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.

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.

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.

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).

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.

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

Line 206. “the the”

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 is your revision. I am just bringing this to our attention for future research.

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.

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.

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.

Line 268 “by by”

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

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.

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.

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.

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.

Line 415. “The the”

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

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

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.

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

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

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.