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The Cryosphere Discussions

Interactive comment on "A numerical study of glacier advance over deforming till" *by* G. J.-M. C. Leysinger Vieli and G. H. Gudmundsson

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"A numerical study of glacier advance over deforming till" by GJM Leysinger-Vieli and GH Gudmundsson

Reviewer: Martin Sharp

In the full review and interactive discussion the referees and other interested members of the scientific community are asked to take into account all of the following aspects: 1. Does the paper address relevant scientific questions within the scope of TC? Yes 2. Does the paper present novel concepts, ideas, tools, or data? Yes 3. Are substantial conclusions reached? Yes 4. Are the scientific methods and assumptions valid and clearly outlined? Mostly, Yes. 5. Are the results sufficient to support the interpretations

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and conclusions? Mostly, Yes 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Mostly, Yes. 8. Does the title clearly reflect the contents of the paper? Yes. 9. Does the abstract provide a concise and complete summary? Some clarification would help. 10. Is the overall presentation well structured and clear? Mostly, Yes. 11. Is the language fluent and precise? Not always. 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Clarified, Yes – see below. 14. Are the number and quality of references appropriate? Yes, with one or two obvious omissions. 15. Is the amount and quality of supplementary material appropriate? N/A.

General Comments

This is a really, really interesting study, for which glacial geomorphology has been waiting over 30 years. At last we are seeing the sort of fusion between studies of ice dynamics and landforming processes that will transform glacial geomorphology into a truly quantitative discipline and lead to much deeper understanding of glacial landscapes and how to interpret them. I congratulate the authors on a beautifully conceived and executed study and an elegant and lucid presentation of their results. I do have some comments and suggestions, however, but these relate primarily to areas where I think the text could be made even clearer, and where the work could be taken a little bit further, turning the paper into what I think will prove to be a true classic of the discipline.

Specific Comments (referenced by page and line number)

2.10: “depth-age relationship” – having read the text I understand that this refers to the ice overlying sediment in the terminus region – but this is not at all obvious at this point. Please clarify.

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2.9: mixed flow: make it clear the ice and sediment are both deforming – it's implicit but should be stated

2.18: "steady-state form" – I understand this refers to the surface morphology – it would be interesting to know whether it also applies to the internal structure of the moraine. Can this be deduced from the model results by tracking particle motion in the sediment through time? Discussing this would be an extremely valuable addition to the paper.

2.19: "resembling single-crested push moraines". How well is the morphology of such landforms constrained by observations? Can it be characterized, for example, by an aspect ratio that could also be computed for the modeled bulge? Could the aspect ratio potentially be used as means to put bounds on the plausible range of values for rheological parameters to be used in till deformation models?

2.23: not obvious to me how mass balance records represent an observation of the advance of glaciers (though I know there's a connection) – I'd delete this statement. In fact, the whole of this paragraph is poorly written and should be re-thought.

3.6: "Glaciers advance by. . .. . ." – what follows is a statement of how glaciers flow. Advance and retreat are really issues of mass continuity in the glacier terminus region, which is partly a function of flow. I would rephrase along the lines "During periods of advance, glacier flow can occur by several mechanisms. . ..."

3.8: relevance of "no surface melt" to whether an advance occurs by overriding is not obvious to me. I can see that whether or not you develop an overhanging front will depend on the rate of melt relative to the rate of horizontal advection of ice towards the terminus – but recumbent folds with axes parallel to the glacier terminus are widely seen in terminal ice cliffs that are non-overhanging (go back to the work of Hooke and Hudleston) – they are just incomplete because ablation is truncating them as they form. This suggests this process is actually fairly common even when there is melt (or dry calving) at the terminus.

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7.2: glacier everywhere in contact with a deformable till layer. OK, but it would be worth commenting on whether this is an assumption that could impact significantly on the results. Would it matter if the sediment were spatially discontinuous or the ice was locally separated from the till surface. It would be helpful to know whether or not this assumption is somewhat restrictive in terms the applicability of the results to the interpretation of field observations. Relaxing it could, for instance, open the way to understanding the formation of other glacial sedimentary landforms like drumlins, flutes and crevasse-fill ridges, for instance.

7.5: assumption of specific and constant till thickness. Similar comment to the previous one – I understand why the assumption is made, but I wonder about the consequences. Some comment on this would be useful.

7.18-7.20: This assumption renders the ELA as conventionally defined a meaningless concept, because it now has a potentially wide range of values. Have to redefine it as the highest elevation at which the annual net balance is zero. It is also an assumption with implications, because it means that the geometry of the terminus is solely determined by coupled ice/till flow in the terminus region, with no influence from ablation. Again, a statement to that effect and a word of caution about how this situation differs from reality would be worthwhile.

7.24: "not affected by details of the starting geometry". How do you know this?

7.24-7.26: I understand the reason for using SIA to get an initial geometry, but it doesn't really matter how you got that geometry if it's true that the initial geometry has no effect on the results. So, it seems to me as if there's a bit of a circular argument here resulting from too great an effort to rationalize what was done. Simplification could be worthwhile.

10.24: not obvious that the temporal evolution of till thickness is plotted here. Next sentence explains why – but I didn't read that until I'd spent some time trying to find it on the figure. Rewording could make life easier for readers.

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11.8/9: I'd reiterate the point that neglect of ablation in the model is relevant to the transferability of this result to the real world

11.17: only now does it become clear that references to age-depth inversion relate to the ice at the terminus. Some mention of recumbent folding in glacier termini as evidence that this may actually happen would be useful. 11.21/22: worth explaining why the velocity maximum occurs at the top of the till here, rather than at the bottom.

12.20: "vertical velocity distribution". Very confusing because it looks like the plot shows velocity vectors rather than magnitudes of the vertical component of velocity, and because it shows both horizontal and vertical variations. Needs rewording.

13.2/3: comment on the reversal in the vertical gradient in effective stress at the ice-sediment interface – and why it occurs.

13.8/9: this sentence is sufficiently unclear that I don't know how to rewrite it. I think you are referring to ice particles previously at the glacier surface coming into contact with the till surface as a result of overfolding within the glacier terminus.

13.11-14: Two comments. (a) Could you compute the internal structure within the till wave? This would be really useful for geomorphologists to see. (b) for purposes of comparison of bulge shapes generated in the different experiments, why not characterize the bulge by an aspect ratio?

15.13-15: could be even more compelling if you could say something about internal structure as well as surface morphology.

16.8: deformation or deformation rate? To me "deformation" implies the finite strain accumulated over a period of deformation – I'm not sure if that's what you actually mean to say.

16.22-26: some similarities here to a propagating surge front on a glacier. Could be worth mentioning.

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16.29: The paper focuses on features produced during a glacier advance. However, sequences of push moraines are often formed during periods of glacier retreat. Must this necessarily imply minor readvances during retreat, or could a slowing of retreat suffice to generate a moraine? It goes back to the importance of understanding advance/retreat as issues of mass continuity in the terminus region – and the limitations of the assumption made here that there is never net ablation at the terminus. This is an area where a bit more focused discussion could add quite a bit to the value of the paper.

17.2: Bit of an over-generalization here. There are numerous examples of very large push moraines in front of glaciers on Axel Heiberg Island. The sediment directly below the ice may be stiff, but the subglacial sediment column contains weaker layers and deformation occurs within them, with overlying material being rafted above the deforming horizon. So, it's a case where the model assumptions do constrain the ability of the model to explain field observations. This is not a big problem, but may be worth noting.

Technical Corrections

2.4: "evolution of the two bodies, and of the contact. . . . .followed through time" 2.5: "rheologies, we. . .." 2.20: "material particle of the till. . .." Rewrite as " shows that particles within the till travel at a different speed from the bulge itself" 3.10: see comment on 2.10. 3.15: "the large number of. . ..studies of the . . ..over a deforming. . .. . ..lack of numerical studies" 3.21: Here, assumptions. . .. 3.25: on a layer of sediment 3.29: "of a glacier over a non-. . .." 4.3: "between the advancing. . ." 4.4: "where the assumptions of. . .." 4.11: "as formulated in. . .." (?) – current sentence reads awkwardly. 5.18: "evolutions. . ..are followed. . ..bodies are. . .." 5.19: "time step, nodes that enter into or out of. . .. . ..conditions are changed. . .. . ." 5.21: "remeshing are employed. . .." 7.7: "added to, or extracted from, the sediments. . . 7.9: "sloping at. . .." 7.17: distribution 8.14: "and grounded tidewater glaciers" 10.7: "extent" (not extend) 11.2: "surface profile at t = 0" - make it clear this is case b on the figure. 12.8: "at a different speed" 12.9: "propagating till wave" (?) 12.28: "in the upstream direction" 15.7: "et al.'s. . .. . ." 15.28:

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"representative of. . ." 17.15: "sediment bed" is confusing. I think you mean the base of the sediment layer. 17.21: " By modelling numerically the advance. . ..sediment layer as a contact. . .." 18.1-3: this comment merits some expansion

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