Dear Authors,

Thank you for your revised version, which I have read in great detail. I am very satisfied with your very careful consideration of the reviewers' comments and for your thorough revisions. Your paper is a useful contribution to firn microstructural characterization and I expect I will soon be able to accept it. Before I do this, however, I would like you to consider the following comment and other minor comments.

Dear Dr. Domine,

Thank you for carefully reading our paper. We agree with many of your comments and suggestions, and we have edited the manuscript accordingly. We believe that your comments have further improved the quality of the paper. Please see our responses in blue text, with changes that we have made to the text of the paper in *italics*. Additionally, we have uploaded the revised manuscript using the "Track Changes" function so that our edits will be readily apparent to you.

An important part of your discussions deals with the comparison of traditional and NIR grain size. As detailed in the comments below, I am not fully convinced by your geometric considerations to explain the larger NIR grain size. I would like you to consider that NIR probes only ice-air interfaces, while in firn a large fraction of grain perimeters are ice-ice interfaces, in fact grain boundaries, which NIR does not detect. Therefore, the lower surface to volume ratio yielded by NIR leads to increased apparent grain size. This artefact is much less important in most snow types where grain boundaries are a much smaller fraction of grain perimeters. I therefore think that interpreting NIR reflectance as grain size is not adequate, as this implicitly implies that there are no grain boundaries. I would rather recommend interpreting NIR reflectance in terms of specific surface area (SSA), which quantifies the air-ice interface without any implicit, and arguably erroneous, hypothesis on grains size or shape. I am fully aware that this may not be the current standard in the field, but I think concepts must evolve as investigating techniques improve and produce different information that should not be interpreted in terms of previous concepts. Please take the time to reflect on this, I am open to discussion.

We appreciate your perspective and recognize that large fractions of firn grains may have grain boundaries. We believe that due to the retrieval method effective grain size is the preferred and most straightforward way to define the parameter we present based on the retrieval, and has a solid optical basis and a long legacy in the literature. Both SSA and effective grain size are ways to indicate ice absorption/path length through particles, and there is a well-defined relationship between the two metrics - so translating to SSA would be possible (as we note in the Introduction). However, because the modeling is based on a collection of spherical particles that reflect the same as was measured, the effective grain radius is a more accurate descriptor of the retrieval.

This method has been applied to snow, firn, and ice (e.g. Donahue et al., 2023, Cook et al., 2020, Bohn et al., 2021, Skiles et al., 2023, Negi and Kokhanovsky, 2011) with the understanding that effective grain size is not directly related to physical/observable grain size, which we do explicitly recognize in the paper (Line 343: *Effective and traditional grain sizes are not expected to be the same.*) The relationship between the two, as presented in this paper, is still interesting and worthwhile to present because the two measurements are available for the same cores, and could be useful as an empirical conversion.

We have added the following text to the end of Section 2.3:

"We directly report the r_e values retrieved through this forward modeling approach, but reiterate that this is an optical property; because of the field of view, and mm-to-cm penetration of NIR light, the resultant pixel reflectance can represent light interactions with multiple grains."

We do agree with your point that differences between effective and traditional grain sizes likely arise because of the differences in measurement techniques. The NIR-HSI is measuring ice absorption, while a "traditional" measurement is made by measuring observable individual grains so it is understandable that these differences could lead to a high bias in effective grain radius. In response to your additional comments below, we have simplified the discussion of the effective/traditional differences in the main text and moved the grain geometric correction to the appendix.

Please also consider the minor comments below. Line numbers are those in the tracked changes version.

Line 13. I suggest adding « effective » to grain size here since this is now what you produce.

We have updated this sentence to now read: *"We leverage the relationship between effective grain size, a measure of absorption by ice grains, and near-infrared reflectance to produce high-resolution (0.4 mm) maps of effective grain size and ice layer stratigraphy."*

Lines 54-55. "Changes in grain size also create differential forward scattering of green light used in laser-altimetry surveys, which can introduce elevation biases by delaying photon returns to the altimeter (Smith et al., 2018)." This process takes place in the top snow layer, about the top 10 cm, and not in the firn. You are not studying this layer and your results are therefore not relevant to this problem. I recommend deleting this. You can readily simulate the irradiance profile at 500 nm using TARTES https://snow.univ-grenoble-alpes.fr/snowtartes/index to realize that with about 10 ppb of soot, the e-folding depth is around 10-15 cm, depending on the snow properties you choose (density and SSA).

Thank you for providing us with the link to this useful tool. Firn can be exposed at the surface of some alpine glaciers and at low elevation regions of Greenland's percolation zone according to our definition of firn, which is most commonly used by researchers studying ice sheet mass balance (The Firn Symposium Team, 2024). Where firn is exposed, firn properties may affect optical scattering in these locations, which also happens to be places where glacier/ice sheet elevation changes are large. However, we do recognize that these impacts are not widespread across most of the ice sheet. Because this sentence does not change our study's motivation, results, or conclusions, we have removed the sentence to prevent any confusion from readers.

Line 63. "altimetry-based mass balance assessments". Likewise, if you are referring to the determination of the level of the surface by visible radiation, ice layers will only affect this determination if they are near the surface. If you are now referring to radar, please specify to avoid confusion.

We apologize for the confusion; thanks for requesting the clarification. We have changed this phrasing to *"mass balance assessments from microwave radar surveys"*.

Line 137. How about specifying how thick those ice slabs are?

This sentence now reads *"... and zones with thick (≥ 1 m) ice slabs..."* as this is how ice slabs are defined by MacFerrin et al. (2019).

Line 206. "the the"

Thanks for catching this typo. We have removed the duplicated word.

Line 212. Eq.1. You therefore only use the data between 962 and 1092 nm. Why then scan the whole 900-1700 nm range? By the way you could also fit the whole spectrum to determine the grain size, for example using TARTES mentioned above. You could easily try it. I am pretty sure it would be at least as good as using the Nolin-Dozier method. That method is good if you only have NIR data, I guess if only a Si photodiode is available, which was probably the case for Nolin and Dozier. If you have an InGaAs photodiode that yields SWIR data to 1700 nm, I am really not sure the Nolin-Dozier method is the best. You do not need to address this in your revision. I am just bringing this to our attention for future research.

Thank you for the comment, but we note that we collected the full range from 900 to 1700 nm because that is how the instrument collects data and is not programmable otherwise. There are pros and cons to the Nolin-Dozier method as well as other retrieval methods, including the spectrum fit method as you suggest. In addition to it being a well established method, we justify our selection of the Nolin-Dozier method over other methods, because it leverages an area of the spectrum with high signal to noise ratio for the instrument used in this study, and the shape of an ice absorption feature that is said to be less sensitive to the absolute magnitude of reflectance as a result of illuminations conditions. The full spectrum fit, however, is very susceptible to the absolute reflectance values and would equally weight NIR bands with high signal to SWIR bands with low signal. Co-author Chris Donahue has experimented with this type of retrieval in a laboratory under idealized lighting conditions and has found spectrum fitting in the SWIR region to be problematic.

Line 212. "in the absence of ice absorption". I am not sure this is the best wording. Ice absorbs significantly at all NIR and SWIR wavelengths. You probably mean "if the 1030 nm band were removed"? Please clarify.

We appreciate your request for further clarification. We have reworded this phrasing to read: *"... represents the reflectance spectrum as if the ice absorption feature at 1030 nm were not present"*.

Line 215. How are your grain size results dependent on the SNICAR optical parameterizations, i.e., on the g and B values? You may compare the SNICAR values to those in Robledano et al. (2023) https://doi.org/10.1038/s41467-023-39671-3 and test whether the latest research would change your final results.

The results from the TARTES parameterization in Robledano et al. (2023) is unfortunately not directly translatable to the SNICAR model. Additionally, the Robledano et al. (2023) parameterization is for seasonal snow, where all snow samples examined were less than 400 kg/m^3, and are likely not suitable for firn. The SNICAR model represents grain shape using two alternative parameters known as shape factor and asymmetry parameter (He et al., 2017). There is currently an ongoing study to parameterize these shape parameters for SNICAR, similar to Robledano et al. (2023), and we are eager to see those results. We did, however, test the 4 selectable shapes within SNICAR that span a range of snow types and found that the spherical particle was the best.

Line 218 "Snow density negligibly affects snow reflectance". This is only true for a semi infinite homogeneous snow layer. In fact, it has no effect. Anyway, your sentence is correct in your context, since your core thickness is probably at least 3 times the e folding depth, but the statement is not generally true as suggested by your writing.

Given that density is an input to SNICAR, it is worthwhile to offer a simple and concise explanation as to why a constant value is justifiable.

Line 268 "by by"

Thanks for catching this typo. We have removed the duplicated word.

Figure 5a. The OIB flightlines appear black to me, not blue.

We now write "*navy blue*" in the figure caption. This may also appear clearer when the full image is uploaded for production.

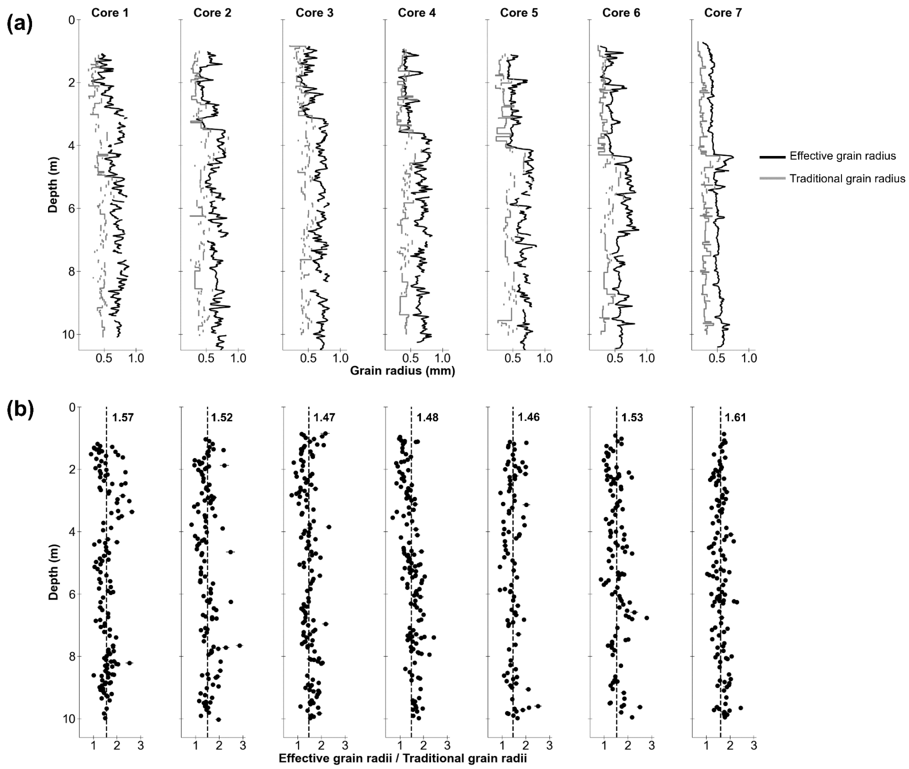
Lines 371-372. Both Lehning 2002 and Vionnet 2012 have produced models for seasonal snow, not for firn. The physical processes determining grain growth are different. References for firn models would be more appropriate.

You are correct that these models were developed for seasonal snow. However, the SNOWPACK model is commonly used to simulate ice sheet firn (e.g., Groot Zwaaftink et al., 2013; Steger et al., 2017; Dunmire et al., 2021; Keenan et al., 2021; Thompson-Munson et al., 2023; Banwell et al., 2023). We cited Lehning et al. (2002) and Vionnet et al. (2012) because these publications present the traditional/optical grain size evolution descriptions used in the SNOWPACK model. We agree that there are likely issues in the simulated grain size evolution because the models were developed for seasonal snow, and we have had some initial conversations about this with researchers that contribute to the model development/applications. To make it more clear that many firn modeling studies rely on model parameterizations developed for seasonal snow, we have revised this section to read:

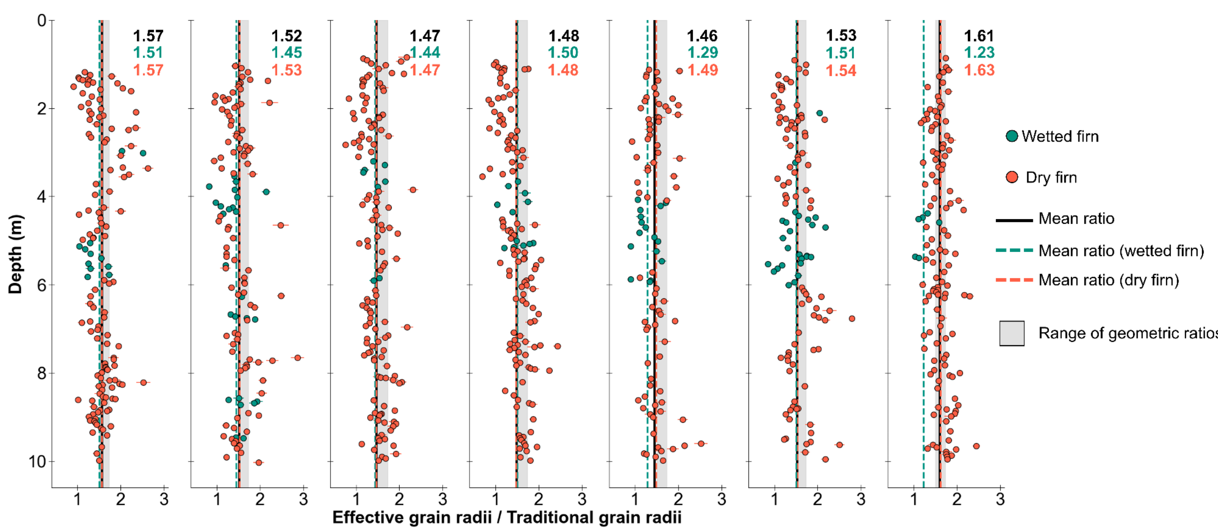
"Traditional grain size measurements are often taken in the field, and these types of measurements have validated grain growth parameterizations (Lehning et al., 2002) in a physics-based land-surface snow model, SNOWPACK, that has been applied extensively to simulate the evolution of the firn layer on ice sheets and ice shelves (e.g., Groot Zwaaftink et al., 2013; Steger et al., 2017; Dunmire et al., 2021; Keenan et al., 2021; Thompson-Munson et al., 2023; Banwell et al., 2023). Furthermore, SNOWPACK also evolves effective grain size based on the description by Vionnet et al. (2012), but this parameterization is still dependent on empirical relationships with traditional seasonal snow grain size. Our effective grain size dataset provides a valuable opportunity to further investigate the discrepancies and the empirical relationship between traditional and effective grain size measurements of firn."

Figure 6b. I really have trouble telling the purple from the black in the numbers at the top. More contrasted colors may be useful. I am not color-blind, by the way.

Because we no longer focus on the geometric correction in the main paper and exclusively focus our discussion on methodological differences driving the effective/traditional discrepancies, Figure 6 now looks like:



The panel originally in Figure 6b has been moved to the appendix where we discuss the geometric correction. We have updated the color scheme so that hopefully the different sections appear clearer.



Lines 379-383. This is not totally correct. You are mixing up snow and firn metamorphism, where processes are very different. In firn, the large radii of curvature mean that vapor diffusion is not always predominant, and surface diffusion can become important. Please see e.g., Maeno

and Ebinuma (1983) doi:10.1021/j100244a023. There are several other references on the topic. The temperature gradient in firn is too low to produce faceted crystals. The reference to Fierz 2009, which is for seasonal snow, is not adequate. This paragraph and the next one could probably be condensed to retain only the aspect actually relevant to your study. I suggest just focussing on firn data and processes. You may then, and separately, extend this to a snow discussion, but such a snow discussion does not seem useful to me. I however let you decide on this last point, but please remember that, for a scientific paper, the shorter, the better.

Thank you for raising this point. We agree that this could be written better. We have significantly revised this section of text, and we now focus more on firn processes and how firn grain shape and bonding results in observed discrepancies between effective/traditional grain sizes. We do, however, mention the differences between snow and firn grains at the end of the paragraph, since we believe that this is an interesting result that to our knowledge has not been shown before. Given that seasonal snow models are applied to simulate ice sheet firn, we believe that the different relationship between effective and traditional grain sizes in snow and firn is important to note.

The revised section now reads:

Effective and traditional grain sizes are not expected to be the same since they are based on different grain properties. Effective grain size retrievals based on NIR reflectance spectra rely on measurements of ice absorption and are ultimately a measurement of optical path length. Alternatively, traditional observations measure the cross-sectional extent of the grain. Previous limited studies report effective snow grain sizes to be ~ 2 – 20 times smaller than traditional grain radii (e.g., Painter et al., 2007; Langlois et al., 2010; Leppänen et al., 2015). However, we find that effective grain sizes are consistently larger than traditional grain size, while the magnitude of the effective-to-traditional radius differences are smaller than in snow. This suggests a different relationship between the two measurements in snow and firn.

The high bias in effective grain size retrievals compared to traditional observations of firn grains is likely caused by the methodological differences in measurement techniques compounded by the shapes of firn grains. While firn grains are typically spherical or spheroidal (Alley, 1997; Meussen et al., 1999), similar to the shape assumed to generate our grain size lookup table, sintering processes further reduce total surface area as firn grains become bonded together. In relatively low density firn (~350 – 550 kg m^{-3}), bonds between grains will form primarily from grain-to-grain vapor diffusion until the radii of curvature become large enough that surface diffusion becomes an important mechanism driving neck growth (Maeno and Ebinuma, 1983). The lower specific surface area of bonded grains, where the space between the grains has become ice-filled compared to two individual grains with an air-filled grain boundary, will cause greater NIR absorption and thus the retrieval of a larger sphere with the same surface-to-volume ratio (Wiscombe and Warren, 1980). The discrepancies between effective and traditional grain *sizes can be further increased when the firn cores are cut into half-round sections. It is highly unlikely that all firn grains are cut exactly through the center. Therefore, a traditional grain size measurement will calculate the diameter of a smaller cross-section than if the spherical grains were cut through the middle. Because NIR wavelengths penetrate to a depth of a few grain diameters, artifacts introduced by firn core cutting will not significantly influence the absorption and resulting grain size retrieval. Therefore, in firn, we largely attribute the >1 ratio of effective-to-traditional grain size to both increasing the retrieved effective grain radius by firn grain bonding that decreases specific surface area of the firn medium, and decreasing the traditional grain measurement by firn core cutting that exposes cross sectional areas smaller than perfect hemispheres. Our reported biases still suggest a distinctly different relationship than seen in seasonal snow grain size comparisons. In seasonal snowpacks, large temperature gradients promote the prevalence of kinetic growth forms with high surface area-to-volume ratios. The thicknesses of these grains can be 50 times smaller than their surface extents and these forms may also be hollow (e.g., Taillandier et al., 2007). The effective grain diameter of needles and plates are similar to their thicknesses (Mätzler, 1997), while a traditional measurement of their maximum extent would be much larger, which likely results in smaller effective grain sizes compared to traditional measurements.*

Lines 398-421. Honestly, I am not too thrilled by your explanation of the grain size difference, as mentioned above. First of all, is there any actual observational evidence of faceted forms in firn? I have not seen any but am open to evidence. The evidence you propose seems to be just a modeling choice that has no real basis, if I understand correctly. There is of course the obvious fact that traditional grain size uses a section where grains are not always cut in their center, therefore showing a smaller section. NIR reflectance on the other hand, penetrates several grains thick, so that it probes to a depth of several grains. As detailed in the start of my comments, I suggest you consider that NIR probes air-ice interfaces only, and does not see grain-grain boundaries, while traditional grain size does consider these boundaries. Since grain boundaries make up a significant fraction of a grain perimeter in snow, NIR will inevitably produce a negative artifact intrinsic to the method. If you agree to this, you may mention it in your conclusion. And by the way, I then think that translating NIR reflectance in terms of specific surface area (SSA) rather than grain size would be much more physically meaningful. Grain size implicitly implies that the whole perimeter of a grain is an ice-air interface, which is not true. SSA makes no assumption. The principle of the method you use to determine grain size was developed for snow, where most of the perimeters are indeed ice-air interfaces. Now that you are moving to firn, I extremely strongly recommend that you adjust your interpretation to the reality of that new medium.

We appreciate your suggestion to revise this section. As discussed above, this section has been substantially revised to focus on methodological differences driving the traditional/effective grain size discrepancies.

We do wish to note that our data come from the top 10 of the firn column, where the firn layer has not yet become isothermal. While seasonal temperature gradients will create vapor pressure gradients leading to vapor diffusion, firn temperature data from Humphrey et al. (2012), Charalampidis et al. (2016), and Harper et al. (2023) show that meltwater or rainwater infiltration can create very steep (> 10 C/m), localized, and deep (> 5 m) temperature gradients that can promote faceting. Furthermore, observations of depth hoar layers in firn by Benson (1962), Alley (1988), Satow and Wantanabe (1990), and McDowell et al. (2023) provide additional evidence that faceting can occur in firn.

We still believe that the high level of similarity between our observations and the idealized grain shape proposed by Humphrey et al. (2021) is interesting, and both reviewers actively engaged with this section in their reviews. Because we agree that the shape was somewhat arbitrarily-chosen and highly idealized, we have moved this short discussion to the appendix. This shortens the main text of the manuscript, yet still provides some discussion with which future work can engage. In the main text of the manuscript at the end of this section, we now write:

"While we largely attribute the discrepancies between effective and traditional firn grain size to the fact that NIR reflectance is governed by ice absorption and traditional grain size is controlled by observable cross-sectional area, we notice that a simple geometric correction relating the grain shape proposed in the grain scale model of meltwater movement through firn by Humphrey et al. (2021) can explain a majority of the effective-to-traditional grain size differences in our dataset. Because the truncated octahedron grain shape proposed by Humphrey et al. (2021) assumes a highly-idealized firn grain, we dedicate a discussion developing a geometric-based correction factor relating effective and traditional grain size in Appendix A."

We add some justification of examining forms with hexagonal facets in the appendix and write:

"To examine whether effective and traditional grain sizes in the firn cores can be explained by the geometric treatment of effective grains as spheres, we assume firn grains take the shape of truncated octahedra proposed by Humphrey et al. (2021) in their grain-scale model of meltwater movement through firn. While highly idealized, this shape treats firn grains as semi-rounded forms containing hexagonal faces. While deep firn is isolated from large temperature-driven vapor pressure gradients, temperature data from the shallow (< 10 m) firn column from Humphrey et al. (2012), Charalampidis et al. (2016), and Harper et al. (2023) show that meltwater/rainwater infiltration and refreezing can create steep temperature gradients conducive to facet formation below the depth of large seasonal temperature gradients."

Line 415. "The the"

Fixed. Thanks again for catching the typo.

Line 437. I am not sure Fierz 2009 applies here.

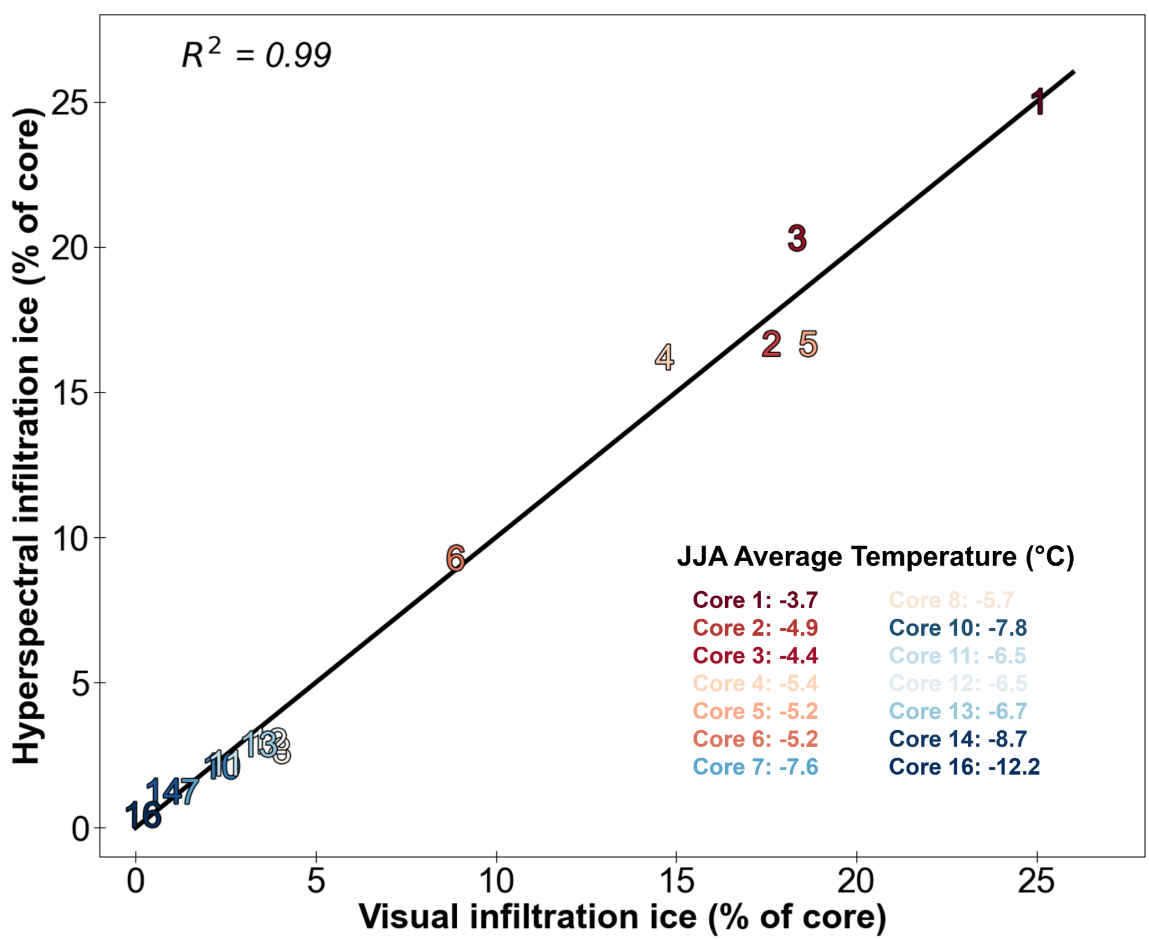
Good point. We now say *"... according to the classification scheme of Benson (1962)..."*

Lines 438-441. Would not this insertion be better placed together with the previous insertion? Your decision.

Thanks for the good suggestion to move the text. We have replaced the insertion we made on line 432-433 with this section since it essentially duplicated the meaning.

Figure A4. Why use the mean annual temperature? Would not the summer temperature be more appropriate? Or even just the July-August mean temperature? Only periods when melting may take place are relevant, it seems.

This is a fair point. We have changed the shading to the summer (June, July, August) average temperatures.



Line 520. Should you refer the reader to Figure 5 here? Your call.

We now include a reference to Figure 5.

Lines 541-542. Are units correct here? Or m rather than mm? No dot please, it is a unit.

Thank you for catching this oversight. The units are in m w.e. yr^{-1}. We have made these changes and removed the periods.

Lines 534-545. I am not really convinced by your explanation of the absence of a melt layer in core 11. Do you mean that the high accumulation rate would have decreased the temperature gradient, which would have reduced surface grain growth and therefore increased albedo? This dos not seem to be written very clearly. Other factors would then come into play. For example, more light snow of lower thermal conductivity will reduce downward heat loss during the warm spell and lead to greater heating of surface snow. This effect would be opposite to the one you propose. A quantitative assessment including all energy-relevant processes would be required to reach the conclusion you propose, if I understand it correctly. How about that the different snow structure, likely lower density, would have favored preferential flow rather than matrix flow, so that some areas would be minimally affected by wetting? Core 11 could then have been drilled in a little-affected spot. Just a thought, your decision. Your subsequent paragraph in fact almost leads to this same suggestion.

Thank you for raising this point. We agree that the inclusion of a hypothetical albedo feedback at the end of this paragraph was a bit lazily pitched. We have removed the final sentence from this paragraph, so that the focus is just on Core 11 being in a different climate setting from the other cores, which could be why the 2012 melt signal is not evident in this core.

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