Dear Editors and Reviewers,

thank you for your positive feedback on our manuscript. We found the comments constructive and insightful. The main changes applied in the revised version pertain to (1) a clearer layout of our scientific motivation including an overhaul of the language, (2) clarification on some aspects in the methods, and (3) revisions of Figs 1, 2 & 7.

The conclusions remain unchanged. Below, all remarks are answered in detail. We found that the applied changes have improved the paper and hope that this is perceived by you in the same way.

On behalf of all coauthors,

Vjeran Višnjević

RV2-1: Summary: This work uses a simple, observationally driven ice flow model to forward model ice shelf stratigraphy with a given atmospheric and ocean scenario. The method is validated with Elmer/Ice model. The model predictions are then compared with radar observations over Roi Baudouin ice shelf. The internal layers in the LMI region resolved by the radar are compared with the model-derived internal layers. As the ice shelf model uses a steady state assumption, this is a way to predict if an ice shelf is in steady state if the mode predictions agree with observations of internal stratigraphy.

Thank you for your review. We have implemented many of the suggested changes detailed below.

Major comments:

RV2-2: This is an important concept. The transition of LMI and CMI and the percentage of the ice shelves that comprise of LMI/CMI component can have important consequences for ice shelf stability. However, the paper is not very clearly written, in my opinion. The authors need to take another look at the sections to improve readability.

Thanks for pointing this out. We implemented a number of structural changes also mentioned by RV1 including revision of abstract, introduction and methods (RV1 3-6 among others).

RV2-3: The white LMI/CMI boundary in Figure 7- I assume it is modeled. Is there a possibility to provide an uncertainty in the depth of the LMI/CMI layer?

In the initial and the revised versions, we quantify aspects of the uncertainty (cf. **RV1-38)** ***pertaining to the LMI/CMI boundary including one (1) numerical diffusion (Sect. 3.2), and (2) now more explicitly flaws in the input forcing (they propagate linearly into the calculation of vertical velocities (Eq 2) and in return***

the calculated age (Eq 1)). Compared to those major points, other uncertainties (e.g., wrongly observed surface velocities or geometries) are minor.

We added a sentence:
" Future changes, but also errors, in the accumulation and melt rate fields will propagate linearly into changes in the position of the LMI/CMI boundary (Eqs. 1&2)"

RV2-4: Why only Roi-Baudouin was chosen for validation? Was any other ice shelf considered for the validation with airborne radar? Is Ross Ice Shelf not a good candidate for comparison?

We focus on presenting our methodology, show synthetic experiments, and apply the method to one real world ice shelf. In future work we plan to apply this method to all ice shelves around Antarctica, and will the also provide the climatic and oceanographic context of the individual ice shelves.

RV2-5: The presence of marine ice may be sporadic on some ice shelves, but extensive on others (example the Ronne-Filchner ice shelf). The limitation of this method needs to be acknowledged, particularly along lines 234-240.

This seems like a misunderstanding. The presence of marine ice will ideally be included in the basal melt rate fields and the dynamic effect will be included in the surface velocities. Therefore, there is no fundamental problem with and as Eq (2) in this case.

RV2-6: Figure 10 needs a panel of basal melt rates from Adusumilli et al. The comparison of LMI/CMI composition with basal melt rates will be interesting and important. For example, does the basal melting pattern differ considerably on either side of the ice shelf?

Such a figure was already included in the initial version. Basal melt rates, surface accumulation rates and velocities are displayed in Figure 1. In the Discussion section (Sect 4.3, L307) we state that the recovered LMI/CMI pattern mainly reflects the velocity field.

Minor comments:

RV2-7: How do the upstream grounded ice surface look like in Figure 7? Are there crevasses, blue ice etc. that would prevent the identification of layers below the LMI/CMI interface? A figure delineating the possible surface conditions would be helpful.

Stokes et al. (2019) report a widespread distribution of supraglacial lakes, as well as blue ice (Matsuoka et al., 2018), in the area upstream of the ice shelf.

Other studies also report on the lack of internal layering. Callens et al. (2012) report that there is no internal layering on the tributary Ragnhild Glacier.

We added:" In this specific example, this can be explained with surface melt water infiltration in austral summers as well as with the existence of supraglacial lakes in the area upstream of the ice shelf (Stokes et al., 2019)."

<u>References:</u>

Matsuoka, K., Skoglund, A., & Roth, G. (2018). Quantarctica [Data set]. Norwegian Polar Institute. https://doi.org/10.21334/npolar.2018.8516e961

Stokes, C. R., Sanderson, J. E., Miles, B. W. J., Jamieson, S. S. R., & Leeson, A. A. (2019). Widespread distribution of supraglacial lakes around the margin of the East Antarctic Ice Sheet. Scientific Reports, 9(1), 13823. https://doi.org/10.1038/s41598-019-50343-5