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.

Review of Linke et al. 'Specifying light absorbing properties of aerosol particles in fresh snow samples, collected at the Environmental Research Station Schneefernerhaus (UFS), Zugspitze.'

General comments:

This paper present results from snow samples analyzed for their aerosols content over one winter season. The authors are using an array of different instrumentation, making this an interesting study that is most certainly within the scope of ACP. In light of the instrumentation used in this study, it appears that the authors have not presented the full potential of the data that should be at hand in the current version of the manuscript. For example: absorption measurements coupled with the BC mass content have been carried out, enabling the authors to present direct MAC values for the particulates in the snow (which could then be used for comparison and additional data in the manuscript). Another issue that should be addressed more thoroughly in the manuscript, is the fact that it seems as there is only the one sample (from March 10th) where the additional analyzes are performed, and from this one sample conclusions about all of the snow samples are drawn. Please see further comments below on these topics, as well as some other questions and recommendations that should be addressed. Lastly, the manuscript's language should be checked and re-evaluated for a better read. Although I'm not a native English speaker myself, I do believe the manuscript would benefit from such a procedure. Some mistakes have been highlighted in the technical corrections below, but I'm sure there are mistakes that I have missed, and so make sure to check the whole manuscript. On the whole, this work would be a welcomed addition to the literature after it has been majorly revised.

We agree with the reviewer that we have not presented the full potential of our measurements and data. That's why we have made efforts to include a more thorough analysis of the data, resulting in additional discussions (which includes now also comparisons with literature data) of (a) the MAC of "Fullerene" soot, (b) the MAC of the snow samples, and (c) the spectral absorption of the non-BC particles in the snow. While the ESEM and fluorescence analysis of further snow samples is certainly something that would strengthen our conclusions, we refrained from doing them simply because these analyses (especially with the ESEM) are very time consuming and expensive, which we couldn't invest at this time for this project. Finally, the manuscript has been improved in language.

Specific comments:

Section 1

The introduction could use some restructuring and clarifications. My opinion is that some references are missing, and the addition of these papers will change some of the claims the authors are making in the introduction. Under 'technical corrections' I have provided comments to specific line numbers on this.

The introduction was completely restructured rewritten taking into account the comments provided in 'technical corrections'

Section 2

Lines 128-129: Do the authors have any idea to what degree the station is affected by the anthropogenic emissions? It would be valuable information to add in the text, if it exists? How close is UFS to the skiing area?

Actually, there exists a study on the influence of anthropogenic activities on the UFS station by Yuan et al. (2019). They detected on weekdays multiple short-term atmospheric CO events and higher atmospheric NO peaks during the daytime (mostly around 09:00 LT), which were interpreted to originate by anthropogenic working activities and less by tourists. During the period the snow samples were collected, the 1 min ambient air eBC data show elevated levels above 1 µg/m3 only on 20 days and only for short periods of less than 20 min giving a total time of about 160 minutes the station experienced this higher BC pollution levels. This time is less than 1 permille of the total snow sampling period. For those days we have no indications that the snow samples are affected by anthropogenic emissions. However, the results of two samples collected on February 6 and February 17 were discarded from the data, because they show inexplicable high BC mass mixing ratios and absorption coefficients (factor 5 to 10 outside the 95th percentile of the other samples), which indicates a possible contamination from local sources.

Line 136: As the manuscript currently reads, 10 cm of snow was collected after each snowfall. What was the procedure of collecting the snow if there was more (or less) snow in the precipitation event? If it was more than 10 cm, for example, BC particles from the beginning of that snow event would be missing in the analysis. On the opposite, if there was a small snow event, producing only a few centimeters of snow, how was that snowfall sampled? In that event, if 10 cm of snow was sampled, it would include not only the fresh snow (and its particulates), but also more aged particles in the snow, leading to possible differences in the analysis.

We made the procedure clearer by rephrasing the paragraph in Sect. 2.1 as following:
"The snow samples were taken either during or just after snowfall events by scraping off only the top few centimeters of the snowpack to avoid sampling older snow. A metallic hand shovel is used to sample the snow from an area of about 30 x 30 cm into a zipper sealed polyethylene household plastic bag with a volume of 1 L (Toppits, Germany). In this way, snow from the beginning of the snowfall event could be missed, but most of the time the events were accompanied by heavy wind, so that it was impossible to completely sample the fresh snow layer. After collection, the samples were stored at the UFS in a freezer at -18°C until they were transported under frozen conditions to the laboratory at KIT. During six months, 33 samples were taken at the UFS."

Also, on the topic of the snow sampling, the authors mention that the collection was from a place exposed to wind, and so how did they manage to collect the new precipitation? One would expect the wind to remove this snow (that is typically is not very dense). A table in the supplement of this manuscript could easily be added, providing valuable information about the snow samples, precipitation events, as well as other basic weather parameters during sampling.

In addition to the changes in Sect. 2.1 given above, we replaced Figure 7 with a stacked figure (Figure 8 in the revised manuscript) that highlights the trends in ambient temperature, sunshine duration, snow precipitation and snow height over the period the snow samples were collected at the UFS station. In this Figure also the ambient eBC and the refractory BC concentration of the snow samples is shown to give the reader an overview about the ambient conditions and their changes during the 6 months period of snow sampling.

Line 144: How do the authors know that using the ultrasonic bath to melt the snow sample did not change the structure of the BC particles? (e.g. break the particles apart into smaller sizes, which obviously will have an impact on their optical properties), or the other particles present in the snow also?

We have no information how the ultrasonic bath might affect the microstructure of the particles. However, we followed the recommendations that are given in the literature concerning snow sample analysis to be at least consistent with those studies. We added the following paragraph to the manuscript to make this clearer:

"Aqueous snow/ice sample sonication prior to the analysis is recommended by several groups (e.g. Kaspari et al., 2011; Wendl et al., 2014) although with inconclusive results of the obtained improvements. The melted samples were never refrozen for later analysis as this can result in a significant particle mass loss of up to 60% (Wendl et al., 2014)."

Section 3

Some sentences in the fourth paragraph are already mentioned in the second paragraph (e.g. lines 235- 237 are mentioned in lines 216-219). Technically, I do not see why the information in this fourth paragraph (which ultimately contains more details) could already be incorporated into the second paragraph.

We restructured Section 3 in the revised manuscript to make it more concise.

Lines 225-226: Could you present any quantitative numbers on this minor influence?

The number nebulizing efficiency and the relative humidity are not changing (within a few percent) when varying the cooled section temperature in the low positive temperature range (i.e. 0 to 5°C). However, the efficiency increases and the r.h. decreases for sub-zero temperatures. Operating the nebulizer at a sub-zero cooled section temperature resulted in regular failures of the nebulizer, most likely due to water freezing in the cooled section.
We changed added "a few percent" to the sentence.

Section 4

Lines 263-266: What about adding the size distribution of the fullerene in a figure, to show that the difference in fullerene MAC's could be explained by differences in size?

We added a figure of the size distributions to the manuscript.

Section 5

The results of the snow sample measurements could be highlighted even more. You have 33 samples from one snow season. How do they differ from one another? In Fig. 7 you show how the BC snow concentration varies over this season, what about the air concentration of BC from the station? (it is mentioned that no correlation was found between the snow and air BC concentrations. I would argue that this is shown, possibly in fig. 7 or a different figure). Is there any seasonality in the size distribution of BC particles from snow? I'm not convinced that there is such a clear decrease in the size distribution of the larger BC particles, please elaborate on this to further convince the reader that this is the case. (It is true that since you have taken relatively fresh snow samples, there would be no thawing and freezing cycles, possibly leading to larger BC particles). One example comes to mind where fresh snow BC size distributions were measured (Sinha et al., https://doi.org/10.1002/2017JD028027). Although in a different setting, please put your results into context by comparing with this study.

We agree with the reviewer that the presentation and discussion of the snow sample results in the context of concurrent meteorological and ambient aerosol measurements is too short or even lacking in the manuscript. That's why we have replaced Fig. 7 by a stacked plot (Fig. 8 in the revised manuscript) showing maximum and minimum temperature, sunshine duration, precipitation, snow height as well as the air BC mass concentration records over the time period the snow samples were taken.
To discuss the measured rBC snow concentrations in context with these other measurements we added the following paragraph to Section 5:

"This BC concentration is shown in **Error! Reference source not found.**c in conjunction with the eBC mass concentration of ambient air that is routinely measured by the German Federal Environment Agency using a Multi-Angle Absorption Photometer (MAAP). Also, a selection of meteorological data is presented in **Error! Reference source not found.** to highlight the trends in ambient temperature, sunshine duration, snow precipitation and snow height over the period the snow samples were collected at the UFS station. Although there is no clear correlation between the fresh snow samples and ambient air eBC mass concentration, the enhanced air eBC mass concentration observed end of March and beginning of April might have resulted in additional deposition of BC particles in the snow surface that is reflected - with a time lag of several days - in the measured snow refractory BC mass mixing ratio. Interestingly, this period of higher air eBC concentration is distinguished by a low precipitation activity, long sunshine periods as well as frequent daily maximum temperatures above the melting point that resulted in frequent thaw/freeze cycles and, consequently, a gradual decrease of the snow height by 30 to 40 cm. All in all, the enhanced air eBC concentration in conjunction with the meteorological conditions would favor enhanced BC mass concentrations in the fresh snow samples collected after precipitation events within this period or shortly after."

Actually, from the 33 samples we use only 31 samples in the revised manuscript as two samples were discarded due to inexplicable high rBC mass concentrations and absorption coefficients (factor 5 to 10 outside the 95th percentile of the other samples). To investigate a possible seasonality in the measured rBC size distributions of the snow samples, we divided the 31 samples into the three periods (1) November to January with 11 samples, (2) February and March with 11 samples, and (3) April and May with 9 samples. For each period the average rBC size distribution is calculated and is plotted in the updated Fig. 8 (which is Fig. 9 in the revised manuscript).

We thank the reviewer for noticing the Sinha et al. paper. In Fig. 8 of their paper the BC mass size distribution of a fresh snow sample is shown that was collected after a snowfall event at Ny-Ålesund, Svalbard, Norway. We also found the study by Schwarz et al. (2013) very useful as this study presents an averaged rBC mass size distribution of snow samples collected shortly after snowfall events in Colorado, USA (Fig. 1 in their study). Our averaged fresh snow sample size distributions peak at similar mass median diameters (MMD) between 194 and 227 nm compared to the 223 ± 28 nm of the Sinha et al. study and the ~220 nm of the Schwarz et al. study. In addition, our BC mass size distributions indicate a non-lognormal shoulder at the upper size limit of our measurement that is in a very good agreement with the Schwarz et al. (2013) samples where the BC mass size distribution was measured up to 2 µm (see Fig. 9 in the revised manuscript which shows a comparison with the Schwarz et al. data). As it is discussed by Schwarz et al. (2013) such a size distribution reflects the typical atmospheric BC distribution at remote locations that is altered by agglomeration and size selection processes during snow formation in the atmosphere.
We also divided the snow samples into the three periods Nov-Jan, Feb-Mar, and Apr-May and present the averaged size distributions of these three periods in Fig. 9 of the revised paper. This clearly shows that here is no significant seasonality in the snow BC data.

We have added the following paragraph to Section 5 to include these comparisons and their

conclusions:

“Figure 9 shows corresponding mass size distributions of the refractory BC concentrations shown in Figure 8c averaged over the periods November to January, February and March, as well as April and May. For comparison purposes, the average size distributions are normalized by the corresponding total mass concentration Mtotal, which was deduced from a lognormal fit. The SP2-derived refractory BC mass size distribution only includes particles up to a mass-equivalent diameter of 560 nm, which means that larger BC particles are not recorded by the SP2. However, the average BC mass size distributions have distinct mode maxima at the mass median diameters (MMD) of 227, 194, and 222 nm for the Nov-Jan, Feb-Mar, and Apr-May periods, respectively. This indicates no strong seasonality in the snow BC mass size distribution even in the Apr-May period where the BC mass concentration in the snow was significantly enhanced (Figure 8c). This further implies that indeed fresh snow was sampled which hasn’t experienced thaw/freeze cycles severe enough to induce an agglomeration of the BC particles in the top snow layer. This conclusion is further supported by comparing the average BC mass size distributions of our snow samples with the BC mass size distribution of a fresh snow sample collected after a long-lasting snowfall event at Ny-Ålesund, Svalbard, Norway by Sinha et al. (2018) and with the averaged BC size distribution from 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 average fresh snow sample size distributions peak at similar MMD between 194 and 227 nm compared to the 223 ± 28 nm of the Sinha et al. study and the ~220 nm of the Schwarz et al. study. In addition, our size distributions indicate a non-lognormal shoulder at the upper size limit of the SP2 measurement that is in a very good agreement with the Schwarz et al. (2013) samples where the refractory BC mass size distributions were measured by a SP2 with modified detector gains up to 2 µm (see Figure 9). As pointed out by Schwarz et al. (2013) such snow BC mass size distributions reflect the typical atmospheric BC mass size distribution that is observed at remote locations altered by agglomeration and size selection processes during snow formation in the atmosphere. The good agreement between the mass size distributions of our snow samples and the average distribution of the Schwarz et al. (2013) samples allows us to estimate the refractory BC mass that is contained in the large particle size shoulder outside our measurement range. According to Schwarz et al. (2013) a fraction of 28% of the total BC mass can be attributed to particles with mass-equivalent diameters larger than 600 nm. A mass correction factor of 1.39 is therefore applied to the SP2-derived refractory BC snow concentrations in the following analysis.”

What about the dust concentrations? How did the described Saharan dust episodes influence the BC mass (and the snow samples)?

Although we cannot directly measure the dust concentrations in the snow samples, we can draw some conclusion by analyzing the spectral signature of the snow mass specific absorption cross section of the non-BC particles, i.e. after subtraction the spectral cross section that is expected from the SP2 mass data using the MAC of Fullerene soot (Equation 3 of the revised manuscript). We added a figure (Fig. 12 in the revised manuscript) that shows a comparison of the non-BC spectral absorption cross section with laboratory absorption data for Saharan dust particles. Thus, Saharan dust is a good candidate to explain the observed non-BC light absorption in the snow samples. We did the same comparison with laboratory data of OC, which reveals that OC could also explain the observed non-BC absorption - not only in terms of wavelength dependence but also in terms of variability.
The whole paragraph on the analysis of the spectral absorption analysis has been rewritten to make our argumentation of a significant influence of non-BC particles on the light absorption in snow clearer.

Through the measurement set-up that the authors present, they should have the data necessary to directly derive MAC values for the particles in the snow. How come this was not done? I believe one of the other referees also commented on this. Either the authors provide these MAC values also, and compare (and discuss) that in the manuscript with the data that they already have. If this

is not possible, then it should be stated why, and more emphasized why the approach used currently in the manuscript is utilized. There is evidently other impurities other than the BC particles, which will influence the MAC value for the particles in the snow, but with the other instrumentation available, it should assist in describing those particles (i.e. the OC content).

We did a thorough reanalysis of the snow samples following this strategy:

1. The snow mass specific absorption cross section σ_{abs} [m^2/mL] was deduced from the absorption coefficient b_{abs} [m^{-1}] using the nebulizer flow settings and the nebulizing efficiency (Eq. 1 of the revised manuscript):

   $$\sigma_{abs} = 10^{-3} \cdot b_{abs} \cdot R_{neb}/R_{pp} \cdot \varepsilon_{neb}^{-1}$$

2. This snow mass specific absorption cross section is plotted as a function of the refractory BC mass concentrations in a new figure (Fig. 10 of the revised manuscript). Linear regression fits to the data then give the BC mass specific MAC of the snow samples. We found MAC values that are up to a factor of two larger than the MAC of Fullerene soot. A table is added to the manuscript contrasting the mass and optical properties (including the MAC) of Fullerene soot with those of the snow samples.

3. To be comparable with other studies (e.g. Doherty et al., 2010), we calculated the equivalent BC mass concentration c_{BC}^{equiv}, i.e. the amount of BC that would need to be present in the snow to account for the measured absorption, from σ_{abs} using the MAC of Fullerene soot (Equation 2 of the revised manuscript):

   $$c_{BC}^{equiv} = \sigma_{abs}/\mathrm{MAC}_{FS}$$

4. We plotted c_{BC}^{equiv} as a function of the refractory BC mass concentration in a new figure (Fig. 11 of the revised manuscript) and found a good correlation of both concentrations but with correlation coefficients of 2.0, 1.9, and 1.4 for 405, 532, and 658 nm, respectively (note that c_{BC}^{equiv} is a function of the wavelength). We conclude from this that there is additional non-BC light absorbing mass in the snow, which is correlated with the BC mass and which has a strong wavelength dependence between the green and the red part of the visible spectrum. This already indicates mineral dust and organic (brown) carbon as possible carriers of this additional absorption.

5. We calculated the snow mass specific absorption cross section of the non-BC particles, σ_{abs}^{nonBC} (Equation 3 in the revised manuscript):

   $$\sigma_{abs}^{nonBC} = \sigma_{abs} - c_{BC}^{SP2} \cdot \mathrm{MAC}_{FS} \cdot 10^{-9}$$

   We added a figure (Fig. 12 in the revised manuscript) that shows the statistical analysis of the σ_{abs}^{nonBC} for the snow samples and that compares this non-BC spectral behavior with laboratory data for Saharan dust and organic (brown) carbon (see answer to the previous comment).

Line 313: What evidence is there to show that this one sample from March 10th is representative for all of the snow samples? (This sample contains a low amount of BC compared to the other samples and has an enhancement factor of 2.7 compared to the 2.34 presented for the others samples). Either present some evidence that this is representative for all of the samples or emphasize in the manuscript that these additional analyzes (presented in the following paragraphs) were only done to the one sample, and so it is difficult to draw conclusions for all of the snow samples. Ideally, I would argue that additional snow samples would be analyzed from different times during the season in the same way as the one sample from March 10th.

Actually, after the reanalysis of the samples, which includes the correction for missing BC mass in the SP2 measurement, the March 10 sample has a mass concentration c_{BC}^{SP2}=2.8 ng/mL and an equivalent BC mass concentration of c_{BC}^{equiv}=6.0 ng/ml for λ=532 nm, which gives an enhancement factor of $\gamma = 2.1$. It is therefore representative for γ, but is on the lower side concerning the c_{BC}^{SP2} and c_{BC}^{equiv} concentrations. We added a mark in the c_{BC}^{equiv} versus c_{BC}^{SP2} plot (Fig. 11 of the revised manuscript) to indicate the representativeness of this sample. As mentioned above, further analyses (especially with the ESEM) are very time consuming and expensive, which we couldn't be invested within the scope of this pilot study. This is now clearly stated in the revised manuscript:

"While these results give a detailed look into the physical and chemical nature of the of the particles that might contribute the light absorption in the March 10 snow sample, they cannot used to draw conclusions for all snow samples. Here, further analyses are required that couldn't conducted within the scope of this pilot study."

Lines 320-326: Although you discuss it further in the following paragraph, please include some sentence (or sentences) of what these specific results indicate about the snow samples?

Will be added to the revised manuscript.

Line 329: It would be interesting to have some information (even if it is hypothetical) on where this biological information originate from? E.g. Local or long distance?

We changed the terminology in the discussion of the ESEM and WIBS analysis by substituting the term "biological" with "biogenic". Although we have indications of biological components in the ESEM analysis, like bacteria, pollen and spores, we cannot conclude that all biogenic EDX patterns are due to microorganisms or their fragments. We added a paragraph on possible origins of the biogenic material found in the March 10 sample, mainly in the context of the observed non-BC light absorption by brown carbon.

"However, one question that arises from the above findings is whether the biogenic particles found in the March 10 snow sample can be attributed to BrC, which was shown to be a good candidate for explaining the additional light absorption in the snow samples (**Error! Reference source not found.**). The term "brown carbon" is not clearly defined or characterized and is mainly related to a strong wavelength dependence of the visible light absorption observed in these materials. From a chemical perspective, BrC can generally be divided into humic-like substances (HULIS) and tar balls (Wu et al., 2016). HULIS can be characterised mainly as a mixture of macromolecular organic compounds with various functional groups and are expected e.g. in oxidation processes of biogenic precursors (Wu et al., 2016). Tar balls are emitted from biomass burning and are of spherical, amorphous structure and are typically not aggregated. Moreover, light absorbing organic material and HULIS can be formed from the water-soluble fraction of biomass burning aerosol compounds, and is therefore suggested as an atmospheric process for the formation of light absorbing BrC in cloud droplets (Hoffer et al., 2004). Further examination of snow samples from different locations as well as systematic investigations on the optical behaviour of biogenic particulate matter is therefore necessary to evaluate the influence of biogenic (including biological), BrC and mineral dust on the aerosol absorption properties in the visible spectral range."

All the comments given in the technical corrections are addressed in the revised manuscript.

Technical corrections:

Lines 29-30: This opening sentence is not structured well. Please revise for a better read.

Line 32: Please remove 'packs' from ice packs. Could say snowpack, but not ice packs here.

Line 32-33: You could argue that a better reference here would be Warren and Wiscombe 1980 https://doi.org/10.1175/1520-0469(1980)037<2734:AMFTSA>2.0.CO;2, look into details of Doherty et al. 2010.

Lines 33-34: How is this sentence different than the previous sentence? I would think that it is better to have this sentence earlier.

Lines 35-36: How does 'this reduction' contribute to the snow-albedo feedback? Please include in the manuscript. What metamorphosis?

Line 41: What 'BC amounts' are you referring to? Please specify.

Lines 44-46: How are permafrost regions also affected? Unclear what you mean how they are affected? Also, after reading the rest of this paragraph, I would argue that you should remove this sentence. Since the rest of the paragraph discusses the Arctic, and these other 'areas' are not brought up again until later in the introduction, it could come then instead.

Line 50: As far as I remember Flanner et al. (2007) did not present any measurements, but based their modeling work on measurements instead.

Line 53: I would argue that you either introduce what the term 'soot' refers to, or stick with only discussing BC.

Line 56: Ice sheet instead of 'ice shield.' Please change also 'extend' to extent.

Lines 55-60: Concerning the Greenland ice sheet, there are also new papers on this topic of impurities, which could be added here (e.g. https://doi.org/10.5194/tc-10-477-2016; https://doi.org/10.5194/tc-11- 2491-2017).
Line 60: Please capitalize a in 'arctic.'
Line 61: Doherty et al. (2010) is already referenced to in the beginning of the sentence.

Line 68: How high amounts of dust? Would be more informative to actually reference to some numbers on this.
Line 69: To my knowledge, Bolch et al. (2012), is incorrectly referenced to here. No studies of BC nor dust were conducted in that paper.
Line 73: Remove 'the' before light absorbing particles.

Lines 77-80: I find this sentence confusing, please rewrite. Mixing the optical method (Doherty et al., 2010), and then the thermos-optical analysis, with the previous sentence about MAC causes some of the confusion. The following paragraph (lines 81-93) dig deeper in each analysis technique and that is appropriate, but the order of this seems strange, in light of the previous paragraph. Actually, I think you could delete the sentences in lines 75-80, and jump right into line 81 and an explanation of the methods (after current sentence ending on line 75).

Lines 92-93: Either remove this sentence or add more information on other instruments and

protocols out there (e.g. DRI and Improve-protocol). I would vote for removing this sentence, I do not think it is very crucial information.

Line 97: I would argue that you do not need quotation marks around Fullerene.

Lines 98-100: This sentence is basically a repeat of the first sentence of this paragraph, please remove.

Lines 102-103: I generally agree with this statement that not much have been reported on the light absorbing properties. But, there has been some publications on this topic addressing it directly and indirectly, e.g. Schwarz et al., 2013 https://doi.org/10.1038/srep01356; Zhang et al., 2017 http://dx.doi.org/10.1016/j.scitotenv.2017.07.100; Dal Farra et al., 2018 doi: 10.1017/jog.2018.29; Dong et al., https://doi.org/10.5194/tc-12-3877-2018. Please add and discuss these references.

Line 104: Please remove 'solar' before albedo.

Lines 103-106: Please clarify the structure by checking the structure. As it currently stands, it is not clear what the main point of the sentence is.

Lines 106-108: Please change this sentence according to the forthcoming changes made for lines 102- 103.

Lines 109-110: The second half of this sentence (starting after 'but) I find problematic. Similar to the comment in lines 102-103, I do believe this topic has been addressed in the literature. For example: Kaspari et al., 2014 (that you already referenced to earlier in the manuscript); Skiles and Painter, 2016 doi: 10.1017/jog.2016.125; Schmale et al., 2017 DOI: 10.1038/srep40501; Zhang et al., 2018 https://doi.org/10.5194/tc-12-413-2018, 2018. Please adjust your claim by including these references on this topic.

Line 137: The fact that the snow samples were collected at 'platform 7' does not add any information to a reader unfamiliar with UFS. Please either elaborate on this, or remove.
Line 147: What does 'Enhanced' refer to? Please explain.

Line 150: I do not find the flow rate for this peristaltic pump anywhere. Please add it. Line 184: Please remove the double reference to 'Fischer and Smith (2018).'

Lines 196-198: Do you mean that the solution was prepared in the same way as in Schwarz et al? If so, please correct. Also, I believe the reference should be Schwarz et al. (2012) and not (2010) as it currently reads.

Line 204: How did you 'drop' 30 mL of fullerene solution onto the quartz filters? Please explain more.

Line 260: What did Zhou et al. (2017) refer to with MACreal? Please clarify.

Line 276: Remove 'before' at the end of the sentence.

Line 294: The presented enhancement factors for the different wavelength appear to be averages, please clarify this.

Lines 315-319: The instruments and methodology presented here should be described in section 2. Lines 344-345: This information should be moved to section 2.

Figure 2. I'm not sure how needed this figure is. I actually think that this figure could be integrated into fig. 1.

Figure 8. Why is there is a data gap around 280-300 nm?

Figure 9. This figure is quite busy right now. Could the data points be zoomed in on more? And could the data points be made smaller?