Towards modelling of corrugation ridges at ice-sheet grounding lines

Hogan et al.

This is a well-constructed manuscript presenting an elegant model–data comparison study into the formation of relatively newly-discovered, delicate grounding line landforms, which have been interpreted as forming daily due to tidal influences on the grounding zone. Verification that tidally modulated grounding line processes – and better understanding of specifically which processes – can produce discrete grounding line landforms is a significant advance that will allow us to better use the palaeo-record to reconstruct past grounding line retreat rates and dynamics and, critically, place bounds on what can be expected in terms of retreat rates from modern ice sheet grounding lines. Here, the different models and their results are well-explained, presented and evaluated; the interpretations and conclusions reached seem sound. (Note that I'm not in a position to comment on the physical formulation of the models.) The limitations of the models (conceptually in set up and in the results they produce) are thoroughly discussed, and assumptions are clearly stated, ensuring that what this work does and does not yet achieve are well-framed. I have a few rather minor comments.

Main comments

1) You state as a given that corrugation ridges are produced at grounding lines. There is good reason for adopting this interpretation for Thwaites and Larsen – occurrence on wedge surfaces, draping MSGLs, displaying lateral continuity – and therefore the approach and research framework here is justified and important. However, other examples have been reported (including by some of the authors) where corrugations are inferred to form in other ways/settings (e.g. not on wedges, and within curvilinear scours attributed to ice shelf or iceberg keels). My reading of the recent Dowdeswell & Graham papers suggests these other interpreted settings are not rejected by new observations, rather that it appears that corrugations might form in various tidally-modulated contexts for re-grounding. I think a couple of additional sentences acknowledging this (for example, around lines 45-52) would be helpful; that the Thwaites and Larsen examples likely formed at the retreating grounding line makes them especially significant and important to understand in terms of formation processes.

A follow-on from this distinction is whether the presence of corrugations in other interpreted settings (e.g. iceberg/shelf keels, forward moving) could help us (further) reject/support different mechanisms. Similarity of form and distribution might suggest that a similar mechanism, in some way associated with tidal settling, generates comparable ridge assemblages, whether by a partially grounded sheet or a re-grounding shelf or berg. But not all mechanisms may operate in different settings, or do so with rather different boundary conditions. For example, till flux isn't relevant to iceberg re-grounding cases, which might support its irrelevance, whereas extrusion (and tidal currents – resuspension?) likely does operate. Can this help your discussion?

Note that this is also relevant to the conclusion ("only two real-world examples currently known") – yes, from a grounding line, but a little mis-leading regarding the general form.

2) On first reading, I thought that the observational data record is not presented explicitly enough in the main manuscript for the reader to follow which real-world characteristics and observations are involved in the evaluation of the model output. Data-model comparison is the crux of the paper, but the relevant observational data is somewhat hidden in Table 2 and the supplementary material – there is no section in the text that explicitly states "this is what we see in the observational record". I think some minor text adjustments can improve this, both in Section 2.2 where you outline the data, and in Section 3.2 which is set up as where model output is compared to data – I've made specific suggestions below. But I also think an extra panel in Figure 3 and Figure 4 displaying a topographic profile across the Thwaites and Larsen series of corrugations, respectively, would really help. (The figure caption can make it clear what aspects of morphology it is unreasonable to expect models to replicate, in case this is a concern.)

Text suggestions:

192-198: I think the final sentence here, and thus all the relevant observational data that serves as a basis for evaluation, gets a bit lost. You could lead earlier (e.g. line 192) with a more active statement, to the effect that morphology and spacing observations from the Thwaites data (Graham et al) are given in Table 2, and serve as metrics to be compared against / to evaluate model output. 164 ridges have [… pertinent characteristics…]. You additionally consider morphological elements such as … You could also note, here or at the opening of section 3.2, that while Supplementary Table 1 describes observed morphology as fully as possible, the main traits used for model evaluation are summarised in Table 2.

255-262: you could state in this opening paragraph to 3.2 which key observational data characteristics you will use to evaluate model output. Picking through the following sections, it seems to me these include 14-day periodicity, ridge height–spacing correlation, production of individual or composite forms … These are the main characteristics you've reported from model output, but then you also consider (a)symmetry of form, repeatability of form, ridge volume (based on cross-profile height and width)… It would help the reader to allude to these in an introductory way, so we know what to expect from the following analysis, whether just listing in-text as I have done here, or by giving a short summary of what the main characteristics of the observational data actually are.

Sections 3.2.1, 3.2.2, 3.2.3: these sections are framed as comparing model output to real-world data. 3.2.1 does just this (or begins to), as does the opening paragraph of 3.2.2; 3.2.3 doesn't make such explicit comparisons of ridge morphology or distribution at all, but rather discusses other aspects of the viability of this mechanism. These additional aspects are interesting and important, but beginning each section with a more systematic data-model evaluation would help the overall logical flow and structure of the discussion. Note that even in 3.2.1, you don't directly call upon all your observations to reject the model: state clearly, for example, that observed ridge morphology doesn't match the modelled morphology (composite forms, correlation of height with spacing…) – help the reader work through Table 2, for each modelled mechanism.

3) While Section 3.1 reports the output of the model experiments, in the final paragraph (lines 242-253) you move on to a comparison of one particular aspect (production of composite ridges and relationship with slope) with real-world data. Yet you haven't yet introduced any other explicit model-data comparison. This paragraph seems a bit out of place, since the section structure is otherwise clear and logical; the phrase "quite unlike the observations", while you haven't drawn the

reader's attention to any observations yet, highlights this section being out of place. I would consider bringing this paragraph into Section 3.2, either up front or towards the end.

4) It is not clear in your discussion of the till extrusion model (3.2.2) to what extent your discussion refers to extrusion with a flux, or to extrusion with compression. The opening paragraph (line 270-280) seems to suggest that the bed slope effect (composite ridges) should rule out extrusion with a till flux, but that is not explicit as you move through the rest of the section and discuss (a)symmetry, and consistency of form from ridge to ridge. Are you arguing in these cases that extrusion-with-flux and/or extrusion-with-compression are (or aren't) valid? (This is another example of where a more structured or systematic work-through of key traits in Table 2 vs each mechanism – see my second point, above – could help.)

From line 302 to 322, you then go on to discuss till supply as a "conveyor belt" and other reported estimates of basal sediment flux. In this discussion I no longer follow which modelled mechanism(s) you are using these "flux" estimates and concepts to evaluate: constant flux, extrusion-with-flux, extrusion-with-compression? What does "this" refer to in line 314? (In fact the whole phrase from "to see if…" is unclear.) What process does the "subglacial till conveyor" (line 320-1) refer to? Some work on clarifying the discussion in relation to the specific models would help this section.

Technical comments

12: use of "meter" and "metre" is mixed throughout the text. The rest of the spelling seems to be British English, so suggest changing to "metre" where necessary.

59: "exact mechanism" actually seems a bit vague to me, following from the previous sentences. Perhaps clarify: we need a mechanism to i) generate relief, and ii) account for the observed spatial distribution of that relief (and we can consider particular elements such as size, shape, spacing).

64: add "spacing" to the observed parameters?

105: on first reading of the manuscript, I got a little lost with references to grounding zone, grounding zone cavity, tidal cavity… In your opening sentence you've defined the grounding zone as the zone across which the ice base becomes un/re-grounded with the tides. It would get a bit clunky to add further terms or definitions in that opening sentence, but here (line 105) would be a good place to clarify that this "grounding zone cavity" or "tidal cavity" is the space that opens and closes with each tidal cycle.

133: suggest refer to Fig 2c and 2d.

140: "leaving the till fixed now unyielded" – this is awkward, can you rephrase?

142-4: suggest you insert separate references to Fig 2c and Fig 2d at the relevant points in this sentence.

146: replace "but now" with "which"?

228-9: check the figure panels referred to here – it's not clear to me that 3e/d and 3g/f are the appropriate panels for the statements they are linked to

290: “we do not expect to be able to reproduce ridge asymmetry…” ? What is the purpose of the Graham et al. ref – do they find asymmetry, or not?

291: suggest breaking the paragraph into two, at “Supporting observational evidence…”

325: define these seismic parameters

325: suggest minor rewording of “over a sharp transition” – “over” made me think stratigraphy, then I realise you are more likely referring to an along-flow transition to stiffer till (?)

328: upstream of the grounding zone

334: does “They” refer to Graham et al or Warburton et al?

355, 359: references to Supplementary Material don’t match the Part/Table labelling in the supplementary document. Should be Table S2 (line 355) and Text B (359)?

363: put commas around “on the order of a few cm/s” … and therefore (missing e)

407: delete the second “off”

430-2: suggest extend this sentence with something like “but are evident in observational data from Larsen Inlet”.

441: can you give this as m/day, as well as km/year (since it serves as a comparison to your results, which you discuss in terms of m/day)?

457: “coarse ridge morphology” is a bit awkward, particularly the word “coarse” which can be used in different ways in referring to landforms and tills. Can you rephrase?

513: misspelling Wåhlin

Supplementary:

You have two captions (one above and one below) for the Supplementary Table in Part A, which is labelled both as Table S1 and S2.

S. 49-50: this sentence duplicates the previous

Unfortunately I couldn’t get the videos to play in the Word document. Consider supplying those as separate files.