**Response to Rene Forsberg, Referee #2** (https://doi.org/10.5194/egusphere-2025-5745-RC2)

Thank you very much for the kind and supportive review of our manuscript. We greatly appreciate the detailed comments and suggestions, which are very helpful and will improve the manuscript, in particular the discussion.

In the following, we respond to the comments and outline how we plan to revise the manuscript. Please find the referee comments in blue italics and our responses in black.

*This is a major effort highlighting the problems of getting precise volume change of Antarctica from laser and radar altimetry. The paper highlights the large difference between IceSat-2 and CryoSat results in the central region of Antarctica, during the common overlap period 2019-24, and finds significant lower values for CryoSat for a range of retrackers and slope correction parameters for CryoSat. The paper also highlights for the same reason that error estimates of current compilations of Antarctic mass changes, especially IMBIE, has too optimistic error estimates.*

*The computations of the paper are all based on known methodology, with very detailed statistical and intercomparion analysis (at times too detailed, making it hard to read and get an overview through the results section). The paper includes comparisons to expected elevation changes from a single SMB firn model (GEMB), which highlight regions of major elevations changes that have a better fit to IceSat-2 than CryoSat, illustrated by useful figures. The paper is therefore an important contribution to improving the determination of Antarctic elevation changes by satellites, and definitely should be published.*

**Major comments**

*With this said, obviously some remarks could be added in text: First of all, the volume changes are based on samples averaged over large footprints from tracks of various spacing, and further interpolated into various grids. In terms of resolution, IceSat-2 have an important advantage over CryoSat, which furthermore needs a challenging retracking and slope correction, both prone to errors. It is also important to note that the two satellite missions are not measuring the same surfaces: CryoSat radar penetrates deep into firn, and attempts to "recover" the actual snow surface through retracking, a near-impossible task requiring knowledge of firn properties (this situation will be improved with the upcoming ESA Cristal mission). In the paper IceSat-2 is used as "ground truth", but even IceSat-2 could miss sampling of large areas of Antarctica due to the repeat orbit, and the snow surface measurement might be affected by sastrugi and windblown snow. So the paper could be a good catalyst for more validation efforts in special areas of Antarctica, e.g. megadune areas.*

Thank you for these very valuable remarks and suggestions. We agree that these aspects deserve clearer discussion in the manuscript and we will consider all of them. We will therefore revise relevant parts of the Introduction and, in particular, the discussion of potential trend error sources in Section 6.5 to more explicitly address the differing sampling and spatial resolution and the uncertainties of both CryoSat-2 and ICESat-2 measurements.

Thank you for drawing our attention to potential uncertainties in the ICESat-2 measurements and elevation change results. As discussed in our response to Anonymous Referee #1 (major comment 2),

we will also revise our wording regarding ICESat-2 as “ground truth” and discuss the ICESat-2 uncertainties more comprehensively.

We further appreciate the remarks regarding future improvements through the upcoming CRISTAL mission and the need for additional validation efforts in specific Antarctic regions such as megadune areas, and we will include these in the revised manuscript.

**Technical comments**

*I would suggest to add the word "Interior" to the title. The paper only treats the Interior Antarctic (CryoSat LRM zone).*

We agree and will revise the title accordingly.

*It would be really interesting to do a similar comparison including the coastal zones with SARiN data, I guess then the altimetry methods would agree better.*

This would indeed be very interesting and would provide additional insight into the agreement and disagreement between both altimetry methods in coastal regions. We hope that future studies will further investigate this aspect.

*On page 2: CryoSat has an approximative repeat period of 369 days, but note the orbit was changed in the period due to the IceSat-CryoSat orbit alignment.*

Thanks for this information. It will be included.

*On page 5: The CryoSat resolution is quoted at 1.65 km. This could be misunderstood, the effective beam-limited footprint resolution used in the computations is much larger.*

We will clarify this in the revised manuscript and additionally include the beam-limited footprint resolution.

*Overall figures and plots are very informative. It would be really useful to add a plot with all basin names before the expensive use of the basin names A, A’, B ..(page 14 onward), and not just refer to an earlier paper.*

We are glad to hear that the figures and plots were found informative. While the basin names are already included in Fig. 1, we agree that they may not be sufficiently visible due to the amount of information shown. In the revised manuscript, we will therefore consider adding an additional panel with a clearer representation of the basin names.

*It would also be useful to add the statement in the conclusions on the OCOG10D retracker in the abstract, coming in as the second-best fit (since this retracker is widely used by many groups).*

Thank you for this suggestion. We agree that this is an important result and we will include this statement in the abstract.