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 #3
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We are grateful to Reviewer 3 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

Main question in the abstract: Is the observed acceleration of the flow and thinning of the glacier due to increased ocean warming and/or marine ice sheet instability?

Method: Infer basal shear stress from observations and calculate a steady state temperature field using a Stokes ice sheet model for 2008 and 2015.

Results: Reduction in magnitude and increase in area of low basal shear stress near the 1996 grounding line and reduction in height above flotation between 2008 and 2015 suggest the grounding line has retreated for Fleming Glacier, southern branch of Fleming Glacier and Prospect Glacier.

A band of higher basal shear stress parallel to the 1996 grounding line at 2008 suggests that Fleming Glacier was still grounded at that time. Subglacial water may be generated from high basal frictional heating upstream of Fleming Glacier. Frictional heating has increased between 2008 and 2015 over a rise between two deep bedrock basins.

As mentioned above, in the revised companion paper, we implemented a new sensitivity test to enhancement factor (E). It reveals that the optimal value of 1.0 should be chosen as the enhancement factor in the CONTROL experiment. So we redid all the simulations in this study with E=1.0, and the high basal shear stress band near the ice front changed into high basal shear spots in 2008, which are suspected to be artefacts. We did not rule out, however, the possibility that the ice front was still grounded on some pinning points. We discuss this point in the first sentence of the Discussion section (Line 359-361).

Comments: I don't think the main question can be answered from instantaneous time slices of the ice flow. The authors need to do forward experiments with various ocean forcing such as different basal melt rates or vertical melting at the calving front. Alternatively, the authors need to pose a different question. The band of high basal shear stress may not be physical realistic. The model error reported in their companion paper is relatively high in this area.

Clearly forward modelling of the Fleming system to study the recent ungrounding transition is a natural next step. That would, as the reviewer acknowledges, require an extensive exploration of forcing influences. In the present work we have clearly shown the differences in basal shear stress distributions for the Fleming system between 2008 and 2015, reflecting different surface elevations and the recent acceleration in ice flow. This has provided insights into the recent ungrounding – and suggested possible feedback processes that may have contributed to the recent changes. We consider this scope has provided sufficient worthwhile material for the present paper. Experiments with future coupled ice sheet-ocean models would also be valuable. We have mentioned this in the Conclusion section (Line 535-537).

In the modified companion paper (Zhao et al., companion paper), the misfit between the simulated and observed surface velocity at the ice front of the FG is very small. The difference between the relaxed and observed surface is < 15 m after three cycles in the CONTROL experiment. It means the modified model with the enhancement factor of 1.0 models the ice front well.

Interesting idea: The authors propose that basal water generated from high basal frictional heating upstream draining towards the front, triggered grounding line retreat of Fleming Glacier. This mechanism is an alternative to the usual ocean forcing explanation. Mass loss could significantly increase, due to marine instability, if the grounding line retreated over a bedrock rise into the second deeper basin. The highest frictional basal heating in 2015 is located over the rise, which may be a potential trigger for the grounding line retreat.

Manuscript in general: The font is too small and the text is not double spaced, which made reviewing the paper tricky. Picking out the references was particularly difficult give the font size and text spacing. Some of figures are too small.

Apologies if the manuscript was not in the format the Reviewer expected. We are happy to comply with whatever formatting requests are made by the Copernicus staff in this regard.

SPECIFIC COMMENTS

Ocean forcing: It seems reasonable to suggest that increased melting at the vertical face of the front of FGL due to incursions of CDW may have affected the pressure boundary condition at the front sufficiently to remove the high band of basal shear stress. However, I don't think your results shed any new light on what has been suggested in the other references you use about ocean basal melting. Forward time-dependent modelling experiments are needed to test these theories and here's an example for Larsen B of how you can extend the work you have done for this paper. Vieli et al 2007 Causes of pre-collapse changes of the Larsen B ice shelf: Numerical modelling and assimilation of satellite observations. Earth and Planetary Science Letters. https://doi.org/10.1016/j.epsl.2007.04.050

As we mentioned above, we agree that transient experiments will be valuable, but are beyond the scope of the current study. We aim to carry out both transient ice dynamic simulations and coupled ice-ocean simulations, and hope we will be able to bring such studies to fruition over the coming years.

Grounding line retreat: The results for 2015 of low basal shear stress and low height above buoyancy confirm the findings of Friedl et al 2017 that Fleming Glacier's grounding line has retreated. The results for PGL are different to FGL: Driving stress appears to be much higher for PGL in 2015.

The revised ratio of driving stress τ_{d2015} to τ_{d2008} (Fig. 3f) shows that the driving stress of PG in 2015 was much lower (not higher) than 2008. We have clarified that the cases for the southern FG and PG are different from the main branch of FG. We did not account in the model for the remaining ice shelf for those two glaciers because we do not have the ice thickness data for the ice shelf. We modified our analysis on those two glaciers (Line 275-284). We think the northern section of the southern FG has been ~2 km behind the 1996 grounding line position based on the ice front position shown in Fig. 1c. However, it is hard to decide whether the southern section of the southern FG or the PGL have also retreated from 2008 (Fig. 3a) to 2015 (Fig. 3b), since we did not account for the normal stress of the remaining small ice shelf at the front of the southern FG (Fig. 1c) in the inverse modelling. Note that the hypsometric model used generate the DEM in 2015 is based on the observed elevation change rates (Zhao et al., 2017). However, the observations are mainly focused in the FG region (Fig. 2a), so the DEM2015 of PG could be an artefact. That might explain why the driving stress was lower in 2015 (Fig. 3f).

Temperature homologous near the 1996 grounding line (for PG) appears much lower in 2015 suggesting that the glacier may have become frozen to the bed?

Note that we have replaced the term "temperature homologous" with "temperature relative to pressure melting point" in the entire text.

The temperature near the ice front/grounding line of PG is indeed colder in the 2015 steady-state calculation. The main difference in the modeled temperature between 2008 and 2015 is due to a reduction in friction heat. This is in turn due to reduced basal shear stress, which occurs in the inversion as a result of the reduced driving stress compared to 2008. This may be due to the lack of observational hypsometric data – the imposed surface lowering (which causes the driving stress reduction) in 2015 is based mainly on data from FG.

A contributing factor could be the steady state temperature assumption, which is almost certainly worse for 2015 than it is for 2008, because the recent acceleration means that the glacier is further from steady state in 2015 than in 2008.

Also, the current modelling approach does not represent the capacity of the subglacial hydrologic system to redistribute heat at the bed. In reality the flow of basal melt water from upstream to downstream will bring more latent heat to the base of the ice sheet near the grounding line.

Is the band of high basal stress at the front of FGL physically realistic? The authors attempt to address this question in the paragraph beginning on line 209. Part A shows that the misfit between the modelled and observed speed is high, where the modeled speed is too fast, and the surface slope is also higher here than over the region of low shear stress. The driving stress is not obviously high given the relatively high surface slope. What concerns me is your model appears unable to model the front.

As mentioned above, the revised companion paper of this study (Zhao et al., companion paper) shows high basal stress spots rather than a band (as previously) at the front of FGL. This may, as the reviewer suggests, be an artefact, owing to various uncertainties. We also do not rule out the possibility that the ice front was still grounded on some pinning points. We clarified this in Line 359-361.

In the revised companion paper (Zhao et al., companion paper), the misfit between the simulated and observed surface velocity at the ice front of the FG has been very small. The difference between the relaxed and observed surface is < 15 m after three cycles in the CONTROL experiment. It means the modified model with the enhancement factor of 1.0 models the ice front in 2008 better now than in the version of the companion paper to which the reviewer refers. It is also worth noting that our 2015 simulations have not had any difficulties modelling the ice front. This suggests that the problem is in the boundary conditions rather than the model itself, which was the motivation for the ice front position and pressure sensitivity experiments in the companion paper. These experiments indicated that the inversion is only sensitive to such ice front uncertainties within a short distance of the front.

What about rheology of the ice near the front? Perhaps the standard A is not appropriate here. Part A shows a large vertical shear at the front where the basal speed is much smaller than the surface. Is the ice stiffer at the front? Vieli et al 2006 Numerical modelling and data assimilation of the Larsen B ice shelf, Antarctic Peninsula, Phil. Trans. R. Soc. A, 364, 1815–1839, doi:10.1098/rsta.2006.1800 solved the inversion problem for effective viscosity. Modelling a front is difficult!

Based on the sensitivity test to various values of enhancement factors (0.5, 1.0, 2.0, 4.0) in the revised companion paper, we found that the value of 1.0 is the optimal value for the overall Fleming system. Various studies of anisotropic ice properties and enhancement factors (e.g. Graham et al. (2018); Ma et al. (2010)) suggest that ice near the ice front could well be stiffer than ice deforming under simple shear near the bedrock in the interior of the ice sheet, however, we have only a uniform enhancement factor E as a control parameter in the present study.

About solving the inversion problem for effective viscosity: it is simple to invert for ice rheology in an ice shelf model, as suggested. Here for the grounded glacier - certainly largely grounded in 2008 - the velocity mismatch can be addressed by adjusting the ice stiffness and the basal drag. Simultaneous inversions for stiffness and basal friction coefficient are possible but beyond the model tools we have available.

What about the direction of the flow? Is there a difference is the modelled flow direction and the observed direction? Is there a change in flow direction between 2008 and 2015 as the ice moves over the sticky band and becomes ungrounded. Also, could ice melange at the front FGL affect the boundary condition?

The inversion scheme we used (following Gagliardini et al. (2013)) only compares the mismatch in modelled and observed speeds, not directions. To a simple visual inspection the velocity directions in 2008 and 2015 are very similar if not identical. A direct overlay of streamlines may allow minor deviations to be identified, but we have not identified an urgent need to such analysis to be carried out.
About the ice mélange at the ice front, we explored the effect of an extra normal force at the ice front (to simulate the potential effect of ice mélange) in the ice front boundary condition experiments of the revised companion paper (Zhao et al.,

companion paper). We calculated that ice mélange back force (~1.1e7 N m^{-1}) used to prevent the rotation of 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, which was included in the experiments with different sea levels.

Basal frictional heating and subglacial water: Could the region of low basal shear stress near the front simply be due to subglacial water from upstream pooling in the bedrock basin, e.g. FGL in 2008? Could the region be partially grounded?

The reviewer may be right. Figs. 4d and 4e show that there is a plateau in the hydraulic potential in the downstream basin. In 2015, we think the downstream basin is mainly ungrounded, based on the inferred basal shear stress (Fig. 3b) and the height above buoyancy (Fig. 5b). There could therefore be some pooling of water in 2008, and it could be partially grounded, but a big cavity is not possible given the geometry. Our inferred basal shear stress (Fig. 3a) and the height above buoyancy (Fig. 5a) show that the downstream basin of the FG in 2008 should still remain grounded.

In the discussion section, we have clarified the issue of basal hydrology, and the potential of water to pool at certain locations (Line 471-476). "The plateaus in hydraulic potential in both downstream and upstream basins of the FG suggest the possibility that basal water may accumulate in those regions, or at least show a low throughput. The downstream plateau appears to be fed by a large frictional heat source over the ridge between the downstream and upstream basins in addition to flow from further inland, while the upstream plateau appears to be fed by an extensive upstream region of basal melting. "

The temperature homologous is high, which prevents the water from refreezing. I think the role of subglacial water could be explained more in the literature review. Paragraph beginning 85:

We modified this sentence (Line 98-102) into "A positive feedback between basal sliding and basal water pressure (through friction heating) upstream of the grounding line could be another possible factor in the glacier acceleration and grounding line retreat (Bartholomaus et al., 2008; Iken and Bindschadler, 1986; Schoof, 2010). The possibility of such a feedback, is not ruled out by Friedl et al. (2018), and is discussed further in Sect. 4.2 and Sect. 5."

You don't explain what the feedback mechanism is. As I understand it, Schoof (2010) talks about the importance of variability of basal water on flow dynamics, with flow accelerating due to a short-lived increase in basal water, but then the flow slows if the basal water stays high. Is that happening here? You have high basal frictional melting in 2015, which you say is speeding up the flow, but figure 3 in Friedl et al 2017 shows that the ice speed of FGL decreased between 2011-2015.

We have added a description about the positive feedback in Sect. 4.2 (Line 288-301). As we have clarified, "Since the reduction of effective pressure is the key process to enhance sliding, this positive feedback is dependent on a positive feedback of melt water generation to water pressure. This dependence can break down when there is sufficient basal water to generate efficient drainage channels (Schoof, 2010). However, such efficient channelization in the subglacial hydrologic system is typically associated with seasonal surface meltwater pulses reaching the bed (Dunse et al., 2012), a process that is not expected to occur for Fleming Glacier (Rignot et al., 2005)."

In the published Fig. 2 and Fig. 3 in Friedl et al. (2018), it shows that the ice speed of

FGL remains stable with a very small median velocity increase (0.06 m d^{-1}) from 2011 to 2016, not decrease. Besides, the speed up in 2015 is relative to the surface velocity 2008. We do not dispute the observed acceleration phrase occurred in Mar 2010-early 2011 found by Friedl et al. (2018).

Figure 1 shows that the ice front for PGL and sFGL has calved between 2008 and 2016, with some advance in the southern part of the bay. Could calving event(s) explain the speed up and lowering of the surface of the streams? Also, from figure 1, PGL has an ice shelf, are you applying the normal stress, hydrostatic pressure boundary condition at the 1996 grounding line or at the real front?

Yes, we agree with the reviewer. The calving events may explain the speed up of the PGL and sFGL. The surface lowering for those two regions have not been confirmed by Zhao et al. (2017) owing to the lack of elevation observations. But inversions will not give a clear answer to this - transient experiments would be needed. A good experiment to do would be to carry out an inversion with an advanced ice shelf, then two transient experiments: one simply carrying on from the inversion, and one in which the shelf is removed. The difference between these transient experiments would be informative as to the impact of calving on flow speeds and surface lowering. Such experiments would be interesting, and we hope to have a chance to carry out such experiments, but that they are beyond the scope of the current study.

As we responded above, we did not account for the remaining ice shelf for those two glaciers because we don't have the ice thickness data for the ice shelf. We modified our analysis on those two glaciers (Line 275-284).

Bedrock plots: The way the bedrock is plotted is a bit inconsistent and unclear. Figure 1 c clearly shows where bedrock is above or below sea level with a white colour band around 0, but the bed elevation colour in figure 4 c and d looks like most of the bedrock is either below or at sea level and figure 2 b is too small. It would be useful to see where the retrograde and prograde slope are.

We modified Fig. 2b to have the same color scale as Fig. 1c. For Figs. 4d, 4e (original Figs. 4c, 4d), we think it is better to use a different color scale to show the retrograde and prograde slopes. Here we plotted the bed elevation with meters above sea level. The regions of retrograde slope are now easily identifiable by eye in Figs. 4d, 4e, but plotting these regions is not trivial to automate.

Figures 3,4,5: useful to have a third column of figures showing the difference between the first two columns.

Thanks for the suggestion. For Fig. 3, we computed the ratio of τ_{b2015} over τ_{b2008} (Fig. 3e), and also the ratio of τ_{d2015} over τ_{d2008} (Fig. 3f) to represent the difference. For Fig. 4 and Fig. 5, we plotted the difference between 2015 and 2008 (2015 minus 2008).

Line 158: The Linear sliding law is fine for the inverse problem because τ remains unchanged, if a higher power of u is used, only the coefficient C would change. However, for a forward run a linear law may be inappropriate.

We agree. There is, however, no transient simulation involved in this study except in the brief surface relaxation step. For the steady-state temperature simulation, the Stokes solver is turned off and the velocity field is fixed. It means that the basal shear stress is fixed for the temperature simulation. So we don't think it is inappropriate to run our simulations with a linear sliding law. For transient simulations we intend to convert C to a distribution that, for whatever sliding law is used, gives the same initial basal shear stress distribution as we obtain from the inversion.

Please define basal frictional heating in your method section.

We have added the equation as Eq. 4.

Figure 2 is too small and/or too detailed. A difference plot of surface elevation may be more informative.

Modified.

Figure 3: The patterns in c and d seem to be influenced by the computational mesh. Have you investigated mesh resolution by halving or doubling element sizes?

The sensitivity tests to horizontal (1 km, 500 m, 250 m, 125 m) and vertical (10 layers and 20 layers) mesh resolution have been carried out in our companion paper (Zhao et al., companion paper). The resolution of 250 m is fine enough for inverse modeling in this study. Also note that the features in Figs. 3c and 3d (now 3d and 3e after revision) are much coarser than the element size – these features are resolved.

The ratio of basal shear stress to driving stress: I'm not sure what figure 3 c and d are showing or why a low value means the ice may be close to flotation.

A balance between tau_b and tau_d is indicative of a heavily grounded regime – longitudinal stresses are low and the dominant force balance is between the gravitational driving stress and the basal resistance. This assumption is similar to the assumption behind the derivation of the "shallow ice approximation", which has been used extensively for long time scale grounded ice sheet simulations. The opposite assumption, the "shallow shelf approximation" (SSA), is a balance between membrane stresses and driving stresses, with basal shear stress being vanishingly small. This is the typical stress balance in a floating ice shelf. So a low value of this stress ratio means we're closer to the SSA regime, i.e. an ice shelf regime.

Figure 4 c and d: Would showing the potential gradient be more informative? I can't see labels on the contours of hydraulic potential.

We agree that the direction of flow is important, but a vector plot of the potential gradient would be rather complicated. The direction of flow, perpendicular to the contours of hydraulic potential, should be clear from the figure, with very few local minima. The magnitude of the gradient can be clearly seen in the proximity of contours of hydraulic potential. Where the contours are close together is where the gradient is steep. Labelling the contours increases the complexity of the plot, and in fact the main purpose of the whole calculation is to demonstrate the pattern of basal water flow rather than to estimate specific values. We have actually tried several options for how to present this Figure, including a scalar field plot of hydraulic potential gradient, but we are still confident that our current combination of bedrock and hydraulic potential contours gives the clearest picture of the pattern of basal water flow.

Figure 5: Height above buoyancy appears to be negative south of the main stream of FGL (and south of PGL). Is the bedrock above sea level there?

No. Note that the height above buoyancy is never negative where the bedrock is above sea level (Eq. 7). Negative values simply indicate that the estimated thickness of ice present should be afloat, given the bedrock is so far below sea level. As we discuss in the text, uncertainties in ice thickness and bedrock data affect the calculation of the height above buoyancy. The bedrock of the southern branch of the FG is below sea level while a little part of the south PG is below sea level. To clarify the question of bedrock values, we modified the Fig. 1c to show the bedrock below sea level only.

Discussion section: Is a maximum melt rate of 1 m/a enough to generate a plume of high enough velocity to entrain incursions of CDW to enhance basal melting beneath the floating ice? You can calculate the flux of subglacial water for each year by Taobub/Lii x area that feeds the grounding line based on the hydraulic potential (or its gradient).

To address this question, we need a 3D ocean model. The buoyant plume is a function of many things, of which subglacial outflow is only one. We'd need to know a lot about the CDW pathways in the area, whether CDW is coming into contact with the grounding line, volume fluxes, heat fluxes, the regional oceanography. So yes, calculating total subglacial outflow is relatively straightforward but simulating the local ocean circulation and plume behavior is way beyond the scope of the current study, and the subglacial outflow is a fairly meaningless number without this oceanographic context.

Line 395: Could you explain the positive feedbacks. Estimating the time scale for the ice to unground from the rise between basins leaving the ice stream vulnerable of marine instability in the upstream basin is good, but I'm not sure you can say height above buoyancy is a measure of potential mass loss.

We have added a description of the positive feedbacks in Sect. 4.2 (Line 288-301).

We did not say the height above buoyancy could indicate potential mass loss. It indicates potential vulnerability. If Z* is close to zero, then the system is close to ungrounding, and would only require a small perturbation to unground. The height above buoyancy would be relevant to sea level rise in a grounding line retreat situation. The accelerated ice flux across the grounding line is probably a separate issue.

TECHNICAL CORRECTIONS

Line 46: abbreviation 'GL' is not defined.

We don't use the abbreviation "GL" for the "grounding line", so we modified "GL" into "grounding line".

Line 88: Not sure the sentence is helpful. Might be better to delete it.

Deleted.

Section 2.2: Is Hmc part of a dataset from Morlighem or have you combined two dataset yourself?

Morlighem has been added as the co-author of both the companion paper and this paper. He generated Hmc for the companion paper (Zhao et al., companion paper). Hmc includes three regions: for fast flowing region, he computed the ice thickness data for fast-flowing regions using the Ice Sheet System Model's mass conservation method (Morlighem et al., 2011; Morlighem et al., 2013), based on ice thickness measurements from the Center for Remote Sensing of Ice Sheets (CReSIS), using ice surface velocities in 2008 from Rignot et al. (2011b), surface accumulation from RACMO 2.3 (van Wessem et al., 2016) and 2002-2008 ice thinning rates from Zhao et al. (2017); for slow flowing region, he adopted data from bedmap2; for the transition region, he smoothed the data. It has been clearly described in the revised companion paper.

Lines 132, 151: Part 1 or Part A

Thanks for pointing this out. It should be "Part A".

Line 145: Is the basal frictional heating calculated from output from the inverse problem and used as an input into the heat equation?

Yes. The basal frictional heating shown in Figs. 4a and 4b, is calculated directly from the output of the inversion process – as the product of inferred basal shear stress and basal velocity – see Eq. 4, which we added at the request of this reviewer (see above). The basal frictional heating is an integral part of the steady state temperature simulation. That calculation does use the velocities and friction coefficients from the inversion, so the frictional heating from the inversion is indeed included.

184: I don't think N needs a numbered equation because it isn't used.

We would like to make this change if the Copernicus proofreaders request it.

Line 222: northern and eastern. It might be helpful to add an arrow indicating North on one of the figures.

Added.

Lines 367, 420: Friedl et al 2017 gave an estimated grounding line for 2014.

Thanks for pointing this out. We have modified the relevant text to grounding line in 2014.

Figure 1: sFGL is not marked on the figure.

Added.

Figure 3: It is difficult to work out where the plotted regions exist in relation to figures 1 and 2. Orientation is given in figure 5 but would be more useful on figure 3.

Thanks for the reviewer's suggestions. We have added an inset map in Fig. 3a to show the plotted region. We also added the north direction in Fig. 3a.

Figure 3: Cannot see cyan contour on printed paper.

We modified the color of all the velocity contours into white color.

Figure 4: I can't distinguish between red and magenta contours.

We changed both colors in Fig. 3a, 3b, 3d, 3e.

Line 529: Case is wrong for Schafer.

Modified.

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