First of all, we slightly modified the results in the new version of the manuscript. The retrieval method used in the last version indeed included a small regularization term to minimize the SSA difference between the retrieval and the measured value contrary to what is written from page 10 lines 18 to 21. As this term had a small weight the impacts on our results are small. However to remain consistent with the description of the method, the regularization term has been removed and all figures and paragraphs impacted have been modified.
The impacts on our results are a small improvement of LAP retrieval performances (r^2 0.74→0.80, mainly due to one point shifting under the sensitivity threshold of 5ng/g) and a small reduction of SSA retrieval performances (r^2 0.73→0.71). The conclusion of the paper remains unchanged.

Answer to Anonymous Referee #2 (Referee):

We would like to thank Anonymous Referee #2 for his pertinent comments pointing out some issues in our manuscript. The comments have been addressed and discussed hereafter.
The reviewer initial comments are written in black, our answer in blue and the corrections in the paper are highlighted in red. The line numbers which are used in the answers correspond to the new version of the manuscript.

This study describes a novel and rapid technique to make in-situ measurements of the vertical profile of light absorbing impurities in snow. The technique relies on spectral irradiance measurements conducted via a narrow probe that is slowly inserted into the snow. Because the technique relies purely on radiative transfer theory, it does not require snow samples to be transported to the laboratory for chemical measurements. The underlying theory is nicely presented, and although the technique 'should' work well in principle, as with many ideal techniques there is substantial bias between the theoretically-derived and directly-measured impurity contents, as clearly acknowledged by the authors. The study presents a nice exploration of sources of uncertainty via parameter perturbations, and as far as I can tell the study has adequately explored all likely sources of bias. Unsurprisingly, the optical properties of BC and dust, which must be known apriori for this technique, are plausible culprits for the bias. Real uncertainty and variability in these properties could, by themselves, explain much of the reported bias. Overall, this is a very thorough and well-written paper describing a novel technique, and I recommend publication after the minor issues described below are addressed.

General issues:

Equation 1: It is noted that Phi represents the dust -> eqBC conversion function but this function is not really described in much detail. Please elaborate on what precisely this function is and/or how it is calculated. A related question is: Why is the eqBC vs dust line shown in Figure 1 not perfectly linear? This suggests that the conversion function is not so simple.

Ψ indeed represents the dust -> eqBC conversion function. This function is computed as follows:

1) The energy absorbed by a semi-infinite snowpack containing a given quantity of dust between 350 and 900 nm is computed for dust concentration spanning the whole range of observations (0→25μ g g^{-1}). The incoming spectral repartition of the energy is done with SBDART as explained in the text.
2) An optimization is ran to find which BC quantity would lead to the same broadband energy absorption between 350 and 900 nm with a resolution of 1 nm.

The equivalent BC concentration computed at a single wavelength, λ, by our method would be linear. Indeed following Equation 11, the equivalent BC concentration can be expressed as:
$C_{eqBC}=(c_{dust}\, MAE_{dust}(\lambda)) / MAE_{BC}(\lambda)$.
The non-linearity is introduced by the spectral dependence of the ratio between $MAE_{dust}(\lambda)$ and $MAE_{BC}(\lambda)$. As the estimated eqBC concentration is the integrated absorption on several wavelengths and the absorption has a non-linear response to LAP concentration , Ψ has no reason to be linear. Moreover, strictly speaking, Ψ also depends on BC concentration, on the SSA of the snowpack and on the selected spectral solar irradiance.
This has been clarified in the manuscript p.6 l.10 .

To do so, the energy absorbed by a semi-infinite snowpack with a SSA of 15 $m^2\, kg^{-1}$ is computed at each wavelength between 350 and 900nm. The spectral incoming irradiance is computed with the detailed atmospheric radiative model SBDART (Ricchiazzi et al., 1998), for mid-latitude winter in clear sky conditions .
It is noteworthy that the function Ψ has a strong dependence to the spectral distribution of the incident solar radiation and on the radiative transfer model parameters, mainly on the selected values of BC and dust Mass Absorption Efficiency (MAE). These MAE values are represented in Figure 1 a) and detailed in section 3.4.2. Strictly

speaking, Ψ also depends on the BC concentration and on the SSA of the snowpack but this minor impact is neglected here.

Minor issues:

p3, lines 26-28: "Picard et al (2016) ... meaning that SIP measurements could be an order of magnitude more sensitive to LAP than albedo measurements." - This statement implies that BC concentrations less than 50 ng/g cannot be detected via albedo measurements. This threshold seems a bit high, especially for visible wavelengths.
Are you referring to broadband albedo? Please clarify or justify.

Indeed this threshold is bit overestimated. It was based on Zege et al.2011 and Warren et al. 2013 but their paper applies only to remote sensing retrieval and terrestrial spectral albedo measurements can be expected to have a better accuracy and then sensitivity.
The sentence has been modified in the manuscript p.3 l.27

"..meaning that SIP measurements could be more sensitive to LAP than albedo measurements."

p6, line 10: "It is to note" -> "It is noteworthy"

The correction has been accounted for.

p6, line 12: "... the unit of ng/g eqBC refers to 1 ng/g of eqBC concentration" – This seems either unnecessarily obvious or needs elaboration.

This sentence (p.6 l.17) has been replaced by:
In the following, the LAP concentrations are expressed in ng g^{-1} eqBC.

p7, line 8: "ice matrix surface (m2)" -> "ice matrix surface area (m2)"

The correction has been accounted for.

p7, Eqns 10 and 11: It is a bit confusing that sigma_a and gamma both represent absorption coefficients of ice. It appears that sigma_a is the absorption coefficient of "snow due to ice", whereas gamma is the absorption coefficient of bulk ice. Please clarify the wording to communicate this.

Indeed the explanation on this two variables was confusing. It has been clarified as follows when the variables are introduced: P.8 L.19
with σ_a (m^{-1}) the absorption coefficient of snow due to ice and B the absorption enhancement parameter. The term $\gamma(\lambda)$ (m^{-1}) is the absorption coefficient of bulk ice and is related to the imaginary part of ice refractive index $n_i(\lambda)$ as follows:

p7, Eqn 10: Maybe clarify that rho is the density of snow, if this has not already been done.

The variable ρ was not introduced in our equations. It has been corrected after Equation 5 (P.8 l.16)
where ρ is the density of snow and SSA is its Specific Surface Area (m^2 kg^{-1} Legagneux et al. 2002)

p10, line 3: "did not fit well the" -> "did not fit well with the"

The correction has been accounted for.

p10, line 27: Please clearly communicate the sign of the bias. i.e., Was the chemically-determined or SOLEXS-derived BC estimate higher?

The sentence mentioning the bias has been completed as follows p.11 l.16
Indeed, the correlation in this range has a r^2 of 0.81 in spite of a significant bias of 14.6 ng g^{-1} eqBC; the chemically measured concentrations being lower than the SOLEXS retrieval

p12, line 6: "an higher" -> "a higher"

The correction has been accounted for.

p13, line 2: "the radiative impact" -> "the calculated radiative impact", correct? Or if not, please clarify this sentence, again with respect to the sign of the bias (higher derived-BC or chemically-measured BC?).

p13: line 27: "In some case, an abnormally" -> "In some cases, an abnormally"

The correction has been accounted for.

p14, line 15: "clearly break" -> "clearly breaks" or better "clearly violates"

The correction has been accounted for, "clearly break" has been replaced by "clearly violates".

p14, line 24: "more impacting" -> "more impact"

The correction has been accounted for.

Figures 6, 13 and 14: In the legend, why does one curve show BC and the other rBC? Please remind readers of why this distinction is needed here. It seems confusing and potentially unnecessary.

In these figures, the distinction is done between BC and rBC because rBC is the quantity measured by the SP2 which is just one way among others to measure BC in snow. As these different measurement techniques can strongly diverge in term of BC concentration (e.g., Lim et al. 2014) this information is important. The following sentence has been added in the caption of Figure 5 of the manuscript.

Note that rBC is the refractory BC concentration measured by SP^2 instrument

It is noteworthy that Figure 5 of the new manuscript was the Figure 6 you mentioned in your comment.

References:

Zege, E. P., Katsev, I. L., Malinka, A. V., Prikhach, A. S., Heygster, G., & Wiebe, H. (2011). Algorithm for retrieval of the effective snow grain size and pollution amount from satellite measurements. *Remote Sensing of Environment*, *115*(10), 2674-2685.

Warren, S. G. (2013). Can black carbon in snow be detected by remote sensing?. *Journal of Geophysical Research: Atmospheres*, *118*(2), 779-786.

Lim, S., Faïn, X., Zanatta, M., Cozic, J., Jaffrezo, J. L., Ginot, P., & Laj, P. (2014). Refractory black carbon mass concentrations in snow and ice: method evaluation and inter-comparison with elemental carbon measurement. *Atmospheric Measurement Techniques*, *7*(10), 3307-3324.