The Cryosphere Discuss., 6, C2064–C2070, 2012
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The Cryosphere Discussions

Interactive comment on "Calving on tidewater glaciers amplified by submarine frontal melting" *by* M. O'Leary and P. Christoffersen

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Received and published: 1 November 2012

We have considered in detail the comments by both referees, and have revised the paper accordingly. The introduction and conclusion have been substantially rewritten, along with portions of Section 3. We include our reply below on a point-by-point basis.

We believe that the revised paper responds to most of the referees' issues with the manuscript, and that the paper has been greatly improved by these suggestions. We are grateful both to the referees and the editor for their comments.

> Referee #1: Reading this manuscript has given me a sense of distress, not for any reason involving the science (which I believe to be of highest quality and of extraordinary creativity), but rather for reason of format and

readability.

My first complaint is that the downloadable PDF version of the “for review” manuscript and the “html” version that is browsed by the TCD website are different! In one, Figure (1) has two panels, in the other, Figure (1) has only one panel. Also, when I searched for the textual citation to Figure (1) in the PDF version, I did not find it. The html version of the paper did not allow me to search for the word “Figure” and hence I was forced to look for the citation by eye and did not find it.

A referee should not be given two different manuscript versions to review! Think of the work that would be wasted if the referee were to review the version that the authors and editor never looked at. . . the authors and editor would think the referee was a kook!

I opt to stop refereeing the manuscript at this point in a serious manner. Until the manuscript can be “fixed” into one form, it should not be reviewed.

Nevertheless, I do have comments based on my reading of the html version (I looked at the pdf version only after I attempted to figure out if Figure 1 was cited anywhere in the text, which I gather it is not).

The science in this manuscript is very good. The writing style is still too choppy and the organization is suffering from excessive familiarity (i.e., the authors view the path through the paper as logical, due to the fact that they know the story; however, the reader, represented by me, is getting lost on the path and not being able to understand or appreciate what is being developed). There are several instances of 1-sentence paragraphs (an overt sign of skeleton-thin writing that needs to be filled in with a little fat and muscle), and the “natural progression” of the narrative is sometimes piecewise continuous.

We have attempted to ‘bulk out’ the text in several places, although we do note that the

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paper only contained a single 1-sentence paragraph, which is no longer present in the revised version.

> I recommend a complete rewrite of some of the introductory material and the material that describes the final results. Specifically, an "easy" introduction with a simple statement of what the problem under investigation is would be far better than the relatively unnecessary literature review that currently serves as the introduction. I would like to see a diagram that helps to explain the problem set up (hence my interest in searching for where Figure 1 was cited, and my confusion after finding that Figure 1 is different depending on which file you look at on the website). After the model is described, I would like to have a simple easy-going narrative to explain how it will be used, what the basic experiments will be, and what will be looked at in the model results in order to establish the key ideas that constitute the results.

We have rewritten and extended the introduction, giving more of a summary of the problem, as suggested. The geometry of the problem is shown in Figure 1, although it appears that the referee was looking at an earlier version of this figure, which had considerably less detail.

> In the conclusion, the results should be explained in a way that is "narrative" and reference should be given to what aspect of the narrative is supported by the numerical experiments (i.e., telling the reader what the experiments have done to complete the picture).

We have rewritten the conclusion completely along these lines.

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I really like this work; but I feel that it would be remiss to say publish it in the current form: readers would not likely appreciate the very creative and outstanding insights that the work has given.

I recommend a revision of at least the Introduction, the Conclusion and the sections that present the figures. I also would like to see more "problem description and set up" with good diagrams (and a consistent view of figures in all versions of the manuscript that are on the TCD website).

I would be happy to review the manuscript again, and additionally provide greater input into suggested writing revisions. I can't do it now, however, because there are at least two different versions of the manuscript on the TCD website. This is a fatal flaw for refereeing in an efficient manner. The authors should be asking whether the TCD is providing a proper service at the review stage under the circumstances.

Again, we thank the referee for providing comments despite these difficulties.

Specific comments:

1. The first paragraph of the introduction seems unrelated to the main subject of the paper, because undercutting of the faces of tidewater glaciers seems unrelated to the mass balance of large-scale ice sheets.

We have rewritten this paragraph to more clearly indicate the link between calving and outlet glacier dynamics, and thus the connection to the larger endeavour of predicting ice sheet mass balance.

2. The second paragraph of the introduction seems too dependent on terminology in the Benn paper. . . I'm not sure I remember what "first order"

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and “second order” refer to, and the paper referred to is in a journal that I don’t have immediate access to. I wonder if these concepts should be developed in more detail for the reader’s benefit at this point in the paper.

We have removed the reference to ‘first-order’ and ‘second-order’ processes, as this terminology is unnecessary here.

3. I wonder if the second to last paragraph of the introduction might be, in fact, a perfectly good place to begin an introduction (thus making the introduction more directly related to the study that follows)..? In the last paragraph of the introduction (and elsewhere), sometimes ‘Section’ is used, sometimes the abbreviation. . . one or the other should be used consistently.

This inconsistency has been rectified.

In Section 3.2, I ask whether a diagram might be helpful to readers to more quickly appreciate the elements of geometry being defined there.

This diagram is given in Figure 1 of the more recent version of the paper.

Referee #2 (Doug Benn): This is an interesting paper that makes some progress towards understanding the important problem of how subaqueous melting affects the stress pattern and calving behaviour of tidewater glaciers. The subject is important enough and of a wide enough interest for publication in The Cryosphere. On the whole the text is very clearly written and the scientifica case is sound. In the introductory sections, however,

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some minor revisions may be advisable to make the paper more accessible to those not already deeply involved with the calving problem.

A very brief 'menu' of the paper is given in the final paragraph of section 1, but then the paper leaps straight into a discussion of continuum mechanics. For many readers, I think it would be useful to have a clearer statement of the aims and approach of the paper at this early stage. I.e., it should be made clear that the paper investigates the effects of undercutting on glacier stress regimes, by comparing the static stress fields for a series of specified subaqueous geometries. The stress fields are then used as a basis for investigating the implications for calving. The points made in the existing précis can be introduced as part of this more explicit statement of intent.

We have extended the introduction along these lines.

Similarly, section 3 could benefit from some revision. In section 3.1, it is argued that the stresses near the waterline can be used to characterize conditions for calving, based on similar reasoning to that put forward by Benn et al 2007a. I think this needs to be explained in a bit more detail, as at present it may appear a bit obscure to someone not already immersed in the problem. The key ideas here are: calving occurs due to penetration of crevasses, which reflect the state of stress in the ice; first-order crevasse-depth calving models assume that calving occurs when crevasses reach some critical depth; Benn et al., 2007 chose the waterline as that critical depth because of the water-filling argument, but a full-depth variant was also proposed by Nick et al 2010. My present view of the calving criterion is that the waterline threshold is applicable for many glaciers, but not necessarily for the original reasons put forward in the 2007 papers. In a lot of cases, what happens is that the subaerial part of the front calves, followed

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sometime later by buoyant calving of any subaqueous part (which of course does not exist in your simulations). On other glaciers, the waterline threshold may not be so appropriate. For simplicity, I think it is OK to use the waterline as the reference level for your stress comparisons, though I'd go easy on the water-filling argument – in my experience this has acted as an unnecessary obstacle to people's acceptance of the model.

We broadly agree with this assessment, and have rewritten this section to explain our reasoning more carefully.

The following sections are clearly explained, and a convincing case is made that stress retreat transfers the effects of front melting upglacier, and hence scales calving rate to melt rate. The authors quite rightly state that their interpretations should be used with care, due to the links between calving events and glacier dynamics. To investigate this further will require solving the harder problem of a fully coupled time-evolving model with changing front geometry due to melt and ice flow, as well as application of a calving criterion. Perhaps this could be mentioned in the text.

We have added a caveat to this effect to our rewritten conclusion.

Interactive comment on The Cryosphere Discuss., 6, 3287, 2012.