Many thanks to the editor for these comments, which have helped us to improve the manuscript. We have described the changes made to the manuscript below (editor's comments in black, responses in blue).

Throughout the manuscript, the terms "seawater infiltration" and "brine infiltration" appear to be used interchangeably, e.g., lines 5/29 vs. 5/30. I find the former more evocative (and still accurate), but the latter is in more common use in the literature. If they are indeed referring to exactly the same phenomenon, and I believe they are, then only one such term should be used (I personally prefer "seawater").

This question has caused a lot of discussion amongst the authors as well! We have updated the text to use "seawater" to describe the initial infiltration process, but have retained the use of "brine" to describe the water existing within the firn, as it will have changed its salinity due to freshwater freezing after the initial infiltration process.

Abstract: The abstract could be improved in a couple of places. In particular, the term "variable" is not quite right, as it places the emphasis more on its use in a model than the "presence of liquid water", which is better characterized as a "physical property" of the ice column than a "variable". Separately, the last two sentences are written in the passive voice and could be more direct, e.g., "We find that seawater infiltration…" and "Finally, we identify the most likely locations…" Suggested changes have been made.

2/23: I understand the addition of additional DEMs as a response to reviewer #1, but some of them are now based on older data, e.g., Bamber et al. (2009) uses a combination of ERS-1 and ICESat data. Further, I was surprised to see that Helm et al. (2014, TC), which uses CryoSat-2 data, was not included, as it is publicly available and uses next-generation radar-altimetry data as compared to Bamber et al. (2009) or Liu et al. (2015). Further, since the Bedmap-2 surface DEM is – as best I understand it – intended to improve upon Bamber et al. (2009), that older DEM appears deprecated relative to Bedmap-2. Please reconsider which surface DEMs are most essential to include in this study. If the Bamber et al. (2009) DEM is kept, then it might be better to refer to it as ERS-1/ICESat to keep it more consistent with the other shorthand names.

We hadn't come across the surface DEM produced by Helm et al. (2014) before, but it is certainly a useful addition to the collection. We have replaced Bamber et al. (2009) with data from Helm et al. (2014), and updated the relevant tables in both the main manuscript and supplementary material. This has produced a small change in the mean brine zones, with a maximum absolute change of 0.4% total ice shelf area.

3/5: Aren't "error estimate" and "bias" simply uncertainties? In particular, a bias is not really a bias if its sign is unknown. If a single term can be used for all of the above, please do so.

We have slightly adjusted this sentence to improve clarity. The firn depth uncertainty is a simple uncertainty combined from all of the model inputs. The Bedmap2 uncertainties are provided as RMS random scatter in the data points (<15 m) and systematic bias (<5 m). Of these, only the systematic bias should have a significant effect on the calculated brine zones, but this bias will differ depending on location. Interpolation on ice shelves with sparse data points tends to produce a positive bias. The presence of marine ice, or higher than expected ice density will also produce a positive bias. However, divergence from hydrostatic equilibrium near the grounding line will give a negative bias. Rather than assume which effects dominate in which region, we applied the potential bias in each direction to encompass the two potential extremes.

4/13-14: Admittedly, the nomenclature gets tricky, but aren't "laterally unconfined ice shelves" simply "ice tongues"? I can't see from Figure S3 the names of these particular regions in detail, but if the local ice features at the mentioned coasts are already termed ice shelves, then this comment can be ignored.
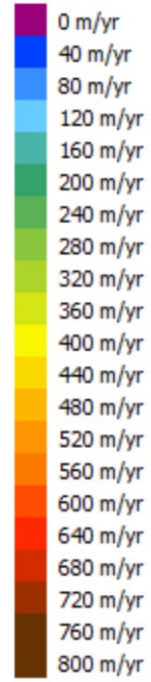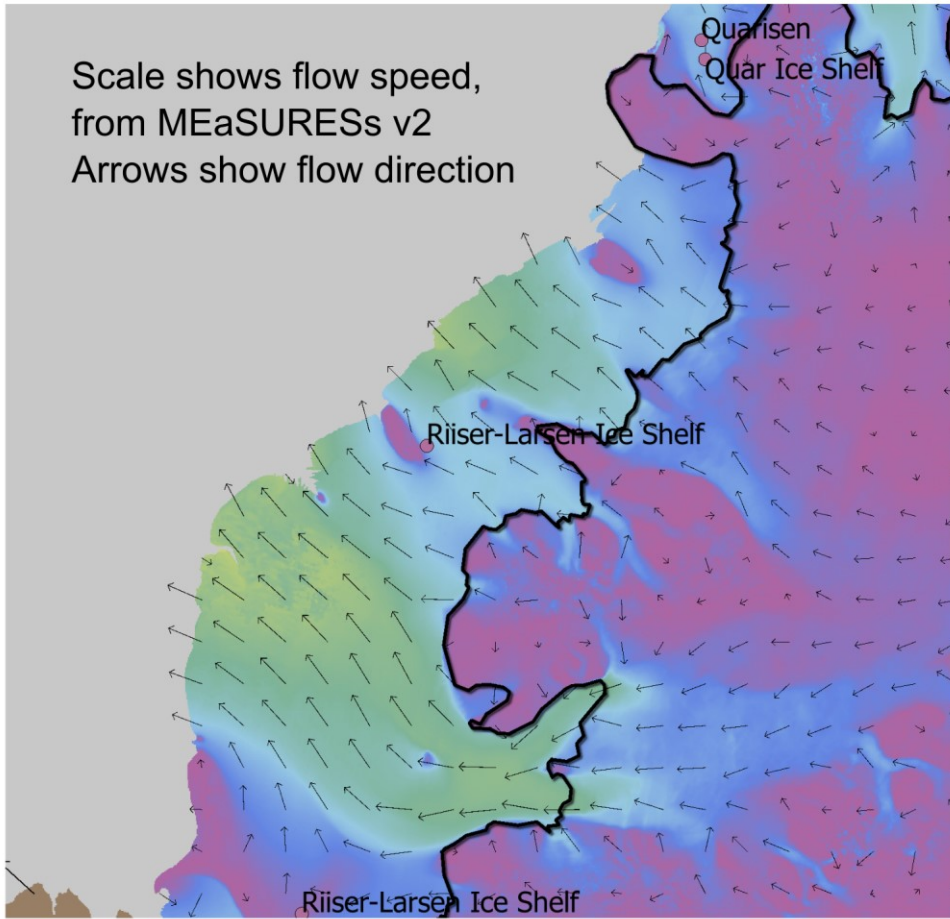
This could have been phrased better. The ice shelves here aren't ice tongues, but they are in shallow embayments, so they have more lateral divergence than ice shelves which are more strongly confined (see attached figure). The sentence has been rewritten to make this more clear.

4/24: This is an interesting insight and useful context, but it's not made clear what the root cause of this model/observation alignment is. Based on the Supplementary Figures, in particular that for ice thickness, I suspect that an ice-thickness gradient there is the underlying cause for the different in predicted infiltration extent. Is that correct? Either way, further explanation would be appreciated.

We have added the following sentence: "This area of Larsen C experiences high rates of longitudinal spreading, which is likely the cause of both the rifting and the presence of brine, but it is unclear how far brine in the firn layer might affect the growth of the rifts."

5/22: IceBridge: This has been corrected

Scale shows flow speed,
from MEaSUREEs v2
Arrows show flow direction

Quarisen
Quar Ice Shelf

Riiser-Larsen Ice Shelf

Riiser-Larsen Ice Shelf

Princess Ragnhild Coast

0 m/yr
40 m/yr
80 m/yr
120 m/yr
160 m/yr
200 m/yr
240 m/yr
280 m/yr
320 m/yr
360 m/yr
400 m/yr
440 m/yr
480 m/yr
520 m/yr
560 m/yr
600 m/yr
640 m/yr
680 m/yr
720 m/yr
760 m/yr
800 m/yr