Response to Comment 1

Thanks for these comments, which greatly improved the paper.

This Short Comment focuses exclusively on correcting some inaccurate representations of our findings that we reported in Khazendar et al. (2019). We gratefully thank the authors in advance for their kind consideration.

- The authors incorrectly characterize one of the main conclusions of our study in Lines 276-282: “Maximum melt rates for 2012 to 2015 are estimated to be ~8-10 m/yr for concentrated plumes with limited spatial extent, or about a factor 3 less if the melt water emerges with a uniform distribution from beneath the grounded ice (Khazendar et al., 2019). Whether concentrated or evenly distributed, the factor-of-3 reduction should roughly represent the average melt rate across the terminus. Thus, scaling the plume rates from Khazendar et al. (2019) by a factor of 3 yields approximate average melt between ~3.5 m/yr during the summers with warmest water and ~1.9 m/yr during the summers when the water was coolest.” In their argument, the authors take our values of maximum melt rates and translate them to lower mean rates.

Aside from our typo (m/yr in place of m/d – force of habit), we stand by our statement, which is fully consistent with the results stated in Khazendar et al., 2019. We did work, however, to make the language a bit more precise, as in fact we overestimated the average melt for the plume case in the earlier draft. If we assume that the plume is 100-150 m wide, and average the 10.5 m/day maximum rate across the ~4-km –wide terminus (not in depth), then the average is < 1 m/day (we didn’t compute the exact rate because it depends on the exact width of the plume and the terminus width varies – but these example values yield a width average of ~0.4/day, so saying < 1 m/day is appropriate).

We also now make clear we are referring to width-averaged, not depth-averaged rates. It is important to note that our interpretation of what is said in Khazendar et al. is that the maximum melt rates apply to a single narrow plume (From Khazendar et al “A point-source subglacial plume at the front of Jakobshavn is modelled using ocean temperature data collected in 2019”). So, one gets the most bang for their subglacial melt volume with a uniform distribution, which yields a width-averaged maximum rate of 3.5 m/d after apply the scaling factor provided by Khazendar et al (“During summer, if the subglacial discharge is evenly distributed across the width of the terminus as a line plume, instead of emerging from a single subglacial conduit, melting rates are reduced by roughly a factor of 3”). To the best of our knowledge, these numbers follow directly from the numbers and assumptions in Khazendar et al., and this is consistent with other plume studies in the literature. For example, experimenting with the simple model of Xu et al, 2013, it’s clear that the

maximum total melt is achieved when the melt emerges as a uniform line plume. Taking all of this into account, 3.5 m/day is the maximum melt rate in width-averaged sense based on the Khazendar et al results , which, as we point out, is small relative to a 45 m/day advance. Even a concentrated melt rate of 10.5 m/d is small relative to the advance rate. Moreover, this rate applies to a narrow plume that would be partly offset by the bridging effects of the nearby ice (e.g., for 100-m wide tunnel in a 4-km wide ice front). The now cited Todd et al papers strengthen our assertion.

Yet, we opted to present the maximum melting rates purposefully.

We realize that, but we disagree with that decision, and our statements provide relevant context in which to interpret those numbers. It is disappointing that the melt rates presented in Khazendar et al are not more clearly identified as melt rates for one narrow (100-m scale) location in an ~4000 m ice front (one needs to read the Methods and some of the cited papers to fully appreciate that).

For deep glaciers such as Jakobshavn Isbrae, ocean-induced melting at the front tends to reach its maximum value within ~100 m of the grounding line up the face of the glacier (Carroll et al., 2016).

Yes, but the single plume modeled in Khazendar et al. is most likely to produce a narrow cleft in a wide terminus, as noted above. To make the point clear, we now state the following:

"Maximum melt rates for 2012 to 2015 are estimated to be ~8-10.5 m/d for a concentrated plume with limited spatial extent (~100-150 m) at the terminus of Jakobshavn Isbrae, which when averaged across the width of the terminus face gives a mean rate of <1 m/d (Khazendar et al., 2019). Due to the non-linear relation between melt and subglacial melt discharge (Xu et al., 2013), maximum aggregate melt should be achieved when the subglacial melt emerges uniformly from beneath the terminus. In this case, melt rates are about a factor 3 less than the corresponding plume rates (Khazendar et al., 2019), yielding a maximum width-averaged rate ~3.5 m/d during the recent warm period. Similarly, the maximum width-averaged melt is ~1.9 m/d during cool periods, based on an ~5.7 m/d maximum plume rate. Note all rates reflect the maximum rate at some depth, so the depth-averaged rates should be somewhat smaller (Carroll et al., 2016; Khazendar et al., 2019). It is also important to note that much of the oceanic heat in the fjord goes into melting icebergs (Moon et al., 2018), so these values may be biased high. During the summer, the terminus advances at ~30-45 m/d, so that ice is replenished far faster than it is removed via submarine melting (<1–3.5 m/d) (Joughin et al., 2012a). "

While there is a different emphasis, we believe this is an accurate reflection of the results presented in in Khazendar et al. We are happy to correct any factual errors in this statement.

This enhanced melting has been observed to produce widespread undercutting of the glacier fronts (Fried et al., 2015; Rignot et al., 2015), which could lead to increased calving, frontal retreat and reduced resistance to flow.

These papers do demonstrate some undercutting, but do not demonstrate an accelerated rate of calving (Rink and Store are two of the more stable glaciers in Greenland). Moreover, the glacier examined by Fried et al. is moving an order of magnitude slower than Jakobshavn, so the undercutting is of more comparable magnitude to the ice speed.

Observations and theoretical work have suggested that calving can be a direct response to undercutting at the front (Bartholomaus et al., 2013; Luckman et al., 2015) and that submarine melting and undercutting can contribute to calving that is several times the melting rate (O'Leary and Christoffersen, 2013; Benn et al., 2017; Todd et al., 2018).

The Luckman et al. paper references a glacier where the terminus speed is comparable to the ablation rate. They acknowledge that this process likely dominates at slower glaciers rather than faster glaciers. The Bartholomaus et al. paper is for a slower glacier in much warmer water and a much shallower terminus subject to fairly different calving dynamics. And despite melt rates of 9–17 m/d (average), the glacier is advancing. While a nice piece of work, the O'Leary and Christoffersen paper uses a 2-D model and focuses on the shifting of the stress concentration inland, which does not necessarily increase calving (especially for the near-flotation case). We did add a reference to this paper. The more realistic cases that Krug et al. and Todd et al. model show relatively modest sensitivity to melt, and a far greater sensitivity to mélange. When the mélange is included in the Todd et al. model, plume melt rates, for the most most part, are not significant until they reach very large rates of ~24 m/day. In response to this comment and those by the other reviewers, we added the following to make these points:

"While we cannot entirely rule out melt serving in some way as a "catalyst" (e.g., by undercutting the front) to influence calving, a shift in average melt rate from 3.5 to 1.9 m/d (e.g., average melt decreased by 1.6 m/d) over a few months of the year should not drastically slow the rate of retreat and speedup for a glacier that moves at 30-45 m/d. For those glaciers where undercutting has been observed to have a substantial effect, the melt rate is comparable to the terminus advance rate (Luckman et al., 2015), unlike the case for Jakobshavn Isbrae where width-averaged melt rates are an order of magnitude slower. While a 2-D model does suggest that even modest undercutting may have some effect (O'Leary and Christoffersen, 2012), the main effect for cases near flotation is to shift a relatively weak, broad stress peak inland. A more complex time-dependent model that includes calving with damage indicates that the effect mélange on seasonal variation in terminus position and speed is far greater than that of melt undercutting (Krug et al., 2015). Neither model accounts for basal crevassing, which can be important for calving near flotation (Van Der Veen, 1998). In a full 3D model that includes both basal and surface crevassing, plume melt rates of 12 m/d in combination with uniformly distributed melt rates 3.1 m/d produce little seasonally enhanced calving (Todd et al., 2018). It is only when plume melt rates are increased to ~24 m/d that is there a substantial effect for a glacier flowing more slowly (12–14 m/d) than Jakobshavn Isbrae (Todd et al., 2019). As with the 2-D model (Krug et al., 2015), the 3-D model produces a pronounced variation in terminus position and speed in response to seasonal mélange forcing, consistent with our observations."

Therefore, rather than scaling our quoted melt rates down, it is more likely that the melt rates should be scaled up to represent their potential impact on the calving rate.

> ***Please see arguments above that address this issue. In summary, we scale only using the Khazendar et al. supplied factor and average results across the terminus to compare the rates with the terminus advance rates.***
>
> ***The commenters' argument suggests that we should interpret the referenced papers as showing that undercutting is the dominant factor in calving of Jakobshavn Glacier, and that we should scale the results in Khazendar et al by an arbitrary factor to demonstrate that this is so. We are reluctant to engage in this kind of circular reasoning, particularly because our reading of those papers has not provided a convincing argument for the commenters' premise.***

Furthermore, we emphasized in our paper that while we found that the glacier's thickness changes had a strong correlation with ocean temperature variability, the former was even more strongly correlated with the variability in submarine melting rates.

> ***Since we are both arguing for processes modulated by the ocean temperature, we don't dispute the correlation. But unlike the annual spring elevation data in Khazendar et al., our conclusions are based on much more temporally dense elevation data. These data indicate that thickening primarily commences during winter, when the mélange processes come into play.***

- The authors then continue on Lines 289-291 with another argument to justify rejecting the relevance of submarine melting: "Although submarine melt should have been substantially reduced in the summer of 2016 (Khazendar et al., 2019), the maximum retreat was virtually identical to that of the four prior years and speeds were only slightly reduced, suggesting melt is not directly controlling retreat."

This is a mischaracterization of our data and approach.

> ***This statement is meant to characterize our data, not those of Khazendar et al, and does not include any mention of their approach. Nonetheless it is not inconsistent with the data presented by Khazendar et al.***

It is clear from the Davis Strait mooring data (Figure 3 in Khazendar et al., 2019) that cold water started arriving in Disko Bay in June or July of 2016, and in fact Figure 2 of Joughin et al. shows that glacier speeds in 2016 at Tmax-1km, M6 and M9 all experienced the smallest increases between the spring minimum and the summer peak than any other summer since 2011. Our Figure 3 shows the same.

> ***We state:***

"In contemplating whether submarine melt, particularly in summer, might drive the observed retreat and speedup, it is important to consider the relative timing of the recent changes. As

Figure 6 shows, the colder water first appears in the fjord in the summer of 2016 (Khazendar et al., 2019). While the summer 2016 speeds are moderately slower than prior summers (2012–2015), this decline is not far off a general trend of declining summer peaks following the summer of 2012. During the period when the position of maximum summer retreat was relatively stable (2012–2016), the declining summer peaks were likely a consequence of the evolving geometry that shallowed slopes over time in the near-terminus region (Fig. 3). It is also important to note that to the extent that submarine melt may influence speed, it is through changes in the calving rate that influence terminus position, which then alter speed (e.g., Fig. 5). Since the position of minimum retreat is virtually the same in 2016 as in the prior four summers, the cooler water does not appear to have suppressed calving that summer."

> ***Our main point was that things really begin change in the winter of 2016/2017, when there is a strong terminus advance.***

As we state in our paper, the ocean properties and subglacial discharge volumes we use in calculating submarine melting rates are from the summer of each year, while the thickness changes of Jakobshavn are from the following spring, when the altimetry data were acquired. Regarding the year 2016, this is what we stated: "Most prominently, the sharp drop in ocean temperatures in 2016 and 2017 by 2 °C relative to the peak temperature in 2014 corresponds to the slowing and dramatic thickening of the glacier in 2017 and 2018." Indeed, our observations (Fig. 3 in Khazendar et al., 2019) show that the flow speed of Jakobshavn starts a significant slowdown in the summer of 2016, around the time of the observation of the large drop in ocean temperatures and submarine melting rates. The glacier then reaches its slowest flow speeds in the spring of 2017, coinciding with our measurement of significant thickening. The flow speed then stages only a weak recovery in the summer of 2017. This pattern is also shown by the data in Figure 2 of Joughin et al.

> ***Our text does not contradict that interpretation, except that we feel for the most part the slowdown commences in winter 2016. As noted above, the summer 2016 was only modestly slower. Moreover, the retreat, which governs speed, was identical to that of the summers with warm water (see revised text above).***

Our rendering of the events and their relative timing is consistent, so we request that authors remove the text on Lines 289-291 in its current form as it misconstrues our findings.

> ***Our statement was : "Although submarine melt should have been substantially reduced in the summer of 2016 (Khazendar et al., 2019), the maximum retreat was virtually identical to that of the four prior years and speeds were only slightly reduced, suggesting melt is not directly controlling retreat."***
>
> ***We disagree with the commentors' assertion that this statement misconstrues their findings. We offer the following justification:***
>
> ***1) Figure 3 of Khazendar et al shows a reduction in melt for summer 2016***

2) Our data show the minimum retreat is similar to that in the previous few years. The only point in question is whether the slowdown actually began in summer 2016, and we have a different interpretation – we have added some text to make our point more clear (see above).

As an aside, we note that during the 5-year period with similar terminus extent, the second slowest observed summer speeds occurred during 2014 (about 0.25 m/d slower than 2016 in Fig 3 from Khazendar et al). Yet this is a summer of maximum melt rates (approximately equivalent to 2012 melt rates – the year with fastest observed summer speeds). So, in terms of speed, the correlation with melt rates is weak.

We feel our statement and interpretation are valid, and acknowledge that there is room for continued debate and a need for continued observation and analysis.

More generally, we aimed to be careful in framing the conclusions of our study as not to claim that ocean temperature variability and submarine melting are the sole explanations of Jakobshavn's dynamic evolution. We wrote that we "find the evidence sufficient to conclude that ocean temperature variability, through its influence on submarine melting rates, has been a main, and sometimes dominant, factor in shaping Jakobshavn Isbrae's interannual dynamic evolution since the disintegration of the ice shelf in 2003." We feel this conclusion holds without us having to dismiss the possibility that other processes might also have had a role in shaping the evolution of Jakobshavn. We dedicated parts of our paper (both in the main text and the Supplementary Info) to a discussion of those other potential influences.

And in our paper, we argue that the mélange is likely the "main, and sometimes dominant" forcing that controlled Jakobshavn's behavior over the last decade. And we too did not rule out other processes, such as melt.

- Finally, the authors acknowledge on Lines 284-285 that they ". . . cannot entirely rule out melt serving in some way as a "catalyst" (e.g., by undercutting the front) to accelerate calving, . . ." Other statements in the manuscript, however, read as if the role of submarine melting, as presented in our study, has been entirely and conclusively ruled out.

We feel that our approach is more productive in advancing the scientific debate than was the treatment of mélange in Khazendar et al. There, the only reference to mélange is: "The roles of ice mélange on interannual timescales[4,34–36], and that of cryo-hydrologic warming[37,38], have yet to be elucidated." In fact, a number of papers had, at the time, explored the importance of mélange in controlling calving and glacier advance (e.g. Todd et. al, 2018, and Krug et al, 2015). While the commenters may not agree with our opinion, we feel it is important to continue the open discussion on the relative importance of the different mechanisms.

Such statements appear on Lines 20-21, 274-275 and 330-331. In light of our responses above, we ask the authors to consider either a) providing evidence that justifies those statements, b) adding nuance to those statements to reflect the fact that the conclusions of our study have not been refuted here, or c) simply removing the parts of those statements that concern our study.

A) We improved our arguments as described above.

B) We are not "refuting" the conclusions of Khazendar et al. We presented a different hypothesis. This process is fundamental to how science works, and ongoing observation and analysis will reveal which hypothesis (or some combination) is correct. Our paper is very carefully worded to make clear we are hypothesizing with justification based on observations and the literature (26 instances of "may", 9 instances of "appears", 8 instances of "suggest", 16 instances of "likely"), indicating due diligence with respect to ensuring our points are nuanced.

C) We reworded several statements as described above.

With thanks and best wishes to all,
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