We thank the anonymous reviewer for the helpful comments. These comments helped to substantially improve the manuscript. Below we give detailed answers to the individual reviewer comments in blue.

Referee Comments for: "Specifying light absorbing properties of aerosol particles in fresh snow samples, collected at the Environmental Research Station Schneefernerhaus (UFS), Zugspitze"

General Comments:

This manuscript presents a novel method for concurrently measuring the rBC mass concentration and total aerosol absorption in snow. The authors find a discrepancy between the SP2-measured rBC concentration and a calculation of rBC concentration that is based on total aerosol absorption and a fullerene-standard's MAC. The former being smaller than the latter, the authors conclude the likely presence of non-rBC absorbing aerosols in the snow. WIBS measurements indicate that the larger of these non-rBC aerosol are predominantly of biological origin. These results are both interesting and important, as the presence and properties of light-absorbing aerosols in snow and their impact on snow/ice melt is not presently well understood.

Overall, the methodology of the measurements seems sound, and the text contains all of the necessary information to understand the experiment. I have four main concerns, three of which are science-related, and one regarding the writing. These can be found in the 'Specific Comments' below. I believe that once these things are appropriately addressed, the manuscript deserves publication in ACP.

Specific Comments:

My first concern is that this study's calculated MAC for fullerene soot is quite high compared to other literature, which typically put it around 7 m2/g or so. The authors acknowledge this, but I don't feel they explore (at least in the text) the underlying cause sufficiently. As this is at least somewhat dependent on the size of the fullerene particles, I suggest that the fullerene standard's size distributions be added to Figure 8.

First, we added a figure (Fig. 6 in the revised manuscript) that shows the refractory BC mass size distributions of our Fullerene suspension standards. Second, these size distributions were fitted by lognormal distributions to get the missed mass beyond the upper size limit of our SP2. The analysis of the Fullerene MAC was than redone with the corrected BC mass, resulting in lower MAC values of 10.5 ± 3.2, 9.5 ± 2.2, and 8.6 ± 3.3 m2/g for 405, 532, and 658 nm, respectively. Third, we completely rephrased Sect. 4 in the revised manuscript that now includes a comparison with published MAC of Fullerene soot and a discussion of potential reasons why our values are larger. This discussion includes the influence of different particle size distributions, different methods to deduce the particle mass, differences in using different Fullerene soot batches as well as the in general higher sensitivity of light absorption to BC electronic band structures and fractal aggregate morphologies compared to the incandescence mass detection.

Second, the authors don't estimate the percentage of rBC in snow that is above the detection limit of the SP2. Again, the authors acknowledge the issue, beginning on line 278, by stating that the SP2 only detects rBC up to 500 nm VED, but then rather simply declare the mass beyond this as small. The size distribution in Figure 8 indicates that there is still non-negligible mass above 500nm, and exactly how much can easily be estimated using a lognormal fit to the distributions. I attempted to estimate this by extracting data from the plot (see my attached figure), doing my own fit, and calculating percentage of area under the curve that is above 500nm. . .I get about 10%. Furthermore, its highly doubtful that the SP2 is detecting with 100% efficiency below 60-70nm or so. This probably has a smaller effect on the total rBC mass, but including that data in a lognormal fit could skew the fit result. If I do the same fit, but only use the size distribution data between 70 – 500 nm, I come up with ~13% of the mass above the detection limit. Of course, this is just an 'eyeball' estimate on my part, and may be off by a bit. I suggest the authors do a more careful check and add the details to the text.

As suggested by the reviewer, we performed a more careful analysis of the refractory BC size distributions of the snow samples. First, we divided the samples in the periods Nov-Jan, Feb-Mar, and Apr-May to check any seasonality in the data. Second, we performed lognormal fits to the data between 70 and 500 nm as suggested by the reviewer and found a mass fraction of 10 to 20% that is beyond the upper size limit of our SP2. Third, we compared our size distributions with the average SP2 size distribution measured for five snow samples collected after three snowfall events in the semi-rural and rural surroundings of Denver, CO, USA by Schwarz et al. (2013). Our size distributions agree very well with the Schwarz et al. distribution, who measured the distribution up to a size of 2 μm (see Fig. 9 of the revised manuscript). Due to this good agreement and the fact that the size distributions are skewed with a shoulder towards larger sizes, we decided to use the 28% given by Schwarz et al. for the mass fraction > 600 nm to correct our snow sample SP2 data.

Ideally, the gain on one of the SP2's incandescent channels should have been set so as to extend the detection limit. The fact that it wasn't does not necessarily damage the story the paper is telling, in my opinion. . .but nevertheless, more care should be given to estimating the effects of this. Ultimately, the SP2-determined rBC concentrations presented in the manuscript are more appropriately viewed as low-bounds until corrected for the undetected rBC mass. Note also that accounting for the undetected rBC would bring the SP2-determined concentration and the PAAS-3L calculated- concentration into better agreement.

See answer to the previous comment. With a correction factor of 1.39 that we applied to the SP2 data in the reanalysis (i.e. accounting for a fraction of 28% missed mass), we are rather on the upper limit for this correction. In this way, however, we are confident that the main conclusion of our paper namely that there is a significant contribution of non-BC particles to light absorption in fresh snow is solid.

Third, on Line 192: The concentration of the PSL standard is higher than I'm comfortable using for my own Marin-5+SP2 setup. . .I wonder if there was any evidence of multiple PSL particles existing within a single SP2 trigger? This could affect the efficiency calculation. It's easy enough to look through the raw SP2 data and confirm that this isn't a common occurrence for their data. I'm not claiming that I expect that there is a major issue, but simply that it should be looked for when using concentrations that high. Further, the authors do not include the size of the PSLs. . .this should be stated.

We think that our PSL concentrations are no too high. The manufacturer suspension has a concentration of 3 10^8 #/mL, which is further diluted by a factor of 100 before it is used in the characterization of the Marin-5. With the Marin-5 flow settings and the known dispersion efficiency of 36% we end up with a few hundred particles per cc at the Marin-5 output, which is definitely not an issue for the SP2 measurement in terms of coincidence.

Finally, in general, the writing needs some 'smoothing', as certain word choices and sentence structures don't flow as well as they could. I would recommend a careful proofreading.

The manuscript has been thoroughly restructured and reworded.

The technical corrections listed below are addressed in the revised manuscript.

Technical Corrections:

General comment: I see no need to continually put 'fullerene' in parenthesis Line 56: extend -> extent

Line 64: I don't like the phrasing "should be" in this circumstance. I'd suggest something like "The authors determined albedo values of only 0.5-0.7 for the ultraviolet and visible range, substantially lower than the 0.97-0.99 that is typical for clean snow [include a citation]"

Line 67: "Most Himalayan glaciers as glaciers. . ." - > Should this be "Most Himalayan glaciers and glaciers"?

Line 74: smooth wording of sentence beginning “In the last decade. . .”

Line 96: analyses -> analyzes

Line 118: remove word ‘used’

Line 207: the authors use the word ‘daily’ twice in quick succession. Again, smooth the writing here

Line 221: depending -> dependent

Line 225: the phrase “it turned out” is too colloquial for a scientific paper

Line 268: remove the word ‘used’

Line 282: please add the word ‘these’ to specify that this statement isn’t generally true (for instance, in the case of snow that has experienced freeze/thaw), i.e. “Thus, the majority of the rBC particles in these fresh snow samples have. . .”

Line 322: The use of the word “therefore” is not appropriate here. Recommend reword- ing.