Response to Reviewer 2

We thank the Reviewer for the careful and constructive comments. The suggestions and corrections have greatly improved the quality of this manuscript.

We have responded to all comments. The line numbers provided refer to the track changes document.

Review comments for **"First Arctic-wide assessment of SWOT swath altimetry with ICESat-2 over sea ice"** by Felix L. Müller, Florian Seitz, and Denise Dettmering

The manuscript presents a comparison of elevations measured by SWOT and ICESat-2 in the Arctic ocean (below the latitude limit set by inclination of SWOT) - most importantly in sea ice covered areas. The paper is an important step in understanding the novel altimeter measurement of SWOT over ice infested waters. The potential of SWOT is large, but we need studies like this to be able to utilise the data.

Overall, the paper is well written, interesting and important and most certainly deserves to be published. However the manuscript would benefit from a thorough, minor revision, adding more rigour and sharpening the overall presentation. The main results - overall agreement of ICESat-2 and SWOT elevations as well as the up to 50 cm differences when comparing left- and right-hand swaths are sound and scientifically the paper is of good quality. Thus, I would recommend **minor** revisions concentrating on presentation as well as adding more information on the SAR data used - including SAR / sea ice interaction as well as discussion on how snow on sea ice affects the difference between ICESat-2 and SWOT.

<u>More specific comments:</u>

Reviewer #1 has already raised a good point of using "SLA" for both elevations of sea ice and water. I'd recommend the authors to use another term, such as surface height or elevation as suggested by reviewer 1.

We modified the text and changed SLA or sea surface heights to surface elevations or heights (see comments Reviewer 1). The changes are visible in the track change document.

Also, already noted by reviewer 1, "sentinel-1 gray scale value" is a vague term. I'd recommend authors add a subsection in the data section explaining what it actually is and where does the data come from. Throughout the paper, S1 images are discussed quite a lot, but what lacks is the background on how SAR and sea ice actually interact. That is to say - there are many ways sea ice can produce high (or low) backscatter and not all of them affect the actual surface elevation of the sea ice surface.

We intentionally kept the description of the processing of Sentinel-1 SAR images to a minimum, as our study focuses on comparing SWOT and ICESAT-2. Sentinel-1 is only intended to visually support this comparison with imaging information, so the physical backscatter values were converted to an 8-bit greyscale range. For this reason, the introduction only shortly mentions the dataset used and refers to the paper by Müller et al., 2023, which contains more information about Sentinel-1 data processing and the scattering behaviour of sea ice surfaces. We have decided not to include a separate section for Sentinel-1 and to cite already existing literature (Müller et al., 2023). However, we agree that the term 'grey-scale values' is too simplistic and changed the text to be more specific how the backscatter values are handled. More information on the interpretation of SAR images are added later in the text (L156-161, see answer to first specific comment).

Another thing I'd like to see in the paper that is now lacking is the effect of snow on sea ice. Currently, snow is not really mentioned at all. Part of the mismatch between SWOT and ICESat-2 is due to penetration depth into snow (which is very shortly mentioned in L251). This should be elaborated.

Snow in connection with sea ice is a very complex topic that would deserve its own study. In the case of ICESat-2, we assume that snow is reflected at the snow surface due to the wavelength (green laser light) and that no significant penetration occurs. Therefore, we assume that the snow depth is included in the height observation and part of the sea ice freeboard (see, e.g., Petty et al., 2023).

In the case of SWOT, the influence of snow on height observations is more challenging to understand and assess, as the structure and composition of snow significantly impact the penetration depth. Moisture, temperature and composition of the snow layer in particular affect the radar scattering behaviour. In general, however, Ka-Band penetrates less deeply into the snow than the commonly used Ku-Band and tends to reflect at the upper part of the air-snow boundary (e.g. Willat et al., 2025; Fredensborg-Hansen et al., 2025; Guerreiro et al., 2016). However, in the case of KaRIn, it is still unclear to what extent the incidence angle in connection with various surface properties has an effect. Similar to nadir Ka-band altimetry, a few recent studies certify that only slight influences are to be expected (e.g. Jutila and Haas (2025), Fayne et al. (2024)). But, to gain deeper insights into KaRIn's penetration behaviour, detailed studies based on in-situ or airborne validation data are required.

With respect to the explanations above, we rephrased and restructured the text.

Text changes L282-295:

With respect to small-scale features of the ice-covered sea surface, surface elevations from SWOT appear smoother and are characterised by more gradual transitions between the sea ice edges and open water. In contrast, ICESat-2 profiles tend to show more pronounced elevation variations and a clearer separation of different height or sea ice features (even if filtered to the sparser SWOT resolution). This indicates that the SWOT LR dataset can resolve small open water and sea ice areas only to a limited extent. Besides wavelength differences, this can also be attributed to the different sample resolutions. Kacimi et al. (2025) also reported that it is difficult for SWOT LR data to capture surface features at shorter length scales. Higher-resolution data sets of SWOT (e.g. HR, pixel-cloud) could likely reduce this discrepancy. However, besides the resolution, other factors contribute as well. One might be the different snow penetration of both instruments.

It is common knowledge that the ICESat-2 laser data hardly show any penetration into snow or ice and represent the entire sea ice surface height (i.e. freeboard), including a possible snow layer (Petty et al., 2023). In contrast, it is still unclear how KaRIn-derived elevations are affected by varying surface conditions, for example by snow load or thin ice. In this context, there is a lack of knowledge about the sensitivity of KaRIn to different snow and sea ice properties under the influence of off-nadir incidence angles, which affect the signal propagation and backscattering behaviour. In general, Ka-band penetrates less deeply into the snow than the Ku-band and tends to reflect at the upper portion of the air-snow interface (e.g. Willatt et al. (2025), Fredensborg Hansen et al. (2025), Guerreiro et al. (2016)). According to first investigations, this penetration behaviour is also to be expected for KaRIN (Jutila and Haas (2025), Fayne et al. (2024)). But in order to gain deeper insights into KaRIn's signal propagation, detailed studies based on in-situ or airborne validation data are required (Armitage and Kwok, 2021).

<u>Extremely specific comments:</u>

L140-143: The driving phenomena behind higher backscatter values is surface roughness. This should be explained and also connected to the fact that surface roughness of sea ice is very much connected to surface height. This is touched upon at ~ L170, but in my opinion quite lightly.

We agree and extend the explanations.

L156-161: In sea ice areas, the SAR image backscatter is mainly controlled by the surface roughness, but also by height and sea ice topographic variations. Leads or open water patches usually tend to appear very dark in SAR images (low backscatter) due to mirror-like scattering characteristics. In contrast, rough sea ice surfaces or topographic features like ridges or hummocks for example result in stronger backscatter due to a more diffuse backscattering (e.g. von Albedyll et al. (2024), Murashkin et al. (2018), Dierking (2013)).

L150-160 (Figures 4 and 5): The discussion here includes few wobbly statements like "grey-scale values show a strong coherence with SWOT", "can be monitored very well" and "captured quite well". These should be re-written to something preferably quantitative. In the big picture of Figure 4, one can see areas where all three datasets agree, but also areas where the correlation is *not* apparent.

Following the explanations above, the grey scale values shown in Figures 4 and 5 are not intended to represent physically quantifiable radar image backscatter or to be directly comparable with SWOT or ICESat-2 measurements. Rather, they serve as a qualitative indicator to provide a visual third reference for contextual comparison between the different datasets. For these "case studies", no quantitative comparison results are available (in contrast to the SWOT-IS2 comparison. Please also note changed x-axis (right) labels.

We added to the explanations of Figure 4 and 5 (L176-177): *In this context, the SAR image is only used as a qualitative indicator to support the comparison between SWOT and ICESat-2*

Figure 5: X-axes should be made equal for all subplots, even if the northernmost beam lacks data in the Western end.

The figure has been updated.

Figure 9: There is an interesting linear growth in STD over winter 23-24. This coincides with thickening ice, increasing surface roughness as well as thickening snow pack.

We agree. However, we did not further discuss this, since it is not clear whether it is related to the thickening ice or to less open water openings. This is a subject for further studies.

L190: I do not disagree, but would like to comment that pan-Arctic sea ice surface elevation distributions often have two maxima - one for first year and other for multi year ice. Would it be possible that the STD here would be a combination of two gaussian distributions, one for FYI and other for MYI?

Figure 7 shows the histogram of offsets between ICESat-2 and SWOT, not the STD. We can't exclude that the offsets depend on the ice properties, however, a relation to the quality of the cross-calibration correction is more probable, especially given the large values of the offsets.

L245: Not precision (as also pointed out by reviewer 1)

We changed that.

L260: Would it be possible to quantify the difference caused by time difference in acquisitions?

This is not only a question of the time difference but also of the speed at which the ice moves. However, it is very difficult to find values from the literature that describe sea ice movement during our investigation time period. This is particularly difficult given the regional and temporal differences in the Arctic Ocean. Therefore, based on the daily OSI SAF sea ice drift products (Global Low Resolution Sea Ice Drift - Multimission; GBL LR SID — OSI-405-c; OSI DAF (2010)), we independently determined monthly averages from November 2023 to April 2024, which show average drift velocities ranging from 280 m/h in April to 415 m/h in January 2024. In the following plot, monthly maps with maximum velocities in January up to ~620 m/h are shown:

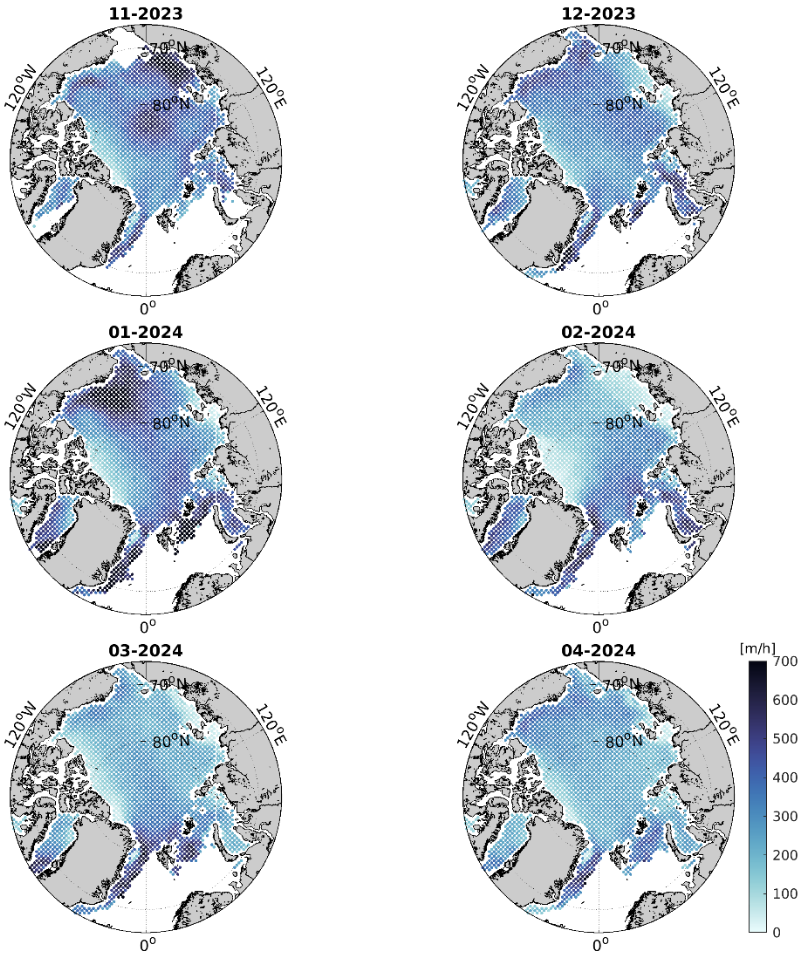

Figure 1 Monthly sea ice drift velocities from GBL LR SID - OSI-405-c from November 2023 until April 2024.

We added the averaged values and the reference to the text (see L296 in the track change document).

Besides the reasons on the side of instruments and data processing, it should be mentioned that the discrepancies of the acquisition times of the satellites add additional uncertainties due to sea ice dynamics or environmental changes, such as strong drifts, which can reach monthly drift velocities ranging from 280±112 m/h in April 2024 until 415±154 m/h in January 2024 on average. Dependent on the season, the highest drift velocities occur in the Beaufort Sea, the Fram Strait, and the East-Greenland Current region (OSI SAF, 2010). Additionally, steep temperature gradients can cause rapid openings and closings of leads.

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