I have now read the revised manuscript, and would like to thank the authors for their careful responses to my review. I have a few remaining and related concerns/suggestions related to melt vs. draft figures.

1. I think that one needs to be very careful when interpreting the variations in area-averaged melt rate with draft presented in Figures 4-6. The crux of this problem is that the submerged iceberg areas and drafts are unknown and difficult to determine. I think the approach of calculating a mean draft makes sense, and actually you can show that Equation 1 is valid for any iceberg in which the submerged volume of ice lies below the horizontal cross-sectional area of the iceberg at sea level (i.e., not just for the cylindrical geometry that the authors assume). The bigger issue is that calculating an area-averaged melt rate requires making an assumption about the iceberg geometry. Enderlin and Hamilton (2014) addressed this issue by considering both cylindrical and cone-shaped icebergs, and found that the assumed geometry affects the area-averaged melt rate by about 10%. Thus it would seem that the assumed geometry doesn't affect the results too much, although I'm not entirely sure if that's true. What I'm having trouble with is that, if you assume that the icebergs are always cylindrical, then you are in essence assuming that the submarine melt rate doesn't vary with depth — and that makes it dificult to see why the area-averaged melt rate should depend on draft... I wonder if it would make more sense to plot the fractional rate of volume change (1/V * dV/dt) vs. draft as that would also be useful for assessing how freshwater fluxes vary with depth and wouldn't require any assumptions about iceberg geometry.

2. Related to item (1), some of the individual data points in Figures 4-6 are based on very small statistics, which makes it difficult to assess the significance of the trends. It may be nice to have a table or figure that somehow illustrates the size distribution of the icebergs that were analyzed, or somehow modify the figures to indicate how many samples are included in each data "bin". A sentence or two in the text may also suffice.

3. The dips in melt rate observed at Upernavik and Jakobshavn (Fig. 3d-e) are within the error bars of the adjacent points, and so the dips in melt rate may not be significant. That should be made clear in the text.

4. Some of the sharp changes in area-averaged melt rates seem counterintuitive. For example, the rapid increase in melt rate at Koge Bugt (Fig. 3h) between 210 and 290 m depth would seem to indicate that the average melt rate over that depth range is something like 2.5 m/d, or more than an order of magnitude larger than the average melt rate over the upper 210 m. The same could also be said for the increase in melt rate between 140 m and 170 m depth at Jakobshavn Isbrae in April 2011 (Fig. 5). Is it plausible to have such sharp changes in melt rates over these distances?

5. I feel much more comfortable with the general trends presented in Figure 3, which seem to be more statistically significant. Figure 3a is especially nice.