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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.

J. Seguinot (Referee)

seguinot@vaw.baug.ethz.ch

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N. Gandy et al. present an application of the higher-order ice sheet model BISICLES to early deglaciation dynamics of a marine-based sector of the British-Irish Ice Sheet in northwest Scotland. The Minch, a submarine trough located between mainland Scotland and the Isle of Lewis, is presented as one of the best documented marine sectors of the former ice sheet, much suited for this exercise.

The numerical experiment is divided in three stages. First, a "spin-up" run is initiated upon a previously published perfect-plastic ice geometry and brings the model to a steady state. Second, "retreat" simulations triggered by an instant increase in air tem-

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perature and sub-shelf melt are used to analyse deglaciation patterns and sensitivity to ice-shelf buttressing. Third, “readvance” simulations started at different stages of deglaciation with instant return to “spin-up” conditions are used to investigate the reversibility of marine ice “retreat”. The simulations evidence the potential for ice retreat in two stages separated by a phase of thinning but near stagnation of the ice margin, and a point-of-no-return after which deglaciation becomes irreversible.

Strongly simplifying assumptions are made (and acknowledged), but the novelty of this study lies in its regional and marine focus. In fact, the authors should be credited for making one of very few (if any other) attempts to date to use the rich, newly available submarine glacial geomorphologic record to validate marine ice sheet modelling, a topic full of physical uncertainties and numerical difficulties. The manuscript is very well written and clearly illustrated.

I strongly support publication of these results, but I would encourage the authors to increase the transparency of their methods and clear a few inconsistencies in the interpretation before publication.

1 General comments

Code availability and reproducibility

As of now, the description of the methods lacks important detail and parameter values (see also specific comments below), which hinders the reproducibility and the traceability of the model results. Since a detailed sensitivity is not (and probably need not) be included, this means that readers have to trust the authors for having made reasonable model choices and used error-proof tools. This is not reasonable given the multiple uncertainties that affect ice sheet modelling.

The statement that “Cornford et al. (2013) provides a full description of BISICLES”

(p. 3, l. 20–21) is not entirely correct as that paper primarily describes the numerical treatment of ice flow and grounding line migration but not boundary model components, and the code has presumably evolved since 2013. Ice sheet models such as BISICLES are complex programs containing numerous uncertain ice physical parameters, multiple numerical approximations and configurable regularizations schemes. Most importantly, they are not exempt from coding errors.

I am certainly not advocating to clutter the article with a full model description here, which would be both disruptive and inefficient. Instead, I think that model code should be made available and the version used clearly stated in the manuscript. The required section on "code and data availability" is missing. I also suggest that authors include a referenced list of the most important ice-physical parameters used in their set-up.

Supplementary material

Related to the previous point, parts of the methods, and results from two sensitivity tests are given in the supplementary. I don't really understand the authors' choice to store this important information in a supplementary file (using a proprietary format with no guarantee of long-term readability). I found it a bit difficult to follow the main text without looking at supplementary figures.

I suggest that Table S1, Figs. S1 and S2, and the description of the basal friction map (with added values and for the friction coefficient, and possibly a reference to the basal sliding equation) are incorporated in the main text. Perhaps Fig. S1 could be merged with Fig. 3, and Fig. S2 with Fig. 4. References are already in place where needed. Fig. S3 does not add much to Fig. 7 and could probably be omitted.

Fast retreat and irreversible retreat

Some of the sentences in the abstract and conclusions (see specific comments) appear to amalgamate rapid retreat and irreversibility. However, it is worth noticing that the area of slow retreat at the straight's mouth (best visible in the supplementary animation) is distinct from the point of irreversibility in shallow waters further upstream. This interesting result indicates a different role of lateral and vertical confinement on marginal retreat. I find this result particularly interesting in the context of the variety of currently observed patterns of tidewater glacier retreat in Greenland and Antarctica, and believe that it should be emphasized in the abstract and conclusions.

2 Specific comments

p. 1, l. 17: the ice stream becomes laterally confined at a "pinning-point"

The phrase "pinning-point" is often used to described ice rises and other contact points stabilizing an ice shelve, which may be confusing. Inline with my general comment above, I suggest replacing with a more descriptive term, e.g. the "straight's mouth" or "the end of the trough".

p. 1, l. 18: the presence of ice shelves became a major control on deglaciation

This statement does not really reflect the model results and argumentations in the main text (also see other comment below). Actually I was surprised to find how little effect ice shelves have on the pace of deglaciation, for which authors provide a very satisfactory explanation. I think this sentence should be reworked.

p. 2, l. 1–3: The previous generation of ice sheet models do not accurately simulate the position of the grounding line, due to the use of the Shallow Ice Approximation (van der Veen, 2013).

It would make sense to mention possible subgrid parametrization of shallow approximations here (Feldmann et al., 2014).

p. 3, l. 21–22: we set up the model domain to cover the majority of the British-Irish Ice Sheet

No mention is made of model results outside of the Minch domain. Does the model performs reasonably well there too?

p. 4, l. 3–4: we initialise ice thickness in the domain from a perfectly plastic ice sheet model

How were other variables, especially ice temperature, initialized?

p. 4, l. 5–6: the 27ka BP margin of Clark et al. (2012) was used for the remainder of the BIIS.

Was the 27 ka BP margin used as a boundary for the plastic model? If so, in which sense is it fortunate that the plastic ice sheet matches it well? If not, how was ice extent converted to ice thickness?

p. 4, l. 8–10: Positive Degree Day (PDD) mass balance model, as described by Seguinot (2013) and Gregoire et al. (2016, 2015).

None of the given references contain a full PDD model description. Actually Gregoire et al. (2016) do not even mention PDD models. Does the model resolves the sub-annual evolution of snow cover? Does it account for meltwater refreezing? Most importantly, which PDD factors are used? Does the reference to (Seguinot, 2013) imply that the authors incorporate daily temperature variability from HadCAM3, or perhaps use the code I wrote for this publication (https://github.com/juseg/pypdd)? Or is the PDD model part of BISICLES? I think a new paragraph is needed here to address these questions.

p. 4, l. 18: To remove the effect of a SMB feedback

This is an important simplification. Although it may be justifiable under Antarctic-like settings, in a warmer climate the surface elevation-mass balance feedback could strongly affect the pace of margin retreat and hysteresis effects discussed further in the manuscript. In this context, introducing a few details about input climate would help. I wonder which parts of the model domain are affected by seasonal melt, and to which extent dynamic thinning due to ice surface lowering would affect the the aforementioned conclusions.

p. 4, l. 23: sub-shelf melt (m/y) is 10$\times$ the annual average SST (K)

Melt rate and temperature are distinct physical quantities. For mathematical rigour I suggest to rework the sentence and introduce an equation and a unit to the factor 10.

p. 4, l. 27–28: We correct the SST to permit for ice shelf formation

Presumably both modelled SST and the aforementioned factor 10 suffer from uncertainties. However, what is the justification for applying a correction on input data rather than a model parameter? Does this correction replaces or supplement the model bias correction mentioned earlier in the manuscript?

p. 4, l. 32: studies of present day ice streams using the same friction law

On which modern ice streams were these studies conducted?

p. 5, l. 2–3: the extent of ice after a 6,000 year spin-up remains comparable even with different magnitudes of basal friction.

Is this because the bedrock topography drops abruptly at the continental shelf edge? Thus changes in basal friction affect ice thickness but not its extent?

Besides, Fig. S1 shows a significant drop in ice volume (and presumably ice thickness) for all runs. Does this decrease concerns the entire BIIS domain, or only the marine-influenced sector of the Minch?

p. 5, l. 6: Isostatic adjustment was simulated using the EUST3 GIA model

I think that a short model description and list of parameters used is needed here. Is the GIA model ran at equilibrium or transiently? Is the value of mantle viscosity below the Minch well constrained?

p. 5, l. 14: Experiment set-up is summarised in the supplement (Table S1).

I think this table should be part of the main text, and supplemented by a second table listing other, constant model parameters (see general comment above).

p. 5, l. 24: We keep bed topography and sea level constant

Similarly to the assumption on surface elevation-mass balance feedback, I think that this second assumption needs to be put in context of the regional bedrock properties. Is the bedrock response expected to be instantaneous or delayed? What is the expected rate of relative sea-level drop and how could this affect the model results?

Fig. 4 y-label: Ice volume $(10^4\ \mathrm{km}^2)$

Dividing volume by area yields an ice thickness of ca. 20 km. There must be an order-of-magnitude error here.

p. 6, l. 17: halving the domain's ice volume in the first 2,000 model years

This contradicts Fig. 4 where ice volume appears to drop from ca. 83 to ca. 62 axes units, corresponding to only around a quarter of the initial ice thickness.

p. 7, l. 21: the volume change caused by removing an ice shelf is significant.

In the context of earlier simplifications on surface mass balance and bedrock topography, I am not convinced that a 10% change of volume is significant. I suspect that a few sensitivity tests on input climate, basal sliding parameters and uncertain bedrock properties would yield much larger changes in ice volumes (cf. e.g., Seguinot et al., 2016). I would simply remove this statement, and correct the corresponding sentence in the abstract (see previous comment).

p. 8, l. 15–16: the ice stream does not recover to lLGM extent [...] (Figure 7a,b)

I suggest to refer to Fig. 7 here.

p. 8, l. 16–17: a volume 25% smaller, and an area 50% smaller [...] state (Figure 7d).

I think Fig. 7a and b would be more appropriate.

p. 8, l. 18–19: a full shelf-edge glaciation

One could refer to Fig. 7c here...

p. 8, l. 19: a small Hebrides Ice Cap with glaciation in the Minch limited to the east trough (Figure 7)

And Fig. 7d here.

p. 8, l. 21–23: Hysteresis of ice sheet evolution is evidence for an instability during the advance of retreat of an ice sheet (Schoof, 2007). Ice sheets can experience a variety of instabilities (Calov et al., 2002; Gregoire et al., 2016, 2012; Schoof, 2007) which could influence the ice sheet evolution.

I do not really understand how these two sentences relate to the surrounding discussion. Without formal analysis of all intermediate stable states it would be more correct to write that the model results hint at an hysteresis (formally identified by Schoof, 2007). Studies by (Gregoire et al., 2012, 2016) concern a surface elevation-mass balance ice-sheet instability which is not only unrelated but precisely missing in the current study, so I would just remove the second sentence, or move it to a discussion of potential weaknesses.

p. 9, l. 5–6: east of the trough [...] west of the trough

If I am not mistaken, on Fig. 1 these are labelled “east trough” and “west trough”.

p. 9, l. 1–3: the east trough contains a small ice stream whilst the west trough has fully deglaciated and formed a calving bay.

A reference to geological observations is needed here.

p. 9, l. 32–33: Due to the idealised climate forcing, only the pattern, and not the timing, of the retreat can be compared to empirical reconstructions.

After the Mynch has been announced as well-documented by geology, and since the main model results concerns the (at least relative) timing of deglaciation, this sentence comes very disenchanting! I think it could be reworked to something more positive.

p. 10, l. 9–10: Here, we define the reconstructed retreat [...] as the “observed retreat”.

I think this definition is oversimplifying and potentially misleading. The discussion could become more constructive if it instead made clear what are geomorphological evidence, what are sedimentological evidence, and what are geological reconstructions containing a part of interpretation.

p. 10, l. 19: GZW6

I assume this means "Grounding zone wedge 6". Could this be added somewhere on a map?

p. 11, l. 3: However, the observed retreat began at 30ka BP, when the NGRIP δ18O record suggests a cooling climate globally

The connection between numerous arguments against interpreting Greenland δ18O records as proxies for global climate.

p. 11, l. 8–10: There are two likely possibilities that could explain the MnIS retreating in a cooling climate (explained below); i) internal mechanisms caused by the expansion of the rest of the BIIS, or ii) a local SMB change of this sector of the ice sheet at 30ka BP.

I think that a third possibility should be considered here. The assumption of initial steady-state during maximum extent does not necessarily hold true. Although the authors discuss ice piracy from neighbouring ice sheet sectors, the possibility that the maximum extent of Minch ice stream is itself the result of a thermodynamic destabilisation (a surge) following build-up of stiff colder ice during the advance phase exists. This explanation should probably be included in the list.

p. 12, l. 13: This result is evidence for a rapid retreat of the MnIS caused by marine ice sheet instability.

This sentence confuses rapid retreat with instability (see general comment above).

Finally, I would like to congratulate the authors again for their effort to bring palaeoglaciologic data-model comparison at sea, and hope they will find my comments useful in bringing their manuscript to final form.

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