SUMMARY

The authors have addressed each of the comments from my first review. I particularly appreciate the discussion that they have included to show that, in their model, the ice mélange evolves quite quickly, which suggests that it can be treated as being approximately in steady-state. However, the new figure that they added (Fig. 3) raises some concerns that I did not identify previously. In their model, once the ice mélange thins to 0 at the mouth of the embayment it is no longer capable of affecting the calving rate, even if the ice mélange is a couple of hundred kilometers long! I don't see why that should be. Most of the studies that suggest that ice mélange can affect calving are from Jakobshavn Isbræ. There, the ice mélange rarely extends beyond 15–20 km in length, whereas the fjord into which the glacier flows is ~50 km long. I think the issue is with Equation 4,

$$d_{cf} = \beta d_{ex},$$

which states that the ice mélange thickness at the terminus is proportional to the thickness at the end of the embayment. Once the thickness at the end of the embayment goes to 0, the thickness at the terminus also goes to zero. Essentially the ice mélange is pinned to the end of the embayment, and as a glacier retreats it gets stretched thinner and thinner (although the thinning is offset some by increased iceberg production). This seems to be a pretty serious issue that should be addressed as it affects all of the subsequent interpretation. Why should the ice mélange have to extend to the end of the embayment?

A couple of other general comments:

- Perhaps worth discussing in a few sentences why you adopt a continuum modeling approach for ice mélange, and how this is motivated/justified by attempts in the granular mechanics community to develop continuum rheologies for granular materials.

- This paper seems to be motivated by the observation that some calving parameterizations don't seem to have an upper limit on calving rates and can produce very high and unrealistic calving rates. I think we need to ask if there is something fundamentally wrong with the physics of those calving models. Although I generally like the approach taken here and do feel that ice mélange can reduce calving rates, I would suggest putting less emphasis on trying to fix those models with an "ice mélange bandage".

SPECIFIC COMMENTS

P2, L5–17: These two paragraphs are kind of choppy.

P6, L4–9: Some of these variables were already defined on the previous page.

P6, L12: Perhaps say that m is the average melt rate instead of assuming that it is spatially constant?

P7, L4: Suggest "Assuming a viscoplastic rheology and quasi-static flow"

P7, Eq. 4: This equation is where my concerns start. It indicates that the ice mélange thickness at the calving face is zero if there is no thickness at the mouth of the embayment. (See summary comments.)

P7, L16–20: I think this paragraph could more clearly state the implications of this model. Essentially, if the calving rate is low, ice mélange will have little impact on the calving rate because the ice mélange doesn't become expansive enough. Only when the calving rate is high does ice mélange

become important. I'm not sure if that is physically correct—it could be—but at any rate it does seem to be a feature of this model.

P11, L8 & L22: ma^{-1} should be m a^{-1} (milliyears vs. meters per year)

P15, L16–17: This is confusing, since in Section 3 you have argued that the ice mélange quickly adjust its geometry. Why not just use the adaptive approach?

P18, L1: I think you can express the "position-based" calving parameterizations in terms of rates that depend on the thickness gradient. I'm not sure if that is written up anywhere, but my point is that you can probably use this framework for other types of parameterizations than what you have considered here.