

Dear Editor,

thank you for your all of your comments and suggestions, in particular the request to improve the characterization of the mélange using a metric based on the coherence of feature tracking. This analysis has been very successful, supports our argument, and has greatly improved the manuscript.

We discuss below the points raised in this letter and on the marked-up pdf.

Given that this paper has been reviewed thoroughly both via Discussions and by yourself, we wonder whether it would be possible to avoid another round of reviews.

Regards,

Suzanne Bevan and co-authors.

Comments to the Author:

I have read your manuscript several times and I thank you for carefully addressing the reviewer comments. Unfortunately, I do not think your manuscript is ready for publication with The Cryosphere at this time for the following reasons:

1. While I remain open to the story presented in the manuscript - that warm surface waters weakened mélange, I do not see how this idea is supported in the manuscript as it is presented. You simply have no quantified metric of mélange rigidity to really prove your observations. A few times in the text you say that you “believe that warm surface waters weakened mélange”, but science must be more than belief - you must prove it. Other authors have used coherence of InSAR velocities in the mélange to quantify rigidity of mélange cover (Kehrl et al., 2018) or sea-ice fraction (Fried et al., 2018), or surface temperature (Cassotto et al. 2015) and so I recommend that you use one of these to really prove your conclusion.

*We have now included an assessment of mélange rigidity based on velocity mapping similar to Kehrl et al. (2017). This has worked really well and we are glad that you suggested it.*

*With regard to using SSTs or ice fraction products we have investigated the OSTIA sea ice fraction and SST products but they are of rather coarse (5 km) resolution to use in the actual fjord although they are adequate on the shelf. We therefore do not think they are a reliable evidence for conditions in the fjord itself.*

2. It is unclear why you focus on the most recent retreat as your conclusions could be strengthened if you also examined prior retreats (like 2004). If 2004 acted similarly then it would support your conclusion, but even if 2004 did not act similarly, it would allow you to put your results into some context. Perhaps you could also do this for the period in 1997 when no winter advance happened.

*Regarding the 2004 (actually early 2005) retreat we have now taken care to both highlight the temperature anomalies during this period and to refer to Christoffersen et. al. (2011,2012) work on this period. Please see P. 6, lines 25-28. Here we also refer to the lack of advance in 1996.*

3. Finally, I would like for you to revise the text to create separate Results and Discussion sections. I found it difficult to weed through to find your actual results.

*We have done as you suggest and hope this improves understanding.*

4. Other comments are in the attached pdf.

*We have considered all the comments on the pdf. Some suggestions we have adopted, and others have been addressed by including the feature-tracking based metric for mélange rigidity. Below we discuss the remaining comments.*

P3, L15. I'm not convinced that this is a significant observation. One CTD cannot be used to draw much of a conclusion about the water in the fjord as a whole. Additionally, the presence of AW may be driven by runoff and in the fall, runoff has dwindled and thus AW may be less present.

*We propose to keep this reference to the CTD profiles as CTD observations are commonly used to characterize water in the fjord in spite of their limited sampling. The second point is a good point and it supports our conclusion that it was unlikely that increased AW presence and temperature was a key factor in limiting winter advance. We have added text to make this point so thank you for highlighting it.*

P4, L26. Do you think it's rigid only in this one region to the side of the main glacier terminus? Is this sufficient to impact the glacier given that the compressive region is so small?

*With the added evidence of the feature tracking we have decided to remove this figure and reference to it.*

P7. It seems out of place to mention the elevation data so far into the text, when the figure is presented much earlier on. I might suggest re-arranging the presentation of data to keep elevation and velocity data along with Fig. 7.

*As we have now separated the Results from the Discussions we are able to describe and refer to the elevation and velocity data sooner in the text.*

I hope you do not take this decision to heart. I think with a little work you can really prove your conclusions and revise this into a widely-cited paper. I encourage you to do so.

*We hope you agree that the extra analysis has improved the paper and thanks again for your encouragement.*