Authors' response, second round of reviews.

We thank Dr. Sørensen for taking this manuscript through a second round of reviews. The referees have continued to offer helpful comments, and we attach our responses to their reports below. In addition to the responses to the referees' comments, we have also added an *author contributions* section and have revised the caption to figure 6 to respond to comments from the validation of the previous draft.

All the best
-The authors.

Referee 1 report, second round

Thanks again to Referee 1 for their attention to our manuscript. We have clarified the text that led to a misunderstanding about incidence angles for the ATM lasers (described below, comments on lines 249-252 and 485) and have tried to improve the structure of the paragraphs introducing the evaluation of OLCI-based corrections for subsurface scattering. We followed almost all of the referee's suggestions for revisions, which are described inline with the review below. Our responses are in blue, sans serif, and quotes from the revised manuscript are in *italics*.

tc-2023-147
General Comments
As Reviewer 1 of the initial submission, I want to start off by sincerely thanking the authors for their efforts in revising their manuscript in response to the comments they received. Overall, I found this updated version of the manuscript much clearer and more straightforward to follow. Most of the comments I have on this updated version are technical issues with only a few minor comments related to specific points. Once addressed I believe the manuscript would be ready for publication within TC.

Lines 249-252: Would one expect s, and therefore the illumination pattern contribution in Equation 3, to evolve through the ATM swath as the incidence angle changes? The manuscript seems to imply that s is a constant for the wide (1.0 ns) and narrow (0.2 ns) swath. But wouldn't this be the same for overlapping wide and narrow swath incidence angles (i.e., in the 2.5° incidence angles around nadir)? I am also missing the connection between the quantified values (1.0 and 0.2 ns) and the 0.7 m ATM footprint. Could the authors elaborate a bit more on how they arrive at these values?
Line 485: Here the authors suggest the larger incidence angle as a possible reason for the larger wide-swath grain sizes in Figure 7. Does Figure 7 not represent a point-to-point comparison of the narrow and wide swath grain sizes? If so, I would have expected the only points where this type of comparison is possible to all lie within the overlapping strip near nadir and where the wide and narrow swath incidence angles are equal. Is there something I am missing with how the authors are comparing the two ATM datasets? How are the authors comparing grain sizes from

the extreme ends of their wide swath dataset (i.e., the largest incidence angle) with the narrow swath data that don't extend to the same crosstrack extent?

Response: We are sorry for the reviewer's confusion here. When we introduced ATM, we mentioned that it uses "a conically scanning lase," but we did not spell out what that means for the geometry of the beam with the surface. A conical scanner always has the same off-nadir angle (at least for an aircraft in level flight) so it doesn't make any difference whether the points are collected along the aircraft flightline or off to the side- the incidence angle is approximately the same, give or take the surface slope. The point-to-point correspondence in figure 7 does not imply that the aircraft was in exactly the same position for the measurements being compared, just that the measurements were in the same position.
We have updated the text to make this point clear:
"Note that because ATM uses a conical scanning mechanism, each scanner's beam will intersect a flat surface with an incidence angle equal to the scanner's off-nadir angle. "

Section 4.5: I still have trouble following exactly what is happening in this section. For example, on Line 564 where the authors write "… the ICESat-2 bias predicted based on OLCI measurements as a function of ATM-derived grain size.". To me, this reads as though the authors are using ATM grain sizes to calculate OLCI grain sizes to calculate ICESat-2 biases, which I have trouble following the logic behind. Maybe it is the use of "… as a function of …" that is causing the confusion and could "… compared to …" be used to equivalent effect? I would also suggest the authors consider revising the y-axis labels in Figure 12 as, as far as I understand, it is not OLCI or ATM biases they are plotting but modeled ICESat-2 biases based on OLCI and ATM grain sizes (i.e., for Figure 12b, essentially combining and turning the yaxes from Figure 11 into a range bias). I think this may also help clarify things.

We agree that this section is difficult and have tried to make the description of Fig. 12 easier to follow by separating the description of the plot from the description of the results it conveys, and by clarifying our terminology about what biases are derived from what sensor. We have also changed the axis y labels to: "ICESat-2 bias predicted from OCLI" and "ICESat-2 bias predicted from ATM." We did not change the orientation of the plots, because the derived quantity in these plots is the estimated range bias, calculated for different values of ATM or OLCI grain size.

Section 4.6 now reads:
Comparing grain sizes estimated from the different sensors (Figs. 9-11) demonstrates the consistency (or lack thereof) between the datasets, but to address the usefulness of OLCI data in correcting biases in ICESat-2 data, we need to compare biases predicted for ICESat-2 based on OLCI with biases estimated based on ATM waveforms. In these comparisons, the accuracy of the sensor is most important for large grain sizes because ICESat-2 biases predicted by our model (Fig. 4) are approximately zero for small grain sizes, so any correction we calculate will be small, with larger corrections expected for larger grain sizes.

If we assume that the ICESat-2 range biases predicted from the ATM waveforms are approximately correct, we can estimate the accuracy of OLCI-derived predictions of ICESat-2 biases in two ways: We can calculate the distribution of OLCI-derived predictions of ICESat-2 range bias for groups of ATM grain-size estimates (Fig. 12a), and we can calculate the distribution of ATM-derived predictions of ICESat-2 range bias for groups of OLCI-derived grain-size estimates (Fig. 12b). In Fig. 12a, we collect groups of ATM grain-size estimates in logarithmic bins with a spacing of $10^{0.25}$ µm and calculate the median and robust spread of biases of the ICESat-2 biases predicted from the corresponding OCLI grain sizes. In Fig. 12b, we reverse this sampling and calculate the distribution of ICESat-2 biases predicted from ATM measurements for groups of OCLI-estimated grain sizes. In each set of axes, we plot the modeled relationship between grain size and range bias for reference.

The two plots in Fig. 12 cover different ranges of grain sizes because of the different ways that the two sensors sample the ice sheet. Fig. 12a includes large values of grain size from ATM (up to around 11000 µm) because single ATM measurements occasionally sample features on the surface with large grain sizes and includes no ATM measurements with grain sizes smaller than 30 µm because for smaller grain sizes, ATM often reports zero scattering. In Fig. 12b, grain sizes larger than 2000 µm do not appear, because the 1-km OLCI pixels rarely measure the small features where coarse grain sizes are observed. For the smallest OLCI-derived grain sizes, it appears that ATM often returned no-scattering estimates, so the estimated bias is effectively zero for both datasets.

On a per-ATM-waveform basis (Fig. 12a), OLCI bias estimates underestimate the sensitivity of ICESat-2 biases to grain size, especially for large ATM-derived grain sizes. This is likely because OLCI does not resolve small-scale coarse-grained features that are resolved by ATM (e.g. Fig. 8). In Fig. 12b, where the data are binned based on OLCI-derived grain size we see a closer match between the ICESat-2 biases predicted based on the ATM data and those predicted based on the OLCI measurements, at least for OLCI-estimated grain sizes larger than around 250 µm. At smaller grain sizes, the ATM-derived ICESat-2 bias estimates deviate from the OLCI biases, with a roughly uniform value close to 0.02 m for OLCI-derived grain sizes between 20 and 100 µm, a small peak for OLCI biases close to 15 µm, and approximately zero bias for finer grain sizes. This better correspondence shows that when OLCI-derived grain-size estimates can resolve coarse-grained features on the ice sheet, ATM measurements confirm the implied large bias values.

Technical Comments

Line 44: doubled Harding et al. (2011) citation

Deleted

Line 68: missing space after the Fair et al. (2024) citation

Fixed

Line 102: “ATM (the Airborne Topographic Mapper) makes laser-altimetry…”

Fixed

Equation 1: I don't think reff is ever explicitly defined in the text
Now defined: *, r_{eff} is the optical effective grain size, corresponding to the radius of a collection of ice spheres that would have the same surface-to-volume ratio as the scattering medium (Grenfell and Warren, 1999),*

Line 246: "… whose normal makes and angle …"
Fixed

Lines 247 and 249: j versus f. I would recommend standardizing the notation
Fixed

Equation 4: the reff notation used up to this point seems to have been replaced with ro
Fixed

Line 329: do the authors mean when the SNR is low? It appears they are pointing to the upper right portion of Figure 3 and the similar error bars between the Pmax=225 (high SNR) and Pmax=90 (low SNR) scenarios.
The referee is correct. Changed to 'low'.

Line 355: here h_li notation is used whereas on line 338 it is hli. The h_li notation appears at other points in the manuscript as well (e.g., Line 388).
The clearer way to say this is h_li (rather than its subscript representation). Fixed.

Line 361: Is "Over" meant to be capitalized?
This is the first word after a full colon, so it should be capitalized.

Line 403: "… values of B0 and rthr that minimize …"
Fixed

Line 418: Scambos et al. (2012) does not appear in the reference list
Reference updated to Haran et al, 2018.

Line 423: "… (Fig. 5c) …"
Fixed

Figures 9 and 10: I thank the authors for homogenizing AVIRIS and AVIRIS-NG in their revised manuscript, but I would suggest also carrying this through the x-axis labels in these two figures.
Fixed

Line 545: "… (Fig. 11a) …"
Fixed

Line 561: "… (Fig. 4) …"

Fixed

Line 565: should there be a unit give to $10^{0.25}$? Perhaps μm?
Fixed, using μm.

Figure 13: In the caption, two different fonts and font sizes are used when referring to Equations on Line 611 and Line 615.
Good eye. Fixed.

Line 632: Missing space prior to the Fair et al. (2024) citation.
Fixed.

Lines 735-736: "… satellite-driven grain-size estimates of providing estimates that would …" could the phrasing here be improved?
Yes, the phrasing can be improved (I hope): "*Another possible data source for corrections of this type would be grain size predictions driven by a grain size-evolution model driven by meteorological data, remote-sensing data, or model output. Unlike grain-size estimates derived purely from satellite measurements, these would not be limited by the availability of cloud-free observations, and might be able to integrate remote-sensing data from multiple sources to reduce the effects of measurement errors.*"

Referee 2 response, second round

Thanks much to Dr. Ryan for his second look at our manuscript. We have attended to his comments as described below, with our responses in blue sans-serif font, and all quotes from the revised manuscript in *italics*.

This is an expansive study that describes some innovative ways of correcting ICESat-2 range biases by deriving grain size from several sources of observational data. There are several challenges that the authors had to overcome to complete this study. For example, quirks of individual sensors, conflating processes that influence waveforms, spurious relationships between grain size derived from ATM, AVIRIS, or Sentinel-3 OLCI data. etc. Many sections of the manuscript definitely require close attention when reading but the authors should be commended for their honest and thorough description of their approach. I endorse acceptance of this manuscript in The Cryosphere.

I also appreciate the amount of work that the authors have put into the revised manuscript. I do not have many more comments. Those that remain are mostly associated with the new text that may help improve the style and clarity of the manuscript. Note that my line numbers correspond to the tracked changes version.

Technical comments

L42: Sorry I didn't catch this in my previous review but surely laser altimetry techniques allow efficient measurement of "glacier ice" surface elevations as well? Consider removing "snow-" from this sentence to generalize to both cases.
Fixed.

L58: I think it would be more accurate to say ice-sheet "elevation" changes.
Fixed.

L204: Should be Fair "et al." (2024)
Fixed

L115-120: These sentences would read better if the references were at the end of their respective sentences.
Thanks for the suggestion. I changed the second three sentences to fit this model, but left the first, where the placement of the references is relevant to the meaning of the sentence.

L121-122: Please clarify that this is "airborne" altimeter data since there was a lot of text about ICESat-2 in the previous section which may confuse readers.
Fixed.

L257: Subject-verb agreement issue here. Should be "ATM makes…" if the word "system" is singular.
Fixed.

L552-553: "ablation-zone surfaces" is a little vague since that could include snow or ice depending on the time of year. What about just "observed for glacier ice"?
Changed to "*melting snow and glacier-ice surfaces*"

L535-539: I know it's obvious but probably should define r_eff here as well.
Now defined: *r_{eff} is the optical effective grain size, corresponding to the radius of a collection of ice spheres that would have the same surface-to-volume ratio as the scattering medium (Grenfell and Warren, 1999),*

L658: "cases"
Fixed.

L853: Explain why infrared light does not penetrate as far as green light
We added: *"because of the stronger attenuation of infrared light by ice (Warren 2008), "*

L1210-1216: It would be useful to briefly explain the dashed red line in the caption.
Fixed.

L1391: “are” biased?
Fixed.

L1744: But didn’t you use the 1 km OLCI products?
Fixed.

L1745: Check whether Landsat should be all capitalized
Fixed.