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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 #1

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This study presents new estimates of visible ice absorption coefficients, using a large number of measurements of light extinction in Antarctic snow. Precise estimates of the UV and visible absorption coefficient have eluded the scientific community because ice absorbs so weakly at these wavelengths, presenting numerous analytical challenges. This study builds on that of Warren and Brandt (2008) by (1) applying a larger set of measurements, (2) applying Bayesian statistical techniques that incorporate measurements at a larger number of wavelengths, and (3) conducting a rigorous modeling assessment of possible biases introduced by the presence of the fiber optic sensor and housing rod in snow. The current study finds that ice absorbs more strongly in the short-wavelength visible than found by Warren and Brandt (2008), though for unknown

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reasons. Overall, this is an important and very thorough study, certainly worthy of publication. The assessment of potential sensor biases using 3-D Monte Carlo modeling seems particularly rigorous and informative, and I think this aspect of the study will help inform past and future measurements of radiance extinction with depth in snow. I also expect that the new estimates of ice absorption spectra, available as supplementary data to the paper, will be widely used in the scientific community. It would have been satisfying if the authors had presented a convincing reason (or set of reasons) for why their estimates differ from those of Warren and Brandt (2008), but such an assessment may not be possible, or is otherwise beyond the scope of this study. It seems possible or likely that a combination of factors contributed to these differences. Below are some minor issues for consideration. Overall, I think this is an excellent study.

Minor comments:

p1,6: "larger than IA2008 by one order of magnitude..." - Larger at 400 nm, or averaged over the spectra? Please clarify.

p3,1-2: The inference from the quotation from Warren et al (2006), suggesting that more measurements should be made in snow further from sources of contamination, is that IA2008 measurements could be biased towards being too absorptive, but in fact this paper shows the opposite. Although this quotation is presented merely for motivation, reasons for the different findings between these studies appear to remain unknown. In general it would be helpful to offer (elsewhere in the paper) any additional insight or speculation that you have on reasons for the differences between these two studies. Convenient places for such discussion include sections 3.1, 3.4, and section 4.

Equation 1: Technically, "I(z=0)" should be "I(z=0,λ)" for consistency with the left-hand side of the equation.

p5,20: Please clarify what is meant by "the first two factors on the right hand side". The square root term makes this statement a bit ambiguous.

p5,19 and equation (3): It appears that the σ terms refer to extinction coefficients of ice+air. If so, please explicitly clarify this. Otherwise, the distinction between σ_a and γ_{ice} is unclear, as both are absorption coefficients with identical units.

Equation (3): It would be helpful to briefly explain the conditions under which the middle expression of Equation 3 apply, as it appears to be an approximation.

p5,26: Does parameter B have much spectral dependence?

Equation (6): Please define the symbol α_n (perhaps accomplished most conveniently in the description of Equation 4).

p6,17: "σ measures the observation errors which are assumed identical for all the measurements." - Are there any conceivable or plausible conditions where the observation errors would depend on one or more key variables, such as depth in snow, surface irradiance, SSA, snow density, etc? In other words, what is the validity of this assumption?

p6,34: "The most likely value is taken as the average of these two parameterizations" - Do you mean that the available supplementary data are taken as the simple average of the WBG and BAY techniques, as applied to measurements collected from this study? Please clarify.

p7,28: Presumably, the distribution of these step lengths is such that the extinction transmittance obeys Beer's Law. It could be worth mentioning nonetheless.

p7,31: Briefly, how is bias avoided? Is it simply because the cutoff threshold is sufficiently small?

p8,15: Mode 2 (inverse tracking) is a creative solution to this problem.

Section 3.3: This is a very informative and interesting analysis. It could also be very useful if you can offer any insight into how previous measurements of light extinction in snow should be re-interpreted, in light of this analysis.

12,31-32: This last sentence is unclear to me.

Section 3.5 and Figure 16: This analysis is unclear to me. Please consider revising the description of this sensitivity study for clarity. In particular, the determination of ice absorption using different ice absorption data (caption of Fig 16) seems circular. The third sentence of this paragraph (section 3.5) appears to explain the technique, but I don't quite understand what is being done here.

13,28-30: This sentence should be fixed for clarity.

15,17-18: Could the large underestimations of ice absorption caused by rod-snow interactions be sufficient to explain the discrepancies in absorption between this study and Warren and Brandt (2008)? Insight on this would be helpful.

Figure 6 caption: What is the site of these measurements? Is it the same as in Fig 5?

Figure 7: It is a bit difficult to match the colors of the lines to the legend, because they are so similar, but perhaps this is not important.

Figure 13: Needs a legend or color description in the caption to distinguish the two lines.

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