Reviewer #1

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 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.

Yes at 400nm and around. This is now indicated in the text.

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.

We understand this request and Reviewer #2 had a similar one. It is indeed frustrating to obtain such a large difference and conclude this work without providing a satisfactory explanation. We tried to find one, though. For this we carried out a very detailed analysis of our protocol, so we were able to estimate the bias and uncertainty of our experiment (e.g. the effect of the rod, all the experimental details, etc). We also performed radiative transfer calculations to evaluate the possible impact of light absorbing impurities. Even if these evaluations are somewhat imprecise, our conclusion is that these errors are insufficient to explain the difference with IA2008. Conversely, we haven't performed a similar detailed analysis on Warren et al. (2006) protocol because we don't have enough information regarding their procedures. We have not identified any obvious flaw in the papers but undocumented details of their protocols could still be critical. Regarding AMANDA, the difference with our estimate is much smaller. It seems difficult to explain it as long as the difference to IA2008 remains unexplained.

Only the authors of these studies could reasonably reassess their error budget but we don't expect

much progress on this side because it is a difficult task.
We believe instead that it is easier and more promising to improve the experiments or design new ones to obtain complementary independent assessments. In this perspective, we hope that our paper, by showing up the problem, will motivate such future work where extra care on the protocol (and its description) will be taken. In this paper, we prefer to refrain us from adding speculations on others' experiments. We want to stick on the factual results we got.

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

done

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.

This is solved by reversing the sentence and explicitly naming the factors.

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.

We have added "of the snow" after equation 2 and "σ_s and σ_a are the scattering and absorption coefficients of the snow." after equation 3.

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.

We have added:
"These equations assume that snow grains are randomly oriented particles, weakly absorbing and the real part of refractive index is nearly independent on the wavelength (Libois et al. 2013)."

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

No, because it mainly depends on the real part of the refractive index and on the shape of the grains. We have changed the sentence as follows: "where ρ_{ice}=917 kg m^{-3} is the ice density and B=1.6 the absorption enhancement parameter which has very little dependence to the wavelength.".

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

It is implicitly defined by "All the variables (excepted zn) are random variables". Equations 2 and 4 inspired the form of the equation 6 but the latter is not tight to a particular model of extinction. The only constrain on the model is the proportionality to the square root of the ice absorption. So, α_n is not related to Equation 4 and is nothing more than a "lump" proportional coefficient, i.e. a random variable with wide prior in Bayesian framework.

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?

It is likely that the error depends on various factors, especially the quantity of light. This implies a dependence to the wavelength , the depth and the density and SSA between the surface and the measurement depth. However it is quite complex because Solexs automatically adjusts the integration time to minimize the noise, so the relationship between depth and the noise level is not simple (and hardly predictable). It seems difficult to address such a complexity. In addition, adding more unknown variables to represent the dependence to the wavelength and depth would come at the price of an over-parametrization of the inverse problem with possible consequences on the convergence. We didn't try a more advanced scheme because we found that the posterior sigma is very small, indicating that the observation error is very small on average. It's also clear on the profiles in the supplementary figures that the measurements do not seem more noisy at larger wavelength or greater depth.

To highlight that this assumption is a simplification we slightly changed the sentence:
"σ measures the observation errors which, for sake of simplicity, are assumed identical for all the measurements."

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.

This sentence was linked to the previous one but a newline was inserted by mistake. We have reformulated the paragraph:

"The method WBG is indeed equivalent to considering that the prior of absorption is certain at λ_0=600 nm and ignorance at any other wavelengths. Here, we consider that IA1984 and IA2008 are two extreme estimates (Warren, 1984; Warren and Brandt, 2008). Hence, the most likely prior value is taken as the average of IA1984 and IA2008 and the uncertainty related to the difference between IA1984 and IA2008 through a linear function. This function has an offset to represent the uncertainty when the difference is null, that is for λ > 600 nm. Note that since $\gamma(\lambda)$ spans several orders of magnitude in the visible, we compute difference and average in logarithm scale and choose a log-normal distribution for the prior.

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.

The Poisson random process is used to simulate the extinction events (either scattering or absorption) which is one of the processes of the radiative transfer. The Beer's law is different, it is an approximate solution of the radiative transfer valid for an absorptive or a slightly scattering medium (single scattering is sufficient), it does not apply to snow for which multiple scattering is significant.

Since using a Poisson process is common to many Monte-Carlo Radiative transfer models, we have simply added a reference.

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

It's a refinement to avoid bias due to the cutoff whatever the threshold value. In principle we don't need it because we set the threshold to a value lower than the dynamics range of Solexs. This already guarantees that the bias (without Roulette) is small compared to the values we interpret. Nevertheless, using the roulette adds extra de-biasing. It was already implemented in MCML and is not expensive in terms of computation, we just keep it as it is.

We have added some explanations in the text:
Rays with an intensity less than a specified threshold are to be discarded. However, since abruptly discarding all the rays would result in a small bias (smaller than the threshold), a process known as Russian Roulette \(section 3.9 Wang et al. 1995) is applied. A small proportion of the rays (typically p=10%) is randomly chosen, their intensity is multiplied by 1/p and they are re-injected in the normal process of propagation. All the other rays are discarded. The threshold was set to 10^{-5} (the initial intensity of rays is 1) which ensures bias much smaller than SOLEXS dynamic range.

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

It's a common technique in ray tracing to trace from the camera to the sources especially to account for diffuse radiation (large source, small sensor).

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.

We have inserted a comment at the end of section 3.3.2 (was 3.4 in the discussion manuscript)
“It is worth mentioning that the problem of the rod absorption is evaluated here with a specific objective in mind but concerns any study exploiting measurements with inserted fiber optics (King and Simpson, 2001; France et King, 2012). The interpretation of such measurements remains however highly tied to the specific protocol used.”

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

We have reformulated:
“This offset can be understood by the fact that the rod absorption is significant only over a few centimeters above the fiber tip, what was referred to as the "lower" part of the rod in Section 3.3.1. Hence, the gradient of the log-radiance is affected by the rod absorption as long as this lower part is in a transition between two different homogeneous layers.”

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.

We have completely reformulated the section and the figure caption:

“The uncertainty range caused by the rod-snow interactions is evaluated here by considering the two snowpacks investigated earlier: the homogeneous snowpack which leads to an over-estimation of the AFEC and the 25kmE_1 snowpit which results in the opposite. To perform this evaluation, we ran MCML for each snowpack and for each ice absorption spectrum (IA2008 and BAY) which yields simulated radiance profiles as in Figures 9, 14, 15. We then apply the WGB method on these simulated profiles exactly as if they had been measured with SOLEXS. This yields the absorption spectra plotted in Figure \ref{fig_uncertainty_rod}. They ideally should be equal to the ice absorption spectra used as input for the simulations but differ because of the rod absorption and the properties of the snowpack.

Figure 16} shows as expected that the homogeneous snowpack results in an over-estimation of the ice absorption and the opposite is true for the 25kmE_1 snowpack. The range between these two snowpacks is larger (1 order of magnitude) with the lower absorption spectrum (IA2008) than with the BAY estimate (a factor 2 in linear scale) in the range 350—550 nm. In both cases, this uncertainty is larger than the statistical uncertainty estimated from the posterior (Section 3.2) which indicates that the rod absorption dominates the error budget. Nevertheless, the uncertainty ranges around IA2008 and BAY do not overlap which means that the rod absorption effect is insufficient to explain the discrepancy between both ice absorption estimates, at least considering that Warren et al. 2006 rod and snow properties were relatively similar to ours.

Figure caption:
\caption{\label{fig_uncertainty_rod}Ice absorption estimated with the WGB method applied to simulated irradiance profiles with MCML in the presence of the rod for the homogeneous snowpack (triangles up) and the 25kmE_1 snowpit (triangles down) by using IA2008 (green) and BAY (black) ice absorption spectra, respectively, as input.}

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

We have reformulated:
Although these spectra were obtained in different environmental conditions, we consider they are comparable because the ice absorption in the visible is known to be insensitive to pressure and the sensitivity to temperature is only of the order of +1% K^{-1} (Woschnagg et al. 2001).

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.

This question is addressed in the section 3.5 (now 3.3.3) which was unclear and has been reformulated. The answer to the first question is no, we don't believe the rod absorption can explain the difference, except if the rod used by Warren et al. 2006 was much much more absorbent than the one we use in the MCML simulations (which have been done for our rod). Only in case of a much more absorbent rod and a snowpack similar to Snow25E_1, the rod absorption could explain a very large under-estimation.. Moreover, if it were the case, the profiles of irradiance displayed in Warren et al. 2006 would probably be more irregular because of the dependence to the snow properties. All in all, this is very unlikely.

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

yes, exactly the same data are used. We have updated the caption of Fig 6 based on that of 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.

We have tried different color scales but the overlap of different colors resulted in grayish colors. We decided to add Figure 8 which is more informative. The Figure 7 is nothing more than a transition between Figures 5-6 and 8.

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

It has been added.