Geometric Controls of Fjord Glacier Dynamics - response to reviewers II

Frank, T., Åkesson, H., de Fleurian, B., Morlighem, M., Nisancioglu, K.H.

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1 Response to Anonymous Referee #2

We would like to thank the reviewer again for taking the time to review our manuscript a second time. We reply below by highlighting the reviewer's comments in italics above our answers. Changes made to the text are indicated under each of our answers in blue.

I think it should be pointed out more clearly that the expected fast retreat through a widening fjord only happens after an initial period of almost-stagnant retreat at the head of the embayment, i.e., clearly marking the two different phases seen in the numerical runs (similarly for the other cases).

Indeed, this is a very important aspect of our results! However, we feel that this point is already made in various places in the text. In fact, our entire analysis is centered around the question of why we see the stagnation before fast retreat occurs. For instance, sec. 3.1, 3.2 and 3.3 deal with exactly that question. Also in sec. 3.4, a specific reference is made in line 326. Furthermore, this is pointed out in the discussion in a paragraph dedicated to this point (line 394 - 402) as well as in line 410f and 425f. We therefore do not think it is necessary to change the text in this regard.

Instead of focusing on the somewhat arbitrary definitions of S and dS (whose equations should be explicitly stated), the authors should focus on the physics. In particular, I think what they observe is that widening at the head of the embayment → leads to thinning → reduces velocity in vicinity of GL & GL discharge → slows down retreat (similar arguments can be made for the other cases).

Note that these effects are non-local due to the elliptic form of the SSA equations. Some of this is hinted at in line 427 "Essentially, we thus describe a mass conservation mechanism where a smaller flux gate requires a smaller ice flux to maintain the same grounding line position." but I think the manuscript would be strengthened considerably if the authors made more effort to test this hypothesis (looking at figure S3 seems to confirm this though but the supplemental itself was not accessible to me).

With the brevity of the comment, it is not 100% clear what the reviewer refers to. But we will dissect the comment below as we understand it and respond to it accordingly.

1. "widening at the head of the embayment": In our understanding, head refers to the upstream side of the embayment and this is also what we believe makes more sense in the context of this comment. However, in the previous comment, head of the embayment referred to the downstream end. So we are not entirely sure what is meant here, but we will assume that the upstream end of the embayment is described.

2. "widening of fjord leads to thinning leads to reduced velocities and reduced GL discharge leads to slower retreat" (paraphrased from above): This chain of reasoning is, as we understand it, not consistent with our simulations. Indeed, a widening fjord causes thinning and reduced velocities but not reduced GL discharge, because the reduction in flow velocities is compensated by the wider flux gate. So GL discharge remains the same regardless of the width or depth of the fjord, unless another process (which, as elaborated in the manuscript, we believe is related to lateral and basal friction with the fjord) plays a role. If we interpret the comment correctly, the reviewer argues that a mass conservation argument is enough to describe the dynamics observed in our study, a point already made in the first round of review. However, as we discuss here, and as we have pointed out more extensively in the first response letter, this is not sufficient to explain our results.

3. comment on line 427: In light of the above remarks, we understand that this line may be confusing. We therefore suggest to remove it.

It would be better to talk about stress gradients instead of dS, dS is just a metric to measure these. Similary, it would be better to talk about cross-section or contact area, rather than S.
In the context of our experimental setup and the interpretation of the results, dS is by no means just a metric for stress gradients. In fact, dS is a geometrical property of the fjord, while stress gradients are a dynamic response of the glacier to various external drivers, one being fjord geometry. So rather than dS being a metric for stress gradients, dS is the *cause* of observed stress gradients. Therefore, we do not see how it is meaningful to regard the two as some sort of synonym. Regarding the wetted area S, we have elaborated in the previous response letter on why it is meaningful to define such a quantity in the context of our study, and what the differences are compared to the cross-sectional area of the fjord.

I think the authors should make more effort to clearly link their analysis to previous work: by focusing solely on flux per wetter area, this opportunity is missed, as they point out themselves: "glacier retreating on a prograde bed will experience a reducing wetted area as it recedes, and thus the ratio Q_{GL}/S may increase, but not the absolute grounding line flux". I suspect that most readers will not have an intuitive grasp of Q_{GL}/S.

We do not fully understand how the quote from the text cited here points towards a missed opportunity of putting our work in the context of previous studies. However, we agree with the reviewer that most readers will not have an intuitive understanding of Q_{GL}/S. Therefore, we suggest to add some further explanation in the text in line 391: However, the physical interpretation of the relationship between Q_{GL}/S and dS is not straight-forward. Since Q_{GL}/S is proportional to v_{GL} when there is hydrostatic equilibrium at the grounding line, the expression can be thought of as relating grounding line velocities to along-fjord changes in fjord topography, through the mechanisms described in our stress analysis (sec. 3.3). Accordingly, our results show that velocity evolution at the grounding line over time is also a good proxy for the dynamic response of a glacier to fjord topography (Fig. SA4). This may be specifically useful for less well-studied glaciers with unknown bathymetry. Notably, our geometric relationship is distinct from a typical mass-conservation argument, which simply states that velocities must increase for a decreased flux gate, and vice versa, to maintain the same grounding line discharge. This is because in such an argument, velocities are related to absolute values of fjord width or depth, and not the along-fjord change of fjord geometry. While mass-conservation mechanisms certainly play a role in our simulations, we do not find that such a relationship alone is sufficient to fully explain the dynamics we observe.