: This would be a good place to explain first that you pick these values specifically in order to produce large ice shelves. Otherwise, it just seems like you just replace calving with a constant parameter (not much of a "calving model").

page 4 Line 2: We set the simulations initial conditions to the ice sheet state when the MnIS was at its. . . Line 5: The 27 ka BP margin. . .of the BIIS, which matches well with the reconstructed. . . [In general, you don't need to use the phase "for the purpose of this study", which doesn't convey any information] Line 14: Do the climate model simulations used to force your model take into account ice sheet topography?

If so, say here. If not, you need to justify why this is reasonable. Lines 23-28: If you are eventually just going to strongly adjust the sub-shelf basal melt rate to retain ice shelves, why introduce the linear parameterization in the first place? Why not just simplify this whole part by setting some arbitrary sub-shelf melt rate that either permits or removes ice shelves?

page 5 Line 1: in what sense do you mean magnitude here? Line 13: I'm a bit confused…you initialize with a plastic thickness approximation, but experiments are then begun with a stable ILGM volume. Does the SPINUP run get you from the plastic thickness initial condition to the stable ILGM configuration? The initialization procedure should be explained a bit more clearly. Line 17: How were the magnitudes of the climate perturbations chosen? Are they backed up by modeling evidence for the deglacial change in climate here?

page 6 Line 14-15: Why would you expect the rate of volume and area change to be constant throughout the simulation? If anything, the initial phase of retreat looks something like exponential decay, which is approximately what one would expect from a fairly simple linear response model (i.e. dV/dt = -a*V). Classical work on the ice sheet response time scale under forcing (Nye 1960, 1963, 1965; Jóhannesson et al., Âă1989; Harrison et al., 2003) finds such an exponential decay response.âĂÍLine 27: As the thinning continues, the ice area begins to retreat…

page 7 Line 2: period after hindmarsh citation. Line 3: Ice stream acceleration in response to the sudden collapse… Line 14: how much of the 10% difference in ice volume in the NOSHELF simulation is accounted for by the volume of the ice shelf itself (assuming this isn't volume above flotation, and if so, you should indicate that). Line 20: If you turned the calving rate way down and got a much larger ice shelf, could you stabilize on the retrograde slope?

page 8 Line 26-31: These lines include a lot of redundant information. Could be cleaned up. Line 32: marine or shear margin?

Printer-friendly version

Discussion paper

page 10 Line 19-20: You start talking about GZW6 suddenly here. Bears more explanation and/or pointing to figures. Line 34: It is also the case that a change in ice stream velocity might cause a significant change in ice sheet volume (i.e. through acceleration-induced thinning), but not much change in area. This has a lot to do with local bed topography.

page 11 Line 4-6: As mentioned above, it may be the case that triggering of ice stream acceleration by appropriately-structured climate forcing may cause a larger retreat (see Robel & Tziperman 2016).

page 12 Line 1: provide citations for why hydrology may or may not be important

Figure 3, Panel b: colormap could use more constant, very difficult to see any difference Figures 3, 4, 5, 6, 7: The axis labels and tick labels are far too small and illegible. Also the lines in all plots could have greater thickness to increase legibility. It also looks like the text is grainy because the resolution of the figures is low. Please include higher resolution figures.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-116, 2018.