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The Cryosphere Discussions

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Interactive comment

Interactive comment on "Refinement of the ice absorption spectrum in the visible using radiance profile measurements in Antarctic snow" *by* G. Picard et al.

Anonymous Referee #2

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This paper presents updated estimates of the optical ice absorption spectrum in the visible based on a large set of new measurements of light extinction near the surface in Antarctic snow. The very weak optical ice absorption in the UV and visible creates experimental challenges which different techniques have been used to overcome. Lab measurements have been of limited use due to the required long path lengths and very pure ice samples. Measurements in deep natural ice in Antarctica by AMANDA (e.g., Ackermann et al., 2006) found that absorption in the visible is still dominated by dust contamination even for this extremely clean ice so intrinsic absorption by pure ice is experimentally hard to disentangle from absorption by impurities. Therefore, it has not been possible to definitely determine how strong optical absorption by pure ice actually

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Discussion paper

is and existing measurements can in some sense only set upper limits, unless it can be shown that impurities and instrumental effects are negligible. Warren and Brandt (2008), referenced as IA2008, developed a technique to measure absorption through radiance measurements in highly-scattering Antarctic snow. Their measurement was in the end limited to a single snow layer, but they found an even weaker absorption than measured in deep ice by AMANDA. The current paper uses a similar technique as Warren and Brandt but improves on important aspects: a much larger data set, collected at different locations, is used; instrumental effects related to inserting the optical fiber assembly into the snow is studied with detailed 3D simulations; and they also use more sophisticated Bayesian statistical techniques to fit absorption parameters using all data at once. This work is very valuable in that it adds more information to the question of optical ice absorption near the minimum and also sheds more light on possible systematic uncertainties involved with such snow measurements. The paper should definitely be published, but I have some substantial comments on the current version.

Major comments:

1) The experimental technique, data collection, analysis, and bias discussion is thorough and described overall in a clear way (with some exceptions discussed below). My main comment concerns the interpretation of the result. Is the claim that these measurements arrive at an estimate of the absorption coefficient for pure ice, i.e. the intrinsic absorption by ice without impurities? It would be hard to make this case, considering previous measurements. We know from AMANDA that ice absorption which is still dominated by dust contamination is weaker than these new results. Therefore, the even weaker absorption in IA2008 logically comes closer to the true absorption for pure ice. The weakest absorption measured in AMANDA dips below 5×10-3 m-1, but this is still in ice with considerable dust and the spectral shape is the power law expected from absorption by dust. I would therefore expect pure ice absorption in the visible to be even weaker. The new BAY (clean) measurements show much stronger absorption than the cleanest AMANDA depths. We know that pure ice is at most as absorbing as

the AMANDA ice. So the BAY estimate seems too absorbing. In this context it would also make sense to soften the definitive statement (Page13Line22) that "it is impossible to obtain absorption coefficient as low as IA2008". The large difference between the very weak IA2008 absorption and these new measurements with a similar technique is not understood, but logically IA2008 should be closer to true ice absorption since the AMANDA measurements set an upper limit.

2) I would have liked to see the plotted data and simulation results shown with estimated uncertainties (error bars and bands) whenever possible to aid the interpretation. Can the statistical and systematic uncertainties of the absorption spectra be quantified and added to the figures? A related question concerns the difference between standard deviation (SD) and standard error on the mean (SEM) for a measured variable. Is it correct to say that the BAY method produces SEM and the WBG method produces SD so the spreads in Figures 5 and 6 show different but equally interesting statistical properties of the measurements?

3) The use of Monte Carlo simulations to study possible biases (systematics) due to instrumentation effects is excellent and thorough. The authors show how the radiance profiles are affected by the measurement rod and a possible void/air gap between the ice and the rod, and how these biases depend on snow properties and therefore location. The discussion of the effects on radiance profiles is thorough and persuasive. However, it would really help in understanding and quantifying these effects to also show how the measured absorption spectra are affected by these systematics. It is finally quantified in terms of absorption in Section 3.5 and Fig 16 but this could be done at every previous stage also. I would have liked to see accompanying plots that show how the measured absorption depends on rod, depth, void, snow properties, and even as a function of true absorption. In this way, one could quantify this as a systematic uncertainty on the measurements and add this as an error band.

Minor comments:

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Section 2.2: Some questions about the data selection:

1) How exactly were the homogeneous zones selected? Not all profiles look perfectly linear in the selected (gray shaded) zones.

2) How was the absorption fitted in each zone? A linear fit over the entire zone, or averaged over shorter linear fits to adjacent subsets of readings?

3) Where both descending and ascending profiles used and treated the same? Did they yield consistent results or were there systematic differences?

4) What is the explanation for the often quite large non-homogeneous zones, not close to the surface? Sometimes the whole profile is discarded. What was wrong in those cases, other than that they did not look linear in a visual inspection? Stated slightly differently (and a bit more provocatively): if there was nothing known wrong with the snow in the discarded zones other than that the profile did not look linear, how do you know that the snow in the selected zones is suitable for this measurement?

Page6Line31+: With the BAY method, the authors chose as prior a normal (in log scale) distribution with the average between IA1984 and IA2008 as the mean and as standard deviation the difference between the two (plus an extra SD factor for longer wavelengths where the two estimates agree). Leaving aside the impact of this choice on the result, the physical motivation seems somewhat flawed. The IA1984 estimate is based on lab measurements that are now known to be skewed (to stronger absorption) by scattering effects. The later AMANDA measurements showed that pure ice absorption must be much weaker than IA1984 and could even (in the absence of dust) be as weak as in IA2008. To use IA1984 to define the prior is therefore questionable. Given this objection, it would be relevant to see how much the choice of prior affects the measurement. Since the BAY results (Fig 6), which use this prior, end up close to the average WBG results (Fig 5) the effect of the prior is probably not too strong. However, what would happen if instead the difference between the weakest AMANDA absorption and IA2008 were used instead?

Page8Line27+: Isn't the good agreement at longer wavelengths (Fig 4) completely (not only "partially") explained by the methodology, i.e. that absorption is assumed to be known at 600 nm?

Page8Line31: If the empirical absorption model describing the AMANDA data holds beyond the deep ice, the spectral absorption shape is a combination of a falling power law due to dust absorption at shorter wavelengths and an exponential rise due to molecular absorption at longer wavelengths. The power law shape is fixed but the strength depends on dust concentration. This means that the cleaner the ice, the lower the power law part and the shorter the wavelength at the absorption minimum near the crossover point. This seems to be the trend in the measurements. There is no convincing evidence that any of the measurements are describing pure ice, so the minimum is not known. The minima in the measured spectra depend on dust contamination.

Page8Line34+: The description of the shape of the measured spectra is exactly that of the two-component model describing the AMANDA data. The small scatter at long wavelengths is because there the absorption of ice is measured, whereas at shorter wavelengths the absorption will depend on dust contamination. The results (Fig 5) confirm this picture. The result in Figs 7 and 8 further strengthen this interpretation, showing that the measured absorption depends on distance (and direction) from man-made activity at stations and therefore are most probably affected by dust contamination. In other words, the cleaner the ice, the weaker the absorption. This seems to confirm that dust is still a significant determinant of absorption below 450 nm in these data.

Page8Line37+: It is stated that the WBG absorption spectra (Fig 5) have different measurement quality and are thus not equiprobable. This is undoubtedly true, but uncertainties due to measurement quality should be separated from differences in spectra due to different dust contamination levels (because data is from different locations). In experimental results this "measurement quality" should be reflected in measurement uncertainty (error bars or bands). All spectra are shown as lines, without indicated uncertainty. Perhaps if measurement uncertainties (statistical and systematic) were added, the spectra would all be consistent with the uncertainties? Probably this would be true for a given location but not between locations.

Finally, some minor language points:

In two places: a fiber optics -> an optical fiber

P13L31: back carbon -> black carbon

P10L37: neither -> either, nor-> or

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