We thank the reviewers for their time and helpful comments. We are very pleased that the manuscript was well-received overall: the study is “very relevant”, “generally well-designed” (reviewer 1) and “well thought-out” (reviewer 2) and the paper was “a pleasure to read” (reviewer 1) and “easy to follow” (reviewer 2).

We understand that, at this stage, we are not supposed to prepare our revised manuscript, but instead provide an initial response to the reviewers’ concerns. Here, we reply in detail to the reviewers’ major comments and explain how we will address these if we are asked to revise and resubmit our manuscript.

The minor comments from both reviewers primarily concern adjustments to the writing style and clarifications in places where the current exposition is unclear. In a revised manuscript, we would follow the vast majority of these recommendations.

Reviewer 1

Reviewer 1 had three major concerns. We discuss each of these in turn below.

1. <u>The choice of iceberg areal fraction lambda</u>

We thank reviewer 1 for raising the choice of the iceberg areal fraction lambda. We agree that a maximum iceberg-to-sea ice ratio of 0.35 (Foga et al., 2014) should imply a maximum total iceberg fraction of 0.26, which is significantly lower than the value of 0.6 used in our paper. Our short response to this is that in a revised manuscript, we will add sensitivity experiments that consider lower values of lambda, add a significant section discussing the sensitivity of melt rate and flux to lambda, and reconsider the comparison to melt flux observations accordingly.

In greater depth:

Notwithstanding some evidence for a higher value of lambda considered below, if asked to revise and resubmit the paper, we would run additional simulations with the standard temperature and mélange thickness profile with lower values of lambda. In light of these new simulations, we would extensively discuss the impact of lambda on the melt rate and meltwater flux. At this stage, we hypothesise that a lower value of lambda would lead to less meltwater flux and therefore less cooling in the mélange itself. However, there are also likely to be effects on the dynamics of water flow too, since fewer icebergs in the mélange would lead to less form drag (Hughes, 2022).

However, the reviewer comment also prompted us to look further into observational estimates of lambda. We found relatively few studies providing this information, but we do think there is some evidence for values larger than 0.26. Firstly, Foga et al. (2014) state that their object-based image analysis (OBIA) classification routine will result in the under-classification of icebergs in four distinct scenarios, which means that the lambda value of 0.26 derived from their work will be an underestimate. Secondly, Sulak et al. (2017) classified up to 39% of the total ice area in an ice mélange as icebergs, corresponding to a maximum lambda value of 0.39. Thirdly, Meng et al. (2024) produce elevation maps of ice mélange using a terrestrial laser scanner. Whilst they do not provide a lambda value explicitly, they calculate average mélange elevation near the terminus. Converting this average mélange elevation into an average mélange draft (assuming hydrostatic equilibrium), a back-of-the-envelope calculation suggests a lambda value of 0.26 would mean there would have to be icebergs with drafts deeper than the fjord itself to achieve this average mélange draft. In other words, generating sensible iceberg drafts from the observations from Meng et al. (2024) would require using a lambda value higher than 0.26. In sum, whilst there is certainly a lack of consensus

on a value of lambda, we do think there is (some) evidence in the literature for using values of lambda above 0.26.

2. Description of results

We thank the reviewer for pointing out additional patterns in our results which we have not discussed. If we are asked to revise and resubmit the paper, we will address these comments as follows:

i. The asymmetry in Fig. 4b (whereby there is a higher water velocity and temperature on the northern side of the fjord) suggests that the subglacial discharge plume is primarily travelling along the northern edge of the domain, most likely due to the specific configuration of icebergs in the mélange. This can be added to a revised manuscript.
ii. The asymmetry in the melt rate in Fig. 5b (whereby the melt rate is higher in the northern half of the domain) is likely due to the same reason of the plume preferentially travelling in this section of the fjord. This can be made clear in a revised manuscript.
iii. The spatial correlation between the melt rate and the meltwater flux in Fig. 5b (across the fjord, averaged along the fjord) which is absent in Fig. 5a (along the fjord, averaged across the fjord) can be understood by thinking about what drives the variability in melt rate. The primary variability in melt rate occurs along the fjord – further away from the terminus, decreasing ocean temperature and velocity combined with a thinner mélange lead to lower melt rates. However, in Fig. 5b, this along-fjord variation is averaged over, which allows the second-order variation to come through. This second order variation in melt rate arises due to the position of the plume (see point ii. above), which also influences the meltwater flux. Moreover, the short length-scale variation is due to the presence or absence of large, deep icebergs. These are submerged in deeper, warmer water, and so experience a higher melt rate and produce a higher meltwater flux. This explanation would be added to a revised manuscript.

3. Discussion: contents and structure

We thank the reviewer for their comments regarding the discussion. In a revised manuscript, we would address these as follows:

a. We agree with this suggestion and so we will move the limitations to the start of the discussion.
b. We feel that our study does reflect on buttressing indirectly, because mélange melt rate is a key control on mélange thickness, which in turn determines buttressing force. We will make this clearer and remove material from this paragraph that is more introductory in nature.
c. We will move the comparison of submerged areas into the methods. Additionally, we imagine this section will be revised once we have undertaken the simulations showing the sensitivity to lambda.
d. Thank you for this suggestion – we can comment on the use of our parameterisation within an effort such as ISMIP7. We feel that some additional steps would likely be needed – principally how to go from a mélange melt rate to a buttressing force. These steps could be outlined in a revised manuscript.

Reviewer 2

Reviewer 2 had two major concerns. We discuss each of these in turn below.

1. Iceberg abundance

We thank the reviewer for pointing out the importance of explicitly considering iceberg abundance in our discussion. Following the suggestion of reviewer 1 (see above), we plan to conduct additional simulations concerning the sensitivity to iceberg abundance and fully integrate these into the manuscript, particularly in the discussion. Thus, between those simulations and this request from reviewer 2, if asked to submit a revised manuscript we can add much more detail on the effect of changes in iceberg abundance on melt rates and fluxes.

2. Static iceberg geometries

Thank you – this is an important point and we agree this was not sufficiently clear. In a revised manuscript, we would make this clear in the methods section and bring it up again at the point where we discuss the potential influence of iceberg shape on the simulations.

Kind regards,

Lokesh Jain, on behalf of all authors