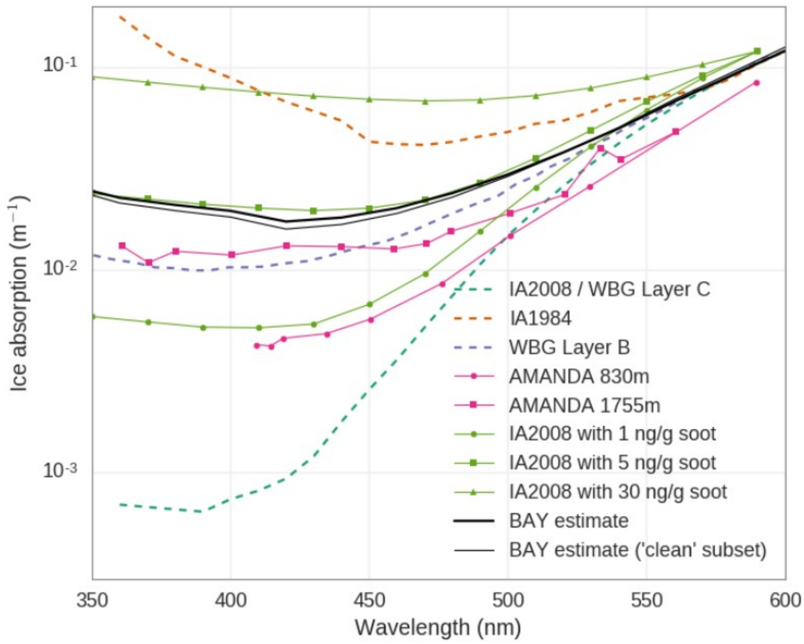
Reviewer #2

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 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.

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.

The first and main goal of our study was to reproduce the Warren et al. experiment because its result is widely used by the snow modeling community. To this end we carried out many more measurements, used a better setup (e.g. finer resolution of the profile, fast execution of the measurements, …), and visited sites further from the Concordia station. The results show a much stronger absorption in the snow compared to Warren et al. 2006 which can not be explained by instrumental and statistical errors. This is the main topic of the result section. The first conclusion of the paper, at the end of the result section, is that the measurements of 2006 are not reproduced by the new experiment and the quotation Page13Line22 tells precisely this.

The second question indeed concerns whether the measured absorption is that of pure ice or not. This is addressed in the discussion section only, because we mostly refer to external results (literature review) to try answering this question. Our conclusion is that impurities content (BC or dust) measured at the surface of the ice-sheet are insufficient to explain the difference between IA2008 and BAY. At depth in the ice sheet AMANDA indeed faced the problem of dust during glacial periods, but modern time background concentrations are much weaker (e.g. dust are ~0.15ng/g eqBC) and could anyway not explain why IA2008 and BAY are different because both are subject to the modern time background. Only a huge increase of the pollution from the station, equivalent to 5ng/g BC could be responsible of such big difference. If the concentration at 25km South were large enough to affect our measurements, ie. 5ng/g eqBC, the concentration at the 4km would be at least (25/4) =6 times larger considering a uniform deposition rate and no effect of the wind. In practice it would be much more because our 25km South site is upwind. Six times is equivalent to 30ng/g eqBC. Such concentration would result in the following absorption (highest green curve):



The fact that AMANDA values are lower than the BAY values is not explained, and we do not have a sufficient knowledge of the details of the AMANDA's protocol and potential biases to further discuss this issue.

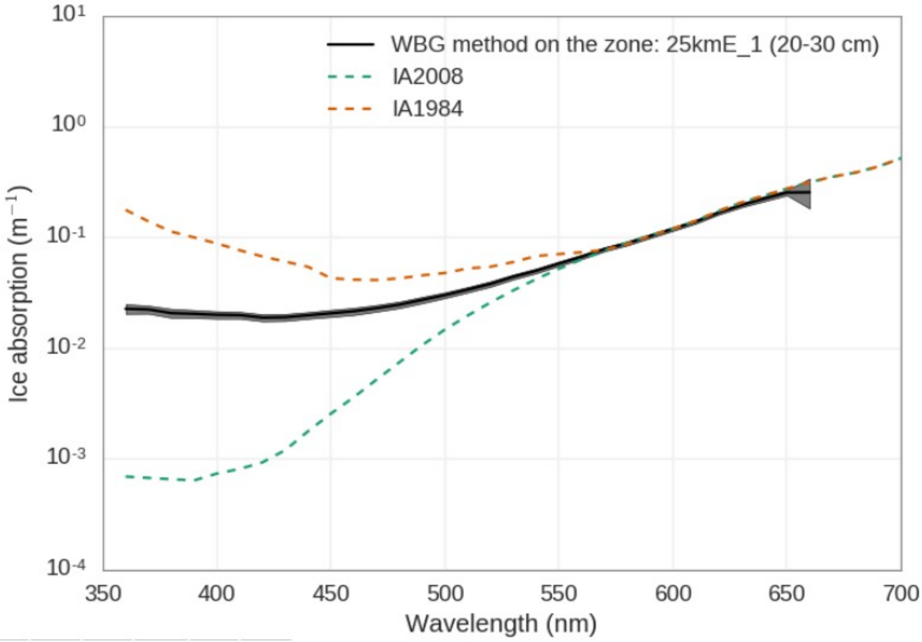
Based on all these results, it is more reasonable to consider that the various measurements available in the literature are biased or that the ice absorption has a hidden (large) sensitivity to some factor (e.g. temperature, …). The last conclusion of the paper is therefore that 1) more measurements with different techniques are still need, the subject is not closed and not simple and 2) meanwhile that considering that IA2008 is based on only one measurement and can not be reproduced, it is more reasonable to use BAY or AMANDA estimates, and to account for this uncertainty in critical calculations.

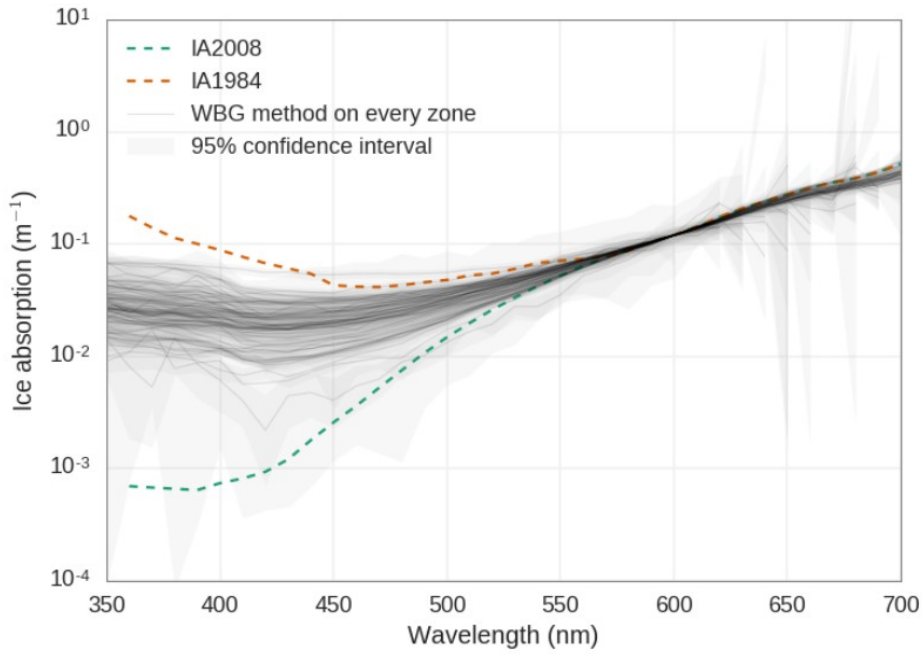
Regarding point 1), we have added the following at the end of the abstract: “future estimations of the ice absorption coefficient should also thoroughly account for the impact of the measurement method.”

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?

Several figures already show these errors. Figures 6 and 7 show set of curves drawn for the posterior distribution, which is equivalent to error bands. Figure 8 also shows the marginal posterior distribution for a given wavelength (a different view of Figure 7 results), where the width of the bell shape curves is exactly the statistical error. The statistical error on the simulations with MCML are not shown because there are weak. By using 10000 rays, the error is typically 1/sqrt(10000)~ 1%. We have added the latter value (1%) in the text as it was missing. Figure 4 did not show statistical errors. We have added this information (see the new figure below) and added a sentence in the text: “In contrast, within this range, the statistical error (95\% confidence interval in gray shade) is very small which indicates the profiles have little noise and the estimation method is robust.”. Figure 5 with confidence interval is also shown here but has not been inserted in the paper, for sake of clarity and another reason evoked below.





Systematic error are more difficult to evaluate, as usual. Figure 16 shows the effect of the instrumental errors and Figure 17 shows the effect of pollution (green curves). Systematic error with MCML can be seen in Figure 9 by comparison with TARTES, it is very small.

As the review mentions, Figure 5 shows many curves which gives a rough idea of the SD but as explained in the text, all these curves have not the same accuracy, they can not be considered as equally valuable. Providing a detailed error analysis of the WGB is not the route chosen in the paper. We preferred to use the Bayesian framework because it is easier to model the errors and the inference/error propagation is more powerful. For instance we can choose the distribution of each variable and possibly even correlation between variable (but this was not done here), whereas the least square used in WGB assumes normal distribution for the dependent variable only. More importantly the Bayesian method really combines all these prior uncertainties of all the zones to produce the posterior. Roughly speaking, it means that thin and noisy homogeneous zones will be given a weaker weight than thicker zones with a perfectly linear log-irradiance profile. Hence, it is not false to say that the BAY method gives a sort of average – and the uncertainty shown in Figure 6 sort of SEM as suggested by the reviewer, but it is not mathematically equivalent.

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.

It is true that the simulations with MCML are focused on the precise problem addressed in the paper which was 1) to help in the decision to discard the near-surface layers and 2) to evaluate the error on the final results for our specific setup (with our rod characteristics, ...).

The difficulty to address the request by the reviewer is two-fold:
1) this is complex to provide easily interpretable results (i.e. not misleading) because the snow properties interact with the rod characteristics (or air gap) and affect the estimated absorption spectrum. Therefore, for given rod characteristics, different absorption spectra can be obtained depending on the precise profile of SSA and density. This is clearly shown in Fig 16 (and associated text) with the homogeneous snowpack and 25kmE_1 snowpits. The homogeneous snowpack yields stronger absorption with more absorbent rod, and the 25kmE_1 yields the opposite. If we attempt to show the estimated absorption spectra resulting from variations of the rod radius or albedo, it would be strictly valid for the homogeneous snowpack and any extrapolation to another snowpack would be speculation. Therefore, it seems difficult to present general and interesting results without taking the risk to convey a misleading message. This may be better addressed in a dedicated study or, even better to our opinion, we recommend that future studies using fiber optics in snow should systematically use MCML-like simulations to evaluate the impact of the specific instrument used in the specific experimental conditions.

2) this request represents a significant amount of additional simulations as the sensitivity studies have been run at 400nm only. To plot ice absorption spectrum as in Fig 16 it would be necessary to run the same simulations for many wavelengths. Note also that the paper is richly illustrated with 17 figures.

Minor comments:

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.

Each of the three authors performed an independent selection and the selections were automatically merged. Only zones for which two operators agree are retained (this is done with a script, there is no subjectivity in this part of the process). Each operator was recommended to remove the surface (up to 8cm), to reject zones where the ascending and descending profiles are too different and to select sufficiently thick portions of linear profile. Too thin zones would not affect the BAY method anyway since such zones would be “given” a low weight. This information is now provided in the text.

We have also provided the profiles in supplementary Figure so the readers can reassess the dataset (and a digital version will be distributed here: http://lgge.osug.fr/~picard/ice_absorption/). Nevertheless, despite the subjectivity of this selection, it is worth noting that the final statistical error (Figure 6) is very small compared to the instrumental error, and the instrumental error is itself small compared to the big difference obtained between IA2008 and BAY estimates. It means that the selection of the zones has no impact on the main conclusions of the paper.

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?

The homogeneous zones are already quite thin (e.g. compared to Warren et al. 2006) so only a fit on each entire zone has been done. We changed the caption of Figure 5 to indicate this:
“70 ice absorption spectra estimated by the WBG method (Warren et al. 2006) on each homogeneous zone from the selection on the 56 radiance profiles measured around Concordia.”

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

The data are combined before the estimation. It means that all pairs (depth, intensity) are taken independently of the direction (ascending or descending) of the acquisition. The direction is only used during the selection by the author to check the quality of the profile. In the supplementary figures, it is clear that the ascending/descending differ mostly by the level of intensity and not by the gradient, so the impact on the estimation should be small. Moreover, in such a case, because the data are not aligned, the profile will be "given" a small weight by the BAY method as it looks like noisy data.

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?

In most cases, non-homogenous zones correspond to actual variations of the physical properties of the snowpack, there is nothing wrong with the measurements. Figure 14 shows that we reproduce the observed variations with MCML when the profiles of SSA and density ares taken into account. The WGB and BAY methods rely on the homogeneity of the zones (even if the instrumental errors were absent). This is a constraint of the methods.

For this reason Dome C is not an ideal site. With an annual accumulation of only 8 cm, the snowpack is usually very heterogeneous (Libois et al. 2014). A site combining high accumulation, weak wind and remoteness (for low impurities) would be ideal.

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?

The arguments of the reviewer are relevant for the prior choice when an informative prior is wanted or needed. This is the case when 1) there is a strong reason to choose one particular value/spectra (here we really don't know which one is good) and 2) the posterior would be very large otherwise (here we obtain a narrow posterior). Choosing an informative prior is necessary as when a strong "regularization term" is needed in variational methods to stabilize the optimization. Here, we consider we don't know the value of absorption in the range 350-600nm, so we want a very large distribution for the prior of the ice absorption (i.e. uninformative prior), so that it does not influence the posterior. For this reason we have chosen IA1984 and IA2008 because they are extreme, they encompass the probable true value (as specified in the text). The choice was not driven by physical considerations or quality/flaw criteria. We could have chosen a uniform random variable between 10^{-5} and 10^{1} in the range 350-600nm without any reference to former ice absorption spectrum estimates, the results would have been essentially the same. This is visible because the posterior is much narrower than the prior. It means that the posterior is constrained by the data, not by the prior.

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?

We don't understand the comment. The methodology imposes the value at 600nm so it influences the results at other wavelengths.

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.

These two comments are related. We agree with the reviewer that there is a link in all the studies between the absorption value and the minimum of absorption. This is stated in our introduction: "A related debate concerns the position of minimum absorption, which in general has shifted towards shorter wavelengths with successive updates.". The interpretation with the AMANDA empirical model sounds possible. The decrease in the UV would be due to a power law due to small scatterers (Rayleigh scattering).

However, there is no evidence that dust is the cause of this scattering in our case (i.e. at the surface of the ice sheet). As written in the response to the major comments, the concentration of dust or soot needed to re-conciliate IA2008 and BAY is about 5ng/g eqBC, which, considering the literature, is impossible with the background (natural) concentration. Alternatively it would require a huge increase of pollution between 1km in ca. 2004 (IA2008 measurements) versus 25km upwind ca. 2012-2013 (our measurements). For comparison, recent chemical measurements at 1km from the Summit station in Greenland yields concentration between 50 and 300 ng/g (Carmagnola et al. 2013 in The Cryosphere) which is equivalent to about 0.3ng/g and 1.7 ng/g eqBC respectively. How concentration as high as 5ng/g eqBC could be systematically found at 25km upwind of Concordia ?

Calling at the dust to explain the discrepancy between the different estimates is not a more reasonable hypothesis than undiscovered instrumental errors, artifacts of protocol, or unknown sensitivity of the ice absorption to temperature, pressure, or crystalline structure.

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.

The statistical error has been added on Fig 4 in the text . The same for Fig 5 has be done in this response. We have no robust way to estimate the systematic errors.

Finally, some minor language points:

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

We have changed everywhere.

P13L31: back carbon -> black carbon
P10L37: neither -> either, nor-> or

done