

Thank you very much for the thorough and constructive 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 an interesting and technically detailed paper comparing CryoSat-2 and ICESat-2 elevation change estimates over the Antarctic interior. The authors apply a comprehensive range of retracking and scattering correction methods and make a useful contribution to understanding radar altimetry uncertainties. I have a few comments below that I believe should be addressed before publication.

Major comments

- 1) Section 3.1 – Due to their differing spatial samplings, are 1 km model fits for CryoSat-2 LRM data more poorly constrained than for ICESat-2? How does varying the search radius affect the level of agreement between the two missions?*

Thank you for raising this important point. We agree that, other than the retracking and scattering correction for CryoSat-2, methodological choices such as varying the search radius in Section 3.1 might also affect the derived elevation change estimates and the level of agreement between ICESat-2 and CryoSat-2. To assess this sensitivity, we performed additional analyses comparing our standard processing to results obtained using alternative parameter choices and external products. In particular, we investigated the effect of increasing the search radius from 1 km to 2 km for both ICESat-2 and CryoSat-2 processing.

We prepared an additional figure (Fig. R1) showing elevation change trends for April 2019–October 2024 and associated trend differences for the following comparisons. Figure R1 additionally reports the mean elevation change trend over the entire LRM zone (M) and the integrated volume change trend (V) associated with each map and map difference.

- 1st row: ICESat-2 this study vs. ICESat-2 ATL15 v4 product (Gridded Antarctic Land Ice Height Change): difference of 0.05 cm yr^{-1} or $3.8 \text{ km}^3 \text{ yr}^{-1}$
- 2nd row: ICESat-2 this study vs. ICESat-2 processing using a 2 km search radius: difference of -0.03 cm yr or $-1.7 \text{ km}^3 \text{ yr}$
- 3rd row: CryoSat-2 OCOG10d this study vs. CryoSat-2 OCOG10d processing using a 2 km search radius: difference of -0.04 cm yr^{-1} or $-2.7 \text{ km}^3 \text{ yr}^{-1}$

The resulting trend difference maps (Fig. R1, right column) show both noise and spatial signal pattern. However, the mean differences remain below 1 mm yr^{-1} , indicating that the choice of search radius has only a minor influence on the overall elevation change trends. We acknowledge that some of our processing choices could be better tailored towards the different ICESat-2 and CryoSat-2 orbit configuration and spatial sampling. Nevertheless, this sensitivity tests indicate that the use of a common 1 km search radius cannot explain the mean ICESat-2–CryoSat-2 trend difference of 0.6 cm yr^{-1} presented and discussed in this study.

In the revised manuscript, we will include the Fig. R1 in the supplement, address the use of a common 1 km search radius for both missions in Section 3.1, while referring to Fig. R1 in the

supplement for demonstrating that varying the search radius does not affect the level of agreement between both missions.

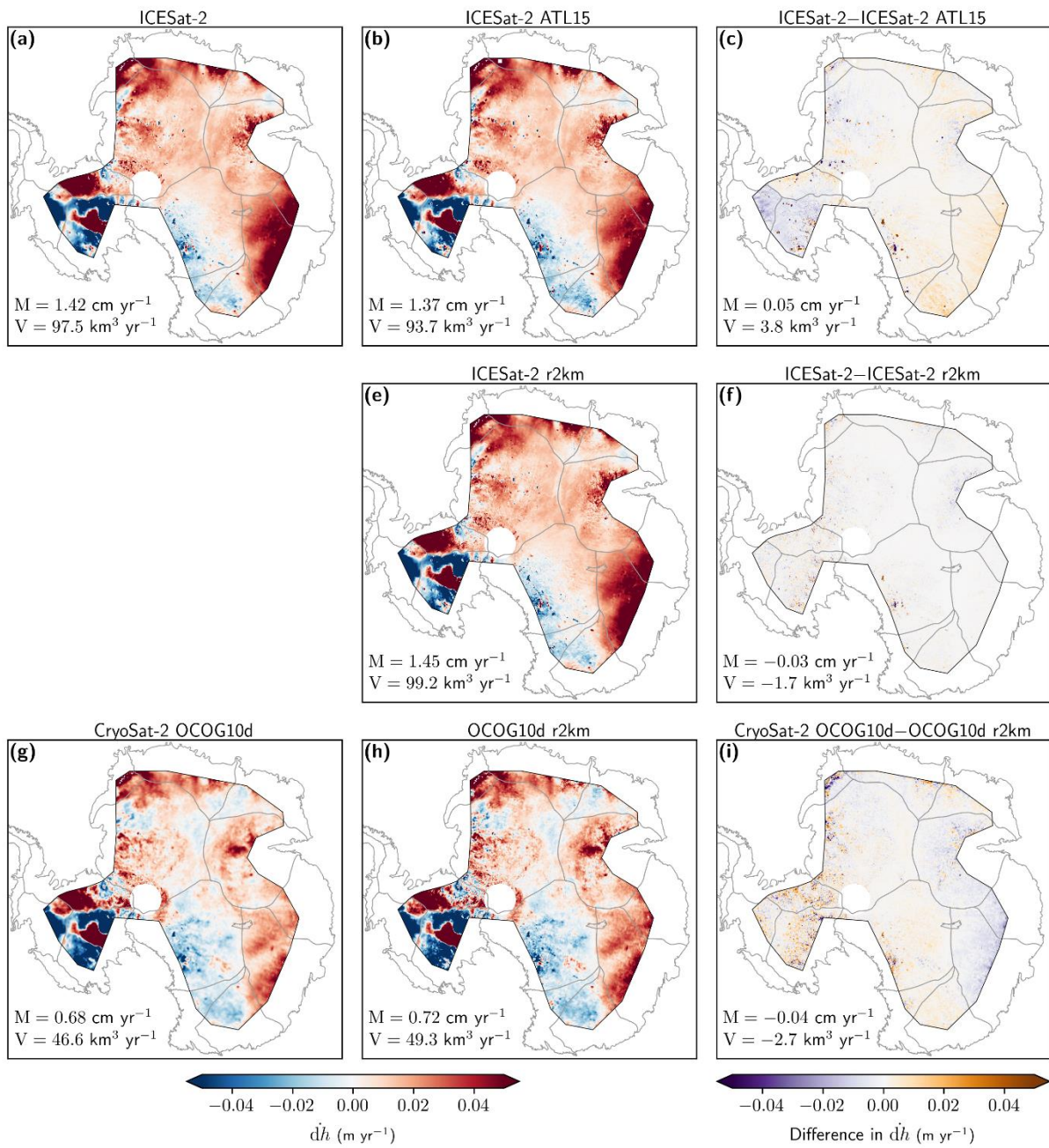


Figure R1.

- 2) *L320 – ‘Since we consider the ICESat-2 elevation changes to be the estimates closest to the truth (i.e. the most accurate estimates), we assume them to be reasonable for validating CryoSat-2 results’. This is a strong assumption. ICESat-2 has its own uncertainties: the green laser penetrates the firn to some degree (Studinger et al., 2024), time-varying subsurface scattering biases could produce spurious elevation change signals (Smith et al., 2025), and scattering from windblown snow could introduce further biases. The authors should more explicitly justify this assumption or caveat their conclusions accordingly.*

Thank you for drawing our attention to potential uncertainties in the ICESat-2 measurements and elevation change results. We agree that these aspects merit explicit discussion, and we acknowledge that our formulations in the manuscript thus far were too strong.

In the revised manuscript, we will therefore avoid referring to ICESat-2 as representing “the truth”. Instead, we will describe ICESat-2 as providing a benchmark for the evaluation of CryoSat-2 elevation change estimates. We believe this wording more appropriately reflects the current understanding that ICESat-2 measurements, while offering great advantages compared to the Ku-band radar altimetry, are themselves subject to uncertainties. To better capture this, we will revise the relevant passages in the Introduction, addressing ICESat-2 uncertainties and setting these in relation to CryoSat-2 uncertainties where necessary, particularly with regard to subsurface scattering.

We will additionally revise the Discussion section to more comprehensively address uncertainties in both ICESat-2 and CryoSat-2 elevation change estimates. While the large spread among our different CryoSat-2 solutions supports large uncertainties in CryoSat-2 results and its processing methods applied to reduce errors related to radar signal penetration, we agree that remaining ICESat-2 uncertainties should also be explicitly considered. To this end, we will extend Section 6.5 by discussing potential ICESat-2 error sources related to (1) time-varying subsurface scattering as the green laser can penetrate the firn to some degree (Studinger et al., 2024, Smith et al., 2025) and (2) scattering effects caused by windblown snow. We will consider the implications of these effects for the ICESat-2 elevation change trend estimates, including whether they make the ICESat-2 trend more negative or more positive, and how this affects the agreement or disagreement with the CryoSat-2 trend.

Finally, we will clarify that our hypothesis that radar altimetry may systematically underestimate thickening due to enhanced signal penetration during snowfall can be considered one of several possible explanations for the observed differences, rather than a definitive conclusion.

- 3) *Figure 5, Table 3, Figure 6 – The reported dh/dt trends in Figure 5 all overlap considering their uncertainties. How does that translate to the small uncertainties for the volume changes given in Table 3?*

In Figure 5, the values in the lower left corner represent the mean and the standard deviation (SD) of all the trend grid cells within the LRM zone. We agree that using the notation “mean \pm SD” suggests that the SD value represents the uncertainty of the mean value, which is incorrect. We thank the reviewer for pointing out this misleading presentation. To clarify that the SD value simply reflects the spatial variability of the trend value within the entire LRM zone and does not indicate an error or uncertainty, we will remove the “ \pm ” notation and specify, for example, $M = 1.4 \text{ cm yr}^{-1}$, $SD = 2.5 \text{ cm yr}^{-1}$ (for Fig. 5a), where M denotes mean.

As this issue affects multiple figures in the main article (Figs. 2, 3, 7 and 9) as well as in the supplement, we will revise all corresponding labels accordingly.

- 4) *Section 6.5 – This section does a really nice job of investigating the differences with respect to slope and SMB separately, but are there areas which are both high slope and SMB anomaly? The authors could explore how this affects the partitioning of the variance in delta h.*

We are glad to hear that our analysis so far have been useful. Thank you for this further suggestion, which we will consider in the revised manuscript and include a brief additional analysis.

- 5) *L524 – The authors discuss the almost zero scale factors in the WAIS, but do not address the variability in the scale factors in the non-D basins (0.19 to 0.65 for OCOG10d). Given the hypothesis that fresh snow drives stronger radar penetration, what does this variability reflect?*

Thank you for this interesting question. We agree that the variability in scale factors across non-D basins would deserve further discussion. While our hypothesis may explain some of the observed behaviour, it does not account for the variation in scale factors between individual basins, particularly in regions with negative ICESat-2 trends (basins E-E', D'-E and D-D').

At this stage, unfortunately, we do not believe that the available data and analyses allow us to draw a robust conclusion regarding the origin of this variability, and we will acknowledge this in the revised manuscript. As suggested by Reviewer #2, we will also emphasise that a better understanding of firn properties, gained through future missions such as CRISTAL, as well as further validation efforts in areas such as Antarctica's megadunes (e.g. across basin E-E'), could improve our understanding of the differences between laser and radar.

- 6) *L543 – First part: The authors point out the possibility that ICESat-2 measurements in 2023 could be biased and affect the trend, but it is quickly dropped. From Figure 6 it looks like this anomaly is largely driving the difference between the volume trends. I feel this deserves more attention, because if ICESat-2 is anomalous or biased during this period the regression analysis, scale factors and offsets are sensitive to this where ICESat-2 is considered the 'truth'.*

We totally agree that this aspect is too briefly addressed in the original manuscript. As noted by the reviewer, Fig. 6 suggests that ICESat-2 and CryoSat-2 time series diverge particularly during 2023, and that this period could potentially influence the derived trends, regression parameters (Eq. 14), and associated interpretations. We therefore agree that this hypothesis requires further investigation.

To assess the sensitivity of our results to the 2023 data, we performed an additional analysis in which we excluded all elevation change estimates from 2023 for both ICESat-2 and CryoSat-2. We then recomputed the trends and repeated the regression analysis (Eq. 14).

We include the corresponding results in an additional figure (Fig. R2), comparing the original trends with those excluding 2023. The resulting trend difference maps (Fig. R2, right column) show largely consistent spatial patterns for both ICESat-2 and CryoSat-2, with only localised differences across basin K-A and at the margin of J''-K. Excluding 2023 reduces the integrated volume change estimates from 98 to 66 km³ yr⁻¹ for ICESat-2 (compare Fig. R2a and b) and from 55 to 39 km³ yr⁻¹ for CryoSat-2 AWI-ICENet1c (compare Fig. R2g and h). While excluding 2023 also reduces the mean ICESat-2–CryoSat-2 trend error from 0.6 cm yr⁻¹ to 0.4 cm yr⁻¹, the overall spatial pattern of the error remains largely unchanged.

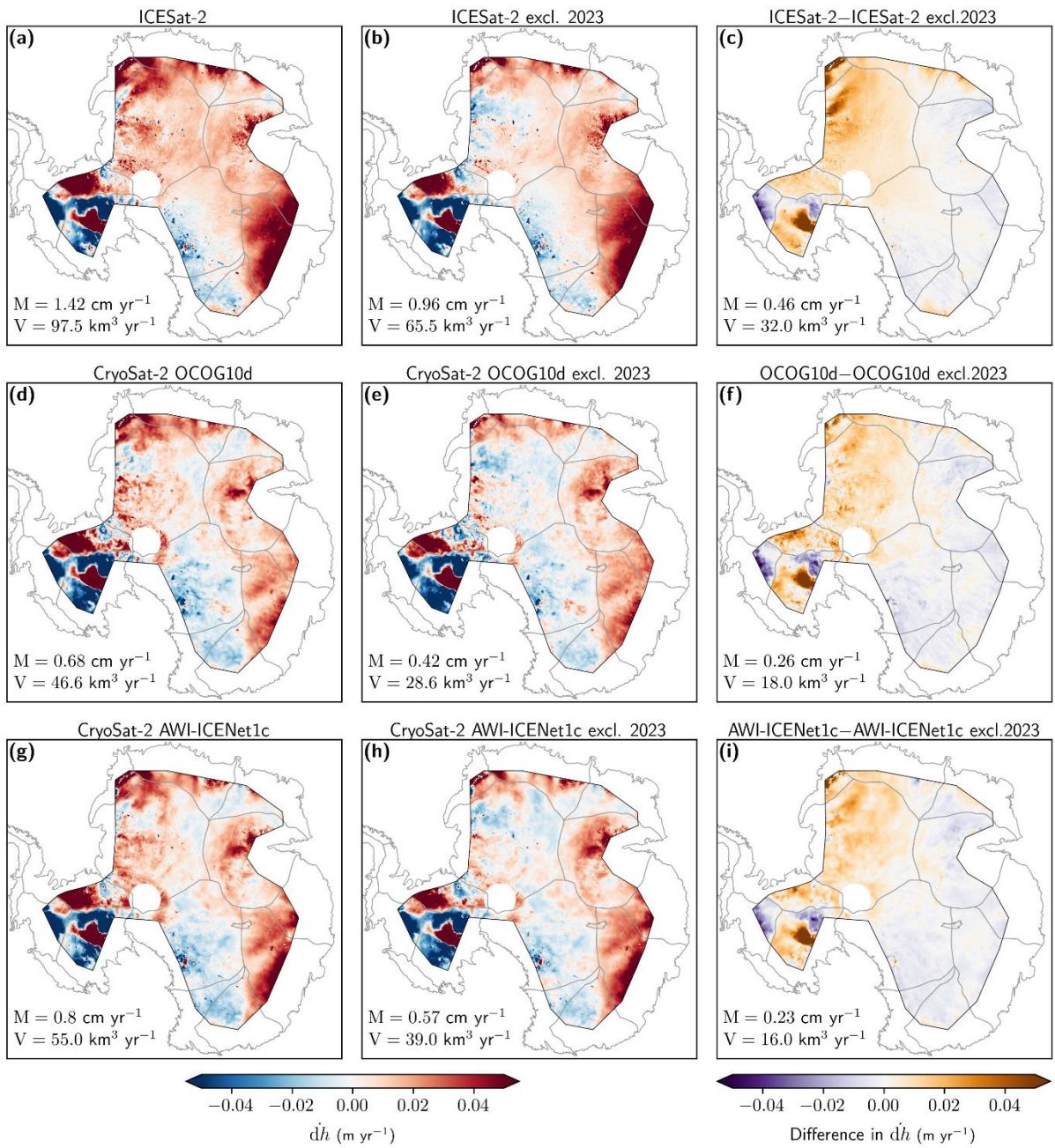


Figure R2.

Table R1. Regression parameters offset and scale estimated to fit the ICESat-2 trend to the CryoSat-2 trend error for OCOG10d and AWI-ICENet1c. Values are given for both the original estimates, as presented in Table 1 of the submitted manuscript (with non-D removed and the basin order slightly altered), and for the additional sensitivity experiment in which no data from 2023 was incorporated (excl. 2023).

Basin/ Sector	CryoSat-2 OCOG10d				CryoSat-2 AWI-ICENet1c			
	offset (cm yr ⁻¹)	offset excl. 2023 (cm yr ⁻¹)	scale (-)	scale excl. 2023 (-)	offset (cm yr ⁻¹)	offset excl. 2023 (cm yr ⁻¹)	scale (-)	scale excl. 2023 (-)
A-A'	-0.1	0.0	0.37	0.39	0.3	0.4	0.25	0.14
A'-B	0.2	0.2	0.56	0.48	0.5	0.3	0.44	0.38
B-C	-0.2	-0.2	0.38	0.34	0.0	0.0	0.18	0.16
C-C'	-0.2	-0.7	0.65	0.69	0.1	0.2	0.40	0.37
C'-D	-0.3	-0.4	0.60	0.60	0.4	0.4	0.24	0.24
D-D'	0.6	0.4	0.19	0.26	0.6	0.3	0.08	0.15
D'-E	0.6	0.3	0.65	0.70	0.7	0.4	0.54	0.51
E-E'	0.5	0.3	0.37	0.44	0.4	0.2	0.37	0.42
E'-F	0.4	0.0	0.01	0.01	-0.2	-0.4	0.02	0.03
G-H	-1.4	-1.4	0.08	0.12	-0.4	-0.3	0.06	0.05
J-J''	-0.4	-0.1	0.22	0.29	-0.4	-0.3	0.29	0.29
J''-K	0.1	0.2	0.42	0.47	0.0	0.1	0.38	0.50
K-A	-0.8	-0.4	0.53	0.39	-0.6	0.0	0.51	0.34
EAIS	0.2	0.1	0.43	0.43	0.2	0.1	0.33	0.30
WAIS	0.1	0.0	0.05	0.06	0.0	-0.1	0.05	0.06
AIS	0.5	0.4	0.14	0.16	0.4	0.3	0.12	0.13

Results of the basin-wide regression analysis based on trends excluding all 2023 data are shown in Figs. R3 and R4 and are summarised in Table R1. For comparison, we refer to the corresponding results based on the full time series (including 2023) provided in Figs. S17 and S18 of the originally submitted supplement. The “new” slope parameters show only minor sensitivity to the exclusion of 2023, with most changes remaining below 0.1. Exceptions occur in basins K-A (for both OCOG10d and AWI-ICENet1c), A-A' (AWI-ICENet1c only), and J''-K (AWI-ICENet1c only). Nevertheless, the scale factors for all the basins predominantly influenced by firn and SMB processes (EAIS and J-J'') remain to be throughout positive (ranging from 0.14 to 0.70 considering both OCOG10d and AWI-ICENet1; Table R1).

Overall, we conclude that the inclusion or exclusion of 2023 does not primarily drive the ICESat-2–CryoSat-2 trend error, nor does it fundamentally affect the relationship between the ICESat-2 trend signal and the trend error. We will rewrite L543–545 to clarify that this sensitivity test was performed and to summarise the key outcome in the revised manuscript. Figures R2–R4 and Table R1 will be added to the supplement.

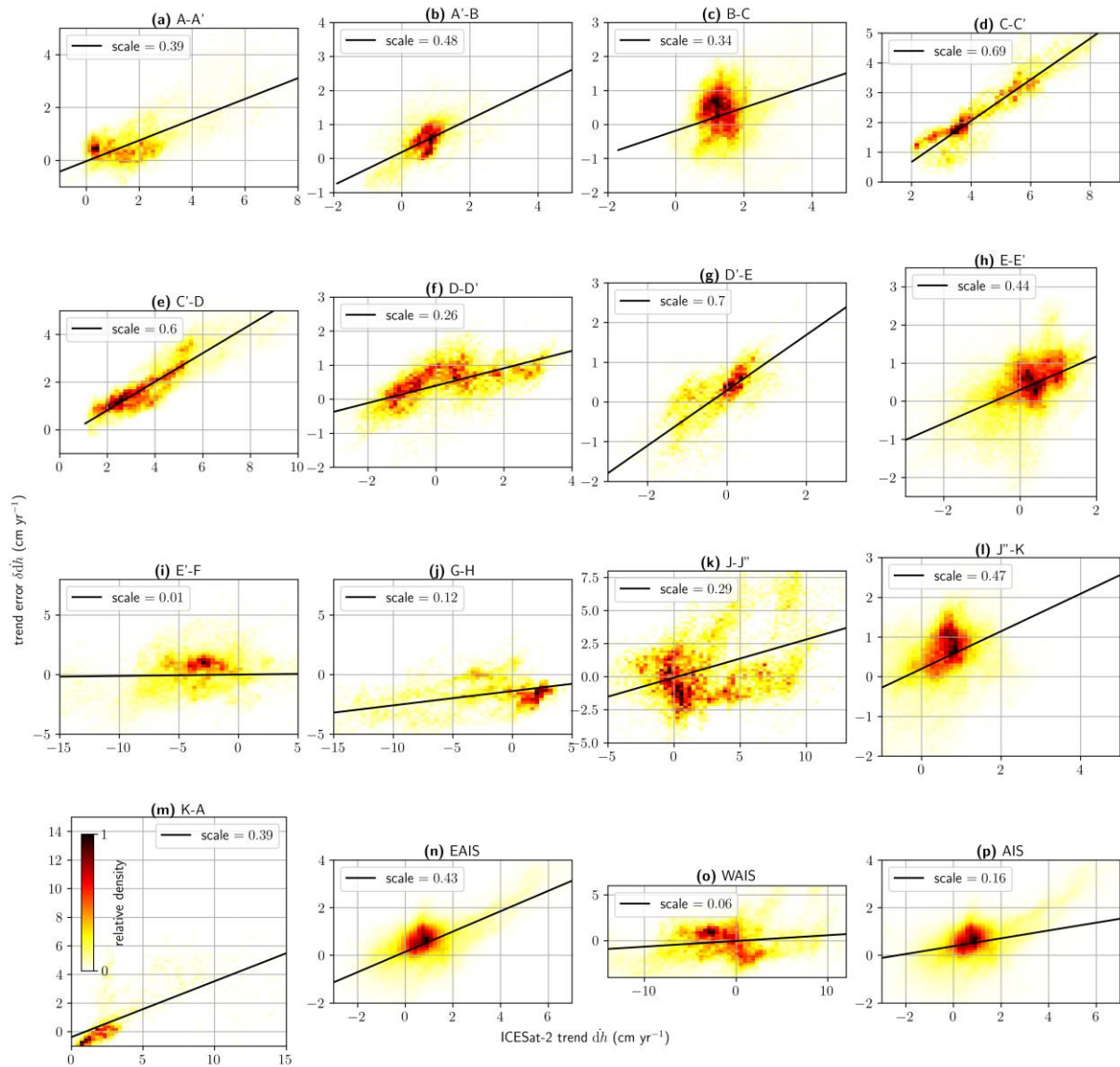


Figure R3. The ICESat-2 trend plotted against the CryoSat-2 trend error using OCOG10d for each basin, based on excluding data of the year 2023.

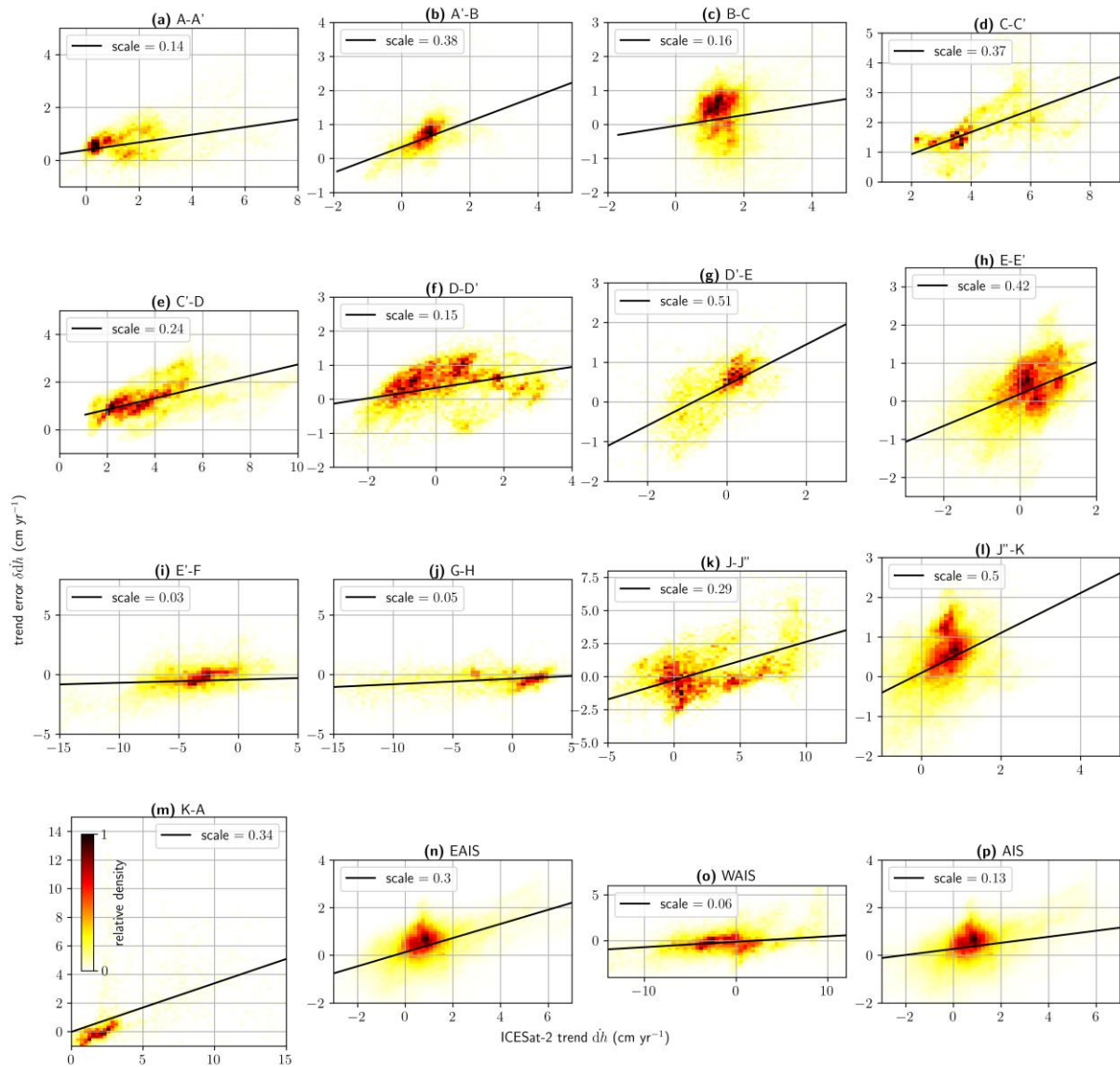


Figure R4. The ICESat-2 trend plotted against the CryoSat-2 trend error using AWI-ICENet1c for each basin, based on excluding data of the year 2023.

L543 – Second part: Indeed, if the 2023 anomaly in J''-K represents a snowfall signal (L411), this undermines the hypothesis that the ICESat-2–CryoSat-2 differences are driven by enhanced radar penetration during snowfall events because the firn model amplitude looks to be in between the two altimeter missions.

Can we trust the magnitude of the firn model's amplitude to serve as a basis for judgement? We refrained from doing so since SMB and firn modelling outputs from different models show a large spread, especially on smaller spatial scales (Mottram et al., 2021; Verjans et al., 2021), and the spatial distribution can be imprecise (Kappelsberger et al., 2024). Instead, we have incorporated the firn model primarily to assess the consistency in the timing of changes in SMB and firn processes, rather than to serve as a reference for absolute amplitude comparisons between satellite-derived elevation change estimates. We mention that there is some mismatch in amplitude without discussing potential limitation in the firn model as this will go beyond the scope of this study.

7) *Section 6.8 – I have several concerns about this section:*

- *Why include the islands? Altimetry estimates of dh/dt and dM/dt are commonly limited to the grounded ice sheet.*

We agree with this comment. In the revised manuscript, we will restrict this estimate to the grounded ice sheet and exclude the islands

- *Is it reasonable to extrapolate LRM differences across the rest of the ice sheet? CryoSat-2 does not operate as a pulse-limited altimeter at the ice sheet margins and so the factors affecting CryoSat-2–ICESat-2 differences there will be different.*

We agree that extrapolating results from the LRM interior to the entire ice sheet is a strong simplification. The intention of this calculation is just to provide a rough estimate of the potential magnitude in altimetry trend error for the entire AIS. We acknowledge that CryoSat-2 operates in different acquisition modes (LRM in the interior and SARIn at the margins), and that error sources and their magnitudes may differ. We will include this limitation in the revised manuscript.

- *Using ice density is not conservative, it is physically implausible.*

We will remove the conversion using ice density and retain only the snow-density-based estimate.

- *28 Gt/yr is less than the uncertainty in Antarctic mass balance reported in IMBIE (Otosaka et al., 2023) over 5-year periods similar in length to the one used here (~40-50 Gt/yr) and of the order of the uncertainty reported by ICESat-2 studies (Smith et al., 2020).*

In this discussion, we will include uncertainty estimates reported by ICESat-2 studies from Smith et al. which are in accordance with the magnitude of trend error estimates presented here.

We also realised that the reference assignments in L625 were mixed up, which may have led to additional misunderstanding or confusion. The sentence should read: “In the light of our minimum AIS trend error of 28 Gt yr^{-1} , the altimetry uncertainties of $\pm 7 \text{ Gt yr}^{-1}$ (Otosaka et al., 2023b) and $\pm 16 \text{ Gt yr}^{-1}$ (Schröder et al., 2019) appear unrealistically small and may not account for systematic errors.” In this way, the uncertainty values and references correspond to those introduced in the previous paragraph. In addition, we understand that the formulation “ $\pm 7 \text{ Gt yr}^{-1}$ (Otosaka et al., 2023b)” should be clarified and revised to “about $\pm 7 \text{ Gt yr}^{-1}$ (read from Fig. 3a in Otosaka et al., 2023b; altimetry only)” in order to make explicit that this value refers to the aggregated altimetry results within IMBIE, rather than to the reconciled estimates.

We further would like to emphasise that the intention of this discussion was to highlight the importance and need for robust and consistent uncertainty characterisations in both single- and multi-mission altimetry results. As Boxall et al. (submitted to *The Cryosphere*, 2026) have recently discussed, this is currently lacking, but would improve the use of long-term altimetry estimates to produce reconciled mass balance estimates. In the revised manuscript, we will reword this section of the discussion to provide a better explanation.

- *Why only convert the ICESat-2 changes to mass?*

We only convert the ICESat-2-derived trends for comparison with GRACE to mass because the CryoSat-2 analysis in this study is restricted to the LRM zone and does not include SARIn-mode measurements over the ice sheet margins, whereas our ICESat-2 analysis covers both the interior and the ice sheet margins. A comparison with GRACE-FO requires consideration of the entire EAIS because the coarse spatial resolution of GRACE-FO would cause substantial signal leakage near the LRM zone boundaries if the analysis were restricted to this zone. This will be explained more clearly at the beginning of the paragraph.

Saying this, the authors are right to highlight that challenges remain.

Minor comments

L4 – 'However, uncertainties in radar-derived elevation estimates remain substantial' — I think this is a somewhat unfair framing of the problem. Uncertainties remain with the laser altimetry and indeed the green light (to a lesser degree) also penetrates into the firn pack (Smith et al., 2025; Studinger et al., 2024). And, as the authors mention later, good agreement has been found between the two missions elsewhere already.

We acknowledge the uncertainties with laser altimetry (see response to major comment 2). However, we do not fully agree that the current wording is unfair as radar-derived elevation estimates are affected by surface topography and time-variable signal penetration. Newer radar altimetry mission have improved the accuracy and spatial resolution of measurements and future radar mission will further do, but deriving continuous time series from the radar altimetry records since 1992 remains challenging.

L20 – 'If the mean trend difference here were representative of the entire grounded AIS (12 352 700 km²), it would correspond to an underestimation of AIS volume and mass trends by approximately 74 km³ yr⁻¹ and 28 Gt yr⁻¹, respectively'. Again I find this to be an unfair framing – 28 Gt/yr is less than the uncertainty in Antarctic mass balance reported in IMBIE (Otosaka et al., 2023) and of the order of the uncertainty reported by ICESat-2 studies (Smith et al., 2020).

We agree that the wording “underestimation” may be too strong in this context. We will revise the sentence to clarify that this represents a rough estimation. We further again would like to note that our comparisons to IMBIE refer to the uncertainty estimates of the aggregated altimetry results as presented in their Fig. 3a (Otosaka et al., 2023), which are well below 28 Gt/yr. As already mentioned in our response to major comment 7, we will clarify that.

L61 – 'and their temporal evolution can remain with spurious large variations'. This wording could be improved.

We will rephrase this sentence.

L73 – 'as its green laser virtually has no penetration into the firn layer'. See above comments.

This statement will be revised in line with our response to major comment 2.

L373 – 'CryoSat-2 OCOG10d reveals larger σ_{AD} values than ICESat-2 with mean values of 5.9 ± 3.6 cm vs. 3.6 ± 1.7 cm'. These overlap considering their uncertainties so I'm not sure I would agree one is larger than the other.

We will rephrase this sentence in line with our clarification above (response to major comment 3) to avoid the misleading use of the “ \pm ” notation. The sentence will then read: “CryoSat-2 OCOG10d reveals larger σ_{AD} values than ICESat-2 with mean values of 5.9 cm vs. 3.6 cm and SDs of 3.6 cm vs. 1.7 cm”.

In addition, we will check the rest of the manuscript for similar misleading wording and remove the “±” notation.

L374 – 'For both datasets, but particularly for CryoSat-2, the spatial pattern of σ_{AD} seems related to the surface slope and roughness (Fig. 1)'. This can be quantified – ICESat-2 may be similarly related, it is difficult to judge based on the figure alone due to the colour scale.

We agree with this comment and thank the reviewer for pointing this out. The relationship between surface slope and the considered metrics (σ_{AD} , σ_{IC} , σ_0 , and $\sigma_{dh/dt}$) has been quantified, with correlation coefficients provided in Fig. S12 of the supplement, which is referenced in Section 6.5. However, this information is not visible in the main text at this point. We will make this more explicit in the revised manuscript.

L382 – 'Overall, the OCOG10d values are larger than AWI-ICENet1c with mean values of 4.9 ± 2.6 cm vs. 4.3 ± 2.1 cm'. As above, not sure I would agree with this statement – they are comparable.

This will be changed accordingly, in line with our revision described above (response to major comment 3): “Overall, the OCOG10d values are larger than AWI-ICENet1c with mean values of 4.9 cm vs. 4.3 cm and SDs of 2.6 cm vs. 2.1 cm”.

L406 – 'We refer to these as "non-D" basins. Across the non-D basins...'. This seems like unnecessary nomenclature which only serves to confuse the reader.

We agree that introducing the term “non-D” basins as additional nomenclature is unnecessary and may cause confusion. We will therefore remove this terminology in the revised manuscript. Instead, we will either use short descriptive explanations, such as “basins where elevation changes are primarily driven by firn and SMB processes”, or directly refer to the relevant regions (e.g. the EAIS and basin J–J”). We will also consider presenting some analyses currently applied to the “non-D” basins, such as the regression in Eq. (14), directly for the EAIS. This revision will not affect the main results or conclusions but should reduce complexity and improve the readability of the manuscript.

L527 – 'The throughout positive scale...' apologies I found this sentence hard to read.

Yes, this sentence is too long and complicated to read well. We will rephrase it.

L536 – 'As the instrument ages, the transmit power usually reduces and the impulse response widens.' – do the authors have evidence for this occurring in CryoSat-2?

We will include further discussion/references (Fornari et al. 2014; Garcia-Mondéjar et al., 2018).

L575 – How do the authors' results compare over the same period (2019–2021)?

Unfortunately, a direct comparison with the results from Yue et al. (2023) over the same period is not straightforward due to differences in both the study area and the presented results.

Yue et al. (2023) analysed surface elevation changes in the Amundsen Sea sector from multi-mission altimetry measurements covering the period 2003–2021. In our manuscript, we referred to their average CryoSat-2–ICESat-2 trend difference of 6.7 cm yr^{-1} , which we obtained from their Fig. 3. However, our study is restricted to the CryoSat-2 LRM zone and begins in April 2019, while the Amundsen Sea sector considered by Yue et al. (2023) only partially overlaps with our study area. In addition, Yue et al. (2023) do not provide spatially resolved CryoSat-2 and ICESat-2 trend differences for this region, making it difficult to assess whether the reported mean difference is representative for the overlapping region of the LRM zone considered in our study.

We will therefore clarify in the revised manuscript that the Amundsen Sea sector is largely outside our study area and that a direct comparison between the reported trend differences is not possible.

Table 4 – Worth mentioning explicitly in the caption why there is no correction d value for AWI-ICENet1.

Good point. We will include that.

Review criteria

Concerns of the review criteria should all be covered in the above comments.

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