Response to the Interactive comment on

"Basal drag of Fleming Glacier, Antarctica, Part B: implications of evolution from 2008 to 2015"

by Chen Zhao et al.

Anonymous Referee #1
Received and published: 5 Feb 2018

We are grateful to Reviewer 1 for the positive and constructive suggestions to improve our paper. We have addressed the comments below. The line numbers in the responses are based on the revised manuscript without change track.

Please note that Mathieu Morlighem created the ice thickness data for the Fleming Glacier system using the mass conservation method, which is very important for most experiments done in this study. We do value his contribution to this paper, so we add him as the co-author in the revised text.

In the revised companion paper (Zhao et al., companion paper), we implemented a new sensitivity test to the enhancement factor (E). It reveals that the optimal value of E = 1.0 should be chosen as the enhancement factor in the CONTROL experiment. Accordingly, we re-ran all the simulations in this study with E = 1.0, and the high basal shear stress band near the ice front in 2008 has decreased into high basal shear spots, which are suspected of being artefacts of the inversion process and are discussed below. We modified the text and figures accordingly. All other result and interpretations are not qualitatively changed from the original manuscript.

General comments

This paper presents some interesting results suggesting that glacier-bed interactions have an important role in the dramatic speedup of Fleming Glacier, Antarctica from 2008-2015. Recent work by Walker and Gardner posited that abnormally warm ocean temperatures in Marguerite Bay over this time period caused the observed changes in the glaciers that fed the former Wordie Ice Shelf. Friedl et al. 2017 used a combination of several remote sensing data sets to show that large areas near the terminus of Fleming Glacier ungrounded between 2008-2015. These data sets showed that Fleming Glacier lies on a retrograde bed slope, and thus is susceptible to runaway retreat via marine ice sheet instability. Zhao et al. argue that both of these explanations leave out a key factor, the interaction of the glacier and its bed. The authors used inverse methods to estimate the basal shear stress under Fleming Glacier in 2008 and 2015. This analysis revealed a band of high basal shear stress near the terminus in 2008 that is no longer present in 2015. They argue that the retreat of the glacier off of this region of high basal friction may also be a factor in the subsequent speedup.

The large changes that Fleming Glacier exhibited make it a valuable test case for understanding glacier change in Antarctica. The authors' results suggest that glacier-bed interactions are an important factor, in addition to ocean melting and geometric instability, in understanding the recent behavior of the glaciers that fed the former Wordie Ice Shelf. While I recommend publication, the work could be improved on a number of fronts. Drawing conclusions about the physics of glacier-bed interactions from the results of inverse methods can be difficult because, as the authors acknowledge, any one feature could be an artefact. Many of the arguments made in the paper are speculative and I think this should be made clearer. Suppose that the high-basal shear band they claim to find at the terminus of Fleming Glacier in 2008 were merely an artefact – what would that mean for the physics?

This is a great suggestion. As mentioned above, in the modified companion paper (Zhao et al., companion paper), we speculate that the high basal shear spots near the ice front may be artefacts. However, the possibility of the high basal friction spots being real features, which might be caused by pinning points near the 1996 grounding line position is not excluded. Based on the inferred basal shear stress (Fig. 3a) and height above buoyancy (Fig. 5a), the 1996 grounding line position may have not retreated prior to Jan 2008, and Friedl et al. (2018) also suggested that the grounding line position may have retreated behind the 1996 position after Jan-Apr 2008.

The discussion in the manuscript has been modified to respond to the reviewer's comment adding (Line 361-365): "If the high basal resistance spots are artefacts, ungrounding of this region in early 2008 is less viable as an explanation for an abrupt increase in ice flow speed, since the loss of backstress would be more gradual. In this case, positive feedbacks, such as the marine ice sheet instability or the basal melt feedback, are even more likely to explain the FG's recent behavior."

In general, with regard to inverse methods, small features can more easily arise as inversion artefacts than larger features. Small basal shear stress features may be locally balanced by extensional/compressional stresses in the ice without needing to balance the gravitational driving stress. For features with larger horizontal scales basal shear stress must approximately balance the driving stress and these features are less likely to be artefacts. All features discussed in the paper arising from the inversion process, aside from the sticky spots near the 2008 front, are large enough that we are confident they are robust features of the inversion and not artefacts.

Currently one possibility for rapid retreat of the grounding line is that there were some sticky spots near the front, and rapid retreat occurred when the ice ungrounded from these sticky spots.

Specific comments

First, I think the abstract could be improved by (1) cutting many of the details that are covered in the discussion section and (2) giving a clearer statement about what this paper adds to the existing knowledge. The main precedents that the authors draw from are Walker and Gardner 2017 and Friedl et al. 2017. What do these two papers conclude, and how do the authors' conclusions agree with or depart from them? For example, both the present work and Friedl et al. 2017 argue that the speedup and thinning of Fleming Glacier is a consequence of ongoing marine ice sheet instability. However, the authors argue that subglacial hydrological effects may have also initiated the retreat off of a stable bedrock high, while Friedl et al. point solely to ocean warming. To my knowledge, this work and the companion paper are the first to use inverse methods to estimate the basal shear stress of this particular site at high resolution, as opposed to low-resolution estimations for all of Antarctica. This information, which ideally would be front-and-center in the abstract, is partly obscured by details that will be addressed in the discussion section anyway. In any case, this problem is more one of presentation and not of actual content.

Thanks for the reviewer's suggestion. We added this point in the first paragraph of

Section 4.1 (Line 225-229) "Although low-resolution estimation of basal shear stress has been carried out for the whole Antarctic Ice Sheet (Fürst et al., 2015; Morlighem et al., 2013; Sergienko et al., 2014), this is the first application of inverse methods to estimate the basal friction pattern of the Fleming system at a high resolution and use the full-Stokes equations." and modified the conclusion and abstract correspondingly.

At several points, the authors pose the question of whether either ocean warming or basal processes are the "dominant" causes of the observed changes (see lines 84, 411). The question of which process is dominant assumes that the two are additive, but if instead the relationship is causative, this question ceases to be meaningful.

The reviewer seems to think that when we say "dominant" we mean "only". Of course, it is possible to have a small perturbation caused by one process (ocean-warming driven basal melting) and massively enhanced by another process (basal process). In this case we would describe the latter as "dominant" because it has caused the biggest change, even if there would have been no change without the former.

In the discussion section, the authors suggest that hydrological effects could destabilize the high-friction band, resulting in speedup, thinning, and ungrounding. In this scenario, hydrology-induced speedup and ungrounding create conditions where the ocean can then melt the ice shelf from underneath. One could also imagine a scenario in which ocean melting comes first and hydrological effects second. For example, ocean melting could push the glacier terminus off of a highly resistive bedrock bump, and the glacier begins to speed up and thin. The reduced overburden pressure then changes the overall hydraulic potential. The authors' hypothesis that hydrology might have initiated the recent changes is still significant and worth considering. Nonetheless, the paper's intent might be clearer by changing questions about which process is dominant to questions about which one came first. Finally, the authors suggest that coupled ice sheet-ocean modeling could help determine which case is more likely. This point could be expanded on further. For example, a coupled model using pre-2006 values of ocean heat flux that does exhibit a hydrology-induced destabilization would show that oceanic forcing is not necessary to explain observations.

We don't see a need to choose to only consider which process comes first or which process is dominant. The two questions are complementary rather than contradictory. When we discuss which process is dominant we do not mean to exclude the relevance of which came first. It was not our intention to propose that the changes were initiated by the subglacial hydrologic system – we don't have a mechanism in mind for that. We don't see how an increase in subglacial melting can happen without an external trigger, except through increased insulation due to ice thickening such as occurs in surging glaciers. But we doubt this is happening here. We suspect the ice shelf collapse triggers a positive feedback at the bed of the fast flow region, and that once the shelf has gone, the melt rates due to the ocean warming do not make much difference. Subglacial melting probably has to be happening all the time under the fast flowing region in any case. Ocean melting/ice shelf collapse provide a triggering mechanism to the ungrounding process, and then the positive feedback between the basal sliding and subglacial water pressure at the bed kicks in. We have clarified the nature and role of this positive feedback mechanism in the Sect. 4.2 and Sect. 5.

We don't think there is a need to expand further about designing coupled experiments as that is well outside the scope of this paper.

The text gives conflicting statements about the authors' degree of confidence in the veracity of their conclusions. For example, in line 314 the authors assert that the disappearance of the high friction band near the calving front is a "likely" trigger for the subsequent retreat, but at other points they equivocate about whether this feature is real or merely an artefact. A lack of complete certainty about this resistive band is entirely reasonable but the paper would be improved if it were more consistent in what kind of assertions are made.

Thanks for pointing this out. Based on the modified companion paper (Zhao et al., companion paper), we speculate that the high basal shear spots in 2008 (rather than the band of high shear seen in the previous version) may be artefacts but we do not rule out the possibility of high friction spots as a real feature caused by the pinning points at the 1996 grounding line. For consistency we modified the text in the manuscript accordingly (Line 359-361).

For the example mentioned by the reviewer, we modified "likely" to "possible" (Line 383). Under this speculation, if the sticky spots were totally artefacts, the reduction in basal drag would be likely due to the positive feedbacks between the basal sliding and basal subglacial water.

A numerical experiment could shed some more light on whether the resistive band near the terminus is real or not. The methods section describes inferring the basal friction using the 2008 ice thickness and the 2015 velocity to examine whether the result is sensitive to the geometry. In this vein, the authors could compute a velocity using the 2015 basal friction and the 2008 thickness. How well does this computed velocity agree with observations, weighted by the error variances? Is the misfit worse than that of the velocity computed using both the 2008 thickness and basal friction? If so, by how much? The presence of a resistive band at the terminus would be doubtful if a basal friction field without this feature can explain the 2008 data just as well as a basal friction field with this feature.

The basal friction field without the sticky spots cannot explain the 2008 data. Although we are not sure whether the high basal drag spots in 2008 are real or not, we are sure that the basal drag of high velocity regions in 2008 should not be as small as that in 2015. However, we still tried this experiment as the reviewer suggested. Results show that the simulated surface velocity was nearly 2.5 times the observed surface velocity of 2008 near the ice front. So we cannot use the suggested experiment to say that the sticky spots are an artefact.

Technical corrections

17-21: Flip the order of the sentences starting with "To explore the mechanism underlying these changes..." and "Recent observational studies..."

Modified.

23-28: Giving too much justification in the abstract obscures your overall point, this could be moved to the discussion.

We agree to remove the sentence about the grounding line position in 2008, but we think the comparison results between 2008 and 2015 should appear in the Abstract.

66-69: "As a marine-type glacier system..." Rephrase or break up into 2 sentences.

The whole sentence has been modified into "As a marine-type glacier system residing on a retrograde bed with bedrock elevation as much as ~800 m below sea level (Fig. 1c), the Fleming system is hence potentially vulnerable to marine ice sheet instability (Mercer, 1978; Thomas and Bentley, 1978; Weertman, 1974). The acceleration and greater dynamic thinning of the FG over 2008-2015 suggests the possible onset of unstable rapid grounding line retreat (Walker and Gardner, 2017; Zhao et al., 2017), which has been confirmed by Friedl et al. (2018). " (Line 74-79).

73-74: "None of these past studies have modelled the glacier system and hence these hypotheses are untested." This suggests that modelling is the only way to really test these hypothesis. It's better to just say that the precise nature of the feedbacks hasn't been established and that you will test them using models.

Thanks for pointing this out. We modified this sentence into "An alternative hypothesis is that the recent changes arise from feedbacks in the dynamics of the evolving glacier, possibly involving the subglacial hydrology. The examination of changes in basal shear stress distributions between 2008 and 2015 in this modelling study provides a first step in exploring possible feedback hypotheses. " (Line 83-87).

88-90: "Changes in basal shear stress..." Rephrase this sentence.

Reviewer 3 has suggested deleting this sentence, since it is not helpful here. We agree with Reviewer 3, so we delete this sentence.

162-165: "To explore their relative impacts..." While this experiment is a good sanity check, the result isn't essential to making your point and this could be relegated to a supplement.

Thanks for the suggestion. We have moved this part into the Sect. S1 in the supplementary material.

294-296: Make this "The change in area", and "additional evidence supporting the hypothesis of rapid grounding line retreat".

We modified it into "This change in area" and "additional evidence supporting the hypothesis of rapid grounding line retreat" (Line 354-356).

313-315: Could the basal resistance band at the front be an artefact of neglecting backstress at the terminus from melange or sea ice?
We have discussed this in Sect. 4.4 of the revised companion paper (Zhao et al., companion paper). The ice mélange back force (~1.1e7 N m^{-1}) used to prevent the rotation of an iceberg at the calving front (Krug et al., 2015) could account for the equivalent of up to ~2.3 m sea level in terms of ice front boundary condition. The experiment with the sea level increased by 10 m shows that the high basal shear spots are decreasing but have not disappeared. The situation at the front is complicated. Sea level, bedrock/ice thickness uncertainty, mélange backstress, ice front positions, these things can all impact on our inversion near the ice front.

324-327: Overly long sentence, break up into 2 sentences.

Modified into "For a glacier lying on a retrograde slope in a deep trough, the grounding line may be vulnerable to rapid retreat without any further change in external forcing, once its geometry crosses a critical threshold, which is the marine ice sheet instability hypothesis (e.g., Mercer (1978); Thomas and Bentley (1978); Weertman (1974)). A similar theory has been proposed on the prospective rapid retreat of Jakobshavn Isbræ in West Greenland without any trigger after detaching from a pinning point (Steiger et al., 2017)." (Line 394-399).

326-327: "...as in the rapid retreat of Jakobshavn Isbrae in West Greenland (Steiger et al., 2017)." There were other factors in the retreat of Jakobshavn, see Motyka et al. 2011 and Holland et al. 2008.

Yes, we agree with the reviewer. We should have made it clear that we were talking about the future behavior of the Jakobshavn here. Steiger et al., 2017 found that after decades of stability and with constant external forcing, the grounding lines of Jakobshavn may retreat rapidly without any trigger due to losing the pinning-points. To make it clearer, we modified it into “A similar theory has been proposed on the prospective rapid retreat of Jakobshavn Isbræ in West Greenland without any trigger after detaching from a pinning point (Steiger et al., 2017).”

336-340: Run-on sentence, break up into 2 or 3 sentences.

Modified into “If the system remains out of balance and continues to thin, the grounding line could eventually move across this bed obstacle. If this occurs, the grounding line is then likely to retreat rapidly down the retrograde face of the FG upstream basin, likely to be accompanied by further glacier speed up and dynamic thinning. ” (Line 409-413)

400-402: "...hard to say how much forcing would be needed to push the grounding line into it." Rephrase.

Modified into “More thinning would be needed to destabilise the upstream basin, and it is hard to estimate how much forcing would be needed to push the grounding line into the upstream basin boundary.” (Line 497-499).

414: Change "simulate" to "estimate".

Modified.

References

Friedl, P., Seehaus, T. C., Wendt, A., Braun, M. H., and Höppner, K.: Recent dynamic changes on Fleming Glacier after the disintegration of Wordie Ice Shelf, Antarctic Peninsula, The Cryosphere, 12, 1-19, 2018.

Krug, J., Durand, G., Gagliardini, O., and Weiss, J.: Modelling the impact of submarine frontal melting and ice mÃ©lange on glacier dynamics, The Cryosphere, 9, 989-1003, 2015.

Mercer, J. H.: West Antarctic ice sheet and CO2 greenhouse effect: a threat of disaster, Nature, 271, 321, 1978.

Steiger, N., Nisancioglu, K. H., Åkesson, H., de Fleurian, B., and Nick, F. M.: Non-linear retreat of Jakobshavn Isbræ since the Little Ice Age controlled by geometry, The Cryosphere Discuss., 2017, 1-27, 2017.

Thomas, R. H. and Bentley, C. R.: A model for Holocene retreat of the West Antarctic Ice Sheet, Quaternary Research, 10, 150-170, 1978.

Walker, C. C. and Gardner, A. S.: Rapid drawdown of Antarctica's Wordie Ice Shelf glaciers in response to ENSO/Southern Annular Mode-driven warming in the Southern Ocean, Earth and Planetary Science Letters, 476, 100-110, 2017.

Weertman, J.: Stability of the Junction of an Ice Sheet and an Ice Shelf, Journal of Glaciology, 13, 3-11, 1974.

Zhao, C., Gladstone, R., Zwinger, T., Warner, R., and King, M. A.: Basal friction of Fleming Glacier, Antarctica, Part A: sensitivity of inversion to temperature and bedrock uncertainty, The Cryosphere, companion paper. companion paper.

Zhao, C., King, M. A., Watson, C. S., Barletta, V. R., Bordoni, A., Dell, M., and Whitehouse, P. L.: Rapid ice unloading in the Fleming Glacier region, southern

Antarctic Peninsula, and its effect on bedrock uplift rates, Earth and Planetary Science Letters, 473, 164-176, 2017.