

Answer to Referee Comment 2 by Matthew Johnson

We are delighted by the feedback given within the overview by Matthew Johnson and thank them for taking time to review our manuscript. We also thank them for pointing out how to improve our manuscript in general and for the detailed description in the following chapters of the review.

We structured our answer according to the referee comment in (1) a scientific and (2) a technical part. The referee comments are given in bold letters, while we kept our answers in common notation. Changes in the text are given within quotation marks in italics.

We want to clarify the change of $ATN_{700-450}$ to $\Delta ATN_{700-450}$ in the revised version of the manuscript. This was proposed by Anonymous Referee #1. We agreed to and conducted this change, as $ATN_{700-450}$ is the difference between the attenuation at 700 and 450°C (without requiring the incident intensity of the transmitted laser signal). To avoid confusion, we stuck with $ATN_{700-450}$ in the answers to both referees.

(1) Scientific

The paper presents an impressive nine-year continuous dataset and introduces a TOA-based method for estimating mineral dust deposition using Fe as a proxy. These are significant contributions. However, the introduction never fully clarifies what gap in the literature this study directly fills. The authors mention missing comprehensive discussions (e.g., mineral-dust bias in EC quantification), but don't articulate: 1) Why earlier studies (e.g., Cerqueira 2010; Tuzet 2020) were insufficient. 2) How this work advances monitoring practice or radiative forcing estimates. I recommend adding a short "This study addresses three gaps..." paragraph at the end of the Introduction to anchor the novelty.

We thank Matthew Johnson for the positive evaluation of our work and agree that an overview of novelty is beneficial for the manuscript.

Firstly, we definitely do not describe earlier studies mentioned in the comment, i.e., Cerqueira et al. (2010) and Tuzet et al., (2020), as insufficient. Apparently, they did not encounter problems with the quantification of organic and elemental carbon via thermal-optical analysis due to lower mineral dust loadings of their samples. Problems with mineral dust were found by Wang et al. (2012) and Gul et al. (2018) and they propose an adapted evaluation method for thermal-optical analysis. To improve the connection between the studies by Wang et al. (2012) and Gul et al. (2018) and our work, we adapted the text, which now reads:

"The co-occurrence of mineral dust leads to an interference in the quantification of elemental carbon in thermal-optical analysis and adapted evaluation of measurement data was proposed without quantifying the impact on elemental carbon concentrations (Wang et al., 2012; Gul et al., 2018)."

Regarding the second point, we adapted the last paragraph of the Introduction section to highlight the novelty of our study. It now reads:

"We present a multi-year time series of snowpack sampling and analysis at a glacier field close to Sonnblick Observatory in the Austrian Alps starting in 2016. Using thermal-optical analysis as the central analytical technique, we report concentrations of elemental carbon and water insoluble organic carbon. Besides the presentation of this data set, the novelty of the paper is its methodological focus. Applying different laser correction methods, the bias in elemental carbon measurements introduced by mineral dust can be quantified and a more reliable method for general monitoring practice of elemental carbon is introduced. A particular advantage is that the laser correction method can also be applied to existing data sets via post-processing. The interference itself, a temperature dependent change in transmittance of the laser applied in thermal-optical analysis, is used to identify samples containing mineral dust and to approximate the mineral dust load in the snowpack. Thus, thermal-optical analysis

can be used for both the quantification of elemental carbon and the approximation of mineral dust concentrations.”

Section 3.3 introduces an Fe-based approximation for mineral dust loading from TOA data. While elegant and practical, several limitations appear under-discussed. First, the limited calibration dataset. Only 14 samples were used to derive the ATN–Fe fit (Eq. 1) and the valid range is stated as ATN = 4–13. Yet the paper applies the relationship throughout nine years of snowpacks, without showing how often values fall outside this range. Please provide a histogram of ATN_{700–450} values for all mineral dust layers to show how representative the calibration domain is.

We agree that the occurrence of ATN-values within and outside of the fitted range should be given for mineral dust samples of all years. Thus, we plotted the data in a histogram as suggested, using a bin width of 1 for the ATN₇₀₀₋₄₅₀ values. The samples used for the fit are hatched. Three samples exceeded the covered range (ATN₇₀₀₋₄₅₀>13). For one of these samples (collected in 2024, in the highest ATN₇₀₀₋₄₅₀ bin), the Fe loading from ICP-OES analysis was available and was compared to the Fe loading based on ATN₇₀₀₋₄₅₀ and the fit. The Fe loading and consequently the mineral dust loading would be underestimated by 30% for this sample based on ATN₇₀₀₋₄₅₀, as linearity is no longer given. For this sample, the mineral dust approximation used in the time series presented in the manuscript was based on the Fe loading from ICP-OES. For the other two samples exceeding an ATN₇₀₀₋₄₅₀ of 13 (both from 2019), no ICP-OES measurements were done. Lacking an alternative, we based the mineral dust approximation on ATN₇₀₀₋₄₅₀, knowing that it will result in an underestimation. This likely contributes to the poor agreement of the mineral dust deposition in 2019 with gravimetry. This was previously described insufficiently in the manuscript.

We added the histogram in the new Appendix C as Figure C1 to make the information easily accessible while not disrupting the reading flow. We added the following text:

“Appendix C: Further information on the fit between the Fe loading and ATN₇₀₀₋₄₅₀

As described in Sect. 3.3.1 and 3.3.2, the fit to approximate the Fe loading based on ATN₇₀₀₋₄₅₀ was calculated using 14 samples from the GOK site and a nearby site, Kleinfleißkees. The fit covered a range of 4.0 to 13 and was applied to all samples from the GOK site between 2017 and 2024. Most samples were in the range of the fit, as seen in Fig. C1Figure . Three samples are outside the linear range (ATN₇₀₀₋₄₅₀ > 13). For one of these samples from 2024 (ATN₇₀₀₋₄₅₀=34), the Fe loading obtained via ICP-OES was available and was compared to the Fe loading based on its ATN₇₀₀₋₄₅₀ value and the fit. Using the latter would underestimate the Fe loading by 30%. For this sample, the approximation of mineral dust was based on the Fe loading obtained via ICP-OES. This leads to a decreased uncertainty of 23% for the mineral dust approximation of 2024. For the other two samples exceeding an ATN₇₀₀₋₄₅₀ of 13 (both from 2019, ATN₇₀₀₋₄₅₀ of 21 and 35), no Fe loading from ICP-OES was available. Lacking an alternative, we based the mineral dust approximation for these samples on ATN₇₀₀₋₄₅₀ and consequently increased the error bar for 2019 in Fig. 6 to 87% to consider the underestimation of these samples.”

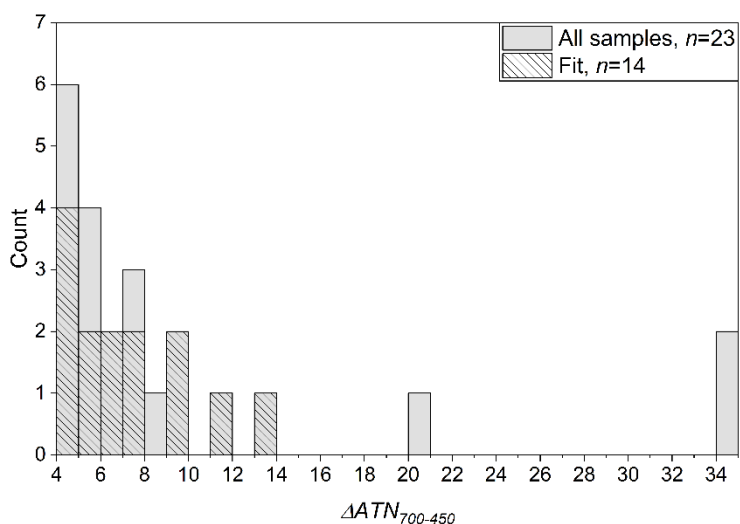


Figure C1: Overview of samples the fit to estimate the Fe loading from ATN₇₀₀₋₄₅₀ was based on and applied to.

We adapted the error bars for mineral dust in 2019 in Figure 6a and b, considering the underestimation by an estimated uncertainty of 30% for the two samples, obtaining a resulting uncertainty of 87% for 2019. We reduced the uncertainty for the year 2024 to 23%, as mineral dust was approximated from ICP-OES for one out of three samples.

Second, the assumption of constant Fe fraction. The method assumes 4% Fe by mass in all long-range dust, but literature and the authors' own comparison (Figure 5) show Fe can vary from ~2% to >11% depending on source and transport history. Please discuss how source-region variability (e.g., Saharan vs. Middle Eastern dust) could bias the long-term trend analysis.

Scheuvens et al. (2013) did an extensive compilation of mass fraction data found in literature and discussed various elemental ratios for source apportionment. Compared to other elements found in mineral dust, they found Fe contents, Fe/Al ratios and Fe enrichment factors to be rather constant and homogeneously distributed in northern African dusts and sediments. For an “average northern African dust”, they find Fe contents comparable to the composition of the upper continental crust. Based on the substantial dataset, significant trends in the distribution were found only for Ca and Mg in northern African dusts.

Comparing dusts originating from a bigger regional scale, Kandler and Scheuvens (2019) report that the local variation in composition of dust can exceed the differences between Asian and African dust.

The data referred to in the comment spanning between 2 and 11% was collected during a citizen science project by Dumont et al. (2023). The study covers a big input area with samples collected from the Pyrenees to the Swiss Alps and the authors describe a gradient in the Fe mass fraction along the dust plume.

In our work, dusts from long-range transport analysed were collected between 2020 and 2024, covering several transport events. Thus, a variety of dust arriving at the site is covered. The observed variation of the Fe mass fraction is much smaller than the range found in literature, which we attribute to the fact that sampling was always conducted at the same site. As described in line 291, the variability of the mass fraction of Fe found for the samples collected between 2020 and 2024 at our site is reflected in the given uncertainty (line 295-297).

Third, the uncertainty is stated as 65% for mineral dust estimates (Section 3.3.2). This is substantial. A sensitivity test showing whether interannual variability is still statistically meaningful under such large uncertainty would strengthen confidence in the conclusions.

We agree that the estimation of mineral dust has a substantial uncertainty, which we do not want to hide. The comparison to other studies giving the spatial variability in ion concentrations puts this substantial uncertainty into context.

Here, we also want to refer to a study by Putaud et al. (2010). They report five approaches for mineral dust calculations based on various tracers used at different sampling sites in the supplementary information. They applied the different calculations to data of three sites and conclude that the uncertainty of mineral dust concentrations can range up to 150%, while assuming it is much lower at several sites.

Regarding the interannual variability, comparing pairs of values for mineral dust input will not give significant differences. This must be considered in future trend analysis, where data from a longer period is necessary. Thus, no changes in the text were made.

Figure 1 provides a qualitative visual comparison of the TOA vs IC method comparison but the text states “very good agreement” without any numerical metric. Please include agreement statistics e.g. percentage of overlapping layer identifications, Cohen’s kappa, number of false positives/negatives per method. This would make the comparison more rigorous.

We appreciate the input regarding the quantification of agreement with a numerical metric. We calculated Cohen’s kappa as suggested. Using the depth resolution of the TOA-profile for the evaluation by the raters (IC approach, TOA approach), we obtained a Kappa Statistic of 0.48. Considering that the two neighbouring profiles may show slight displacement and the suspicious coloured filters, a Kappa Statistic of 0.65 was obtained. According to the classification given in Landis and Koch (1977), considering the limitations of the comparison increases the agreement from moderate to substantial. We added the results in Section 3.1 and the corresponding text reads:

“The agreement of the two approaches was quantified calculating Cohen’s kappa based on the depth resolution of the TOA-profile and evaluating the result based on the classification given by Landis and Koch (1977). While a strict comparison of the layers using the criteria ($ATN_{700-450} > 4.0$ for TOA approach and $pH > 5.6$ and Ca^{2+} concentration $> 8 \mu eq L^{-1}$) showed only moderate agreement, the consideration of the aforementioned limitations (two neighbouring profiles may show slight displacement, layers with coloured filters) increases the agreement from moderate to substantial.”

We added the publication of Landis and Koch (1977) to the References section.

Light-absorbing impurities (LASI) strongly affect albedo and melt timing. The Introduction opens with this motivation, but the Discussion does not return to these impacts -- the paper remains methodologically focused. Please add a paragraph estimating or contextualizing the radiative significance of the observed impurity levels (e.g., based on Di Mauro 2019).

The mentioned effects of LASI are a strong motivation to collect and present concentration and deposition data and thus these basics were included in the Introduction section. However, modelling these effects is not the focus of our current work. While we cannot offer a direct estimation, we expect a bigger influence on the melt timing for years where mineral dust layers occur closer to the surface. There, it can accumulate faster during the melt season, while dust in the lowest layers is covered by snow for the main part of the melt season. We added the following text in Section 3.1:

“In years where mineral dust layers occur close to the surface, e.g., 2018, we expect an increased effect on the duration of the snow cover due to direct and indirect forcing. In contrast, if the mineral dust

layers occur close to the bottom layers and are covered by snow most of the melting season, e.g., in 2017, we expect smaller effects. No further quantification of these effects is conducted within this work.”

The manuscript discusses the issues arising from incomplete snowpack sampling in 2019 (3.2.2 and 3.3.2). However, results for that year are still plotted without any visual cue of lower confidence and results from 2019 are included in averages. Please mark 2019 values in plots as low-confidence (distinct color or hatching) and consider excluding 2019 from interannual statistics, or provide results with/without 2019.

We agree that there is room for improvement regarding the data of 2019 and are thankful for the input. To raise awareness of the lower confidence for the results of 2019, we marked them with a * for both carbon and mineral dust concentrations and depositions in Figure 3 and 6 and gave an explanation in the label of the graphs. Apparently, the mark was not visible enough. We find using another shade of grey or hatching, which was used in Figure 6 for another differentiation, unfavourable and thus stucked with the previous mark, but increased its size markedly.

Regarding the results, we agree that providing results with and without 2019 is beneficial. Thus, we added the results of the interannual statistics without 2019 in Table 1 in brackets. We adapted the label of Table 1 and added a clarification in the text:

“Table 1: Minimum, maximum, average concentrations and depositions and their standard deviations for the accumulation periods 2016-2024 (WinsTC) and 2017-2024 (WinsOC and EC). Results excluding data of 2019 is reported in brackets.”

“Results excluding data of 2019, where the snow cover was sampled incompletely, are provided in brackets.”

Appendix A is very informative and somewhat buried. It explains convincingly why coagulants were rejected, but some conclusions are speculative without quantitative data. Please summarize key quantitative changes (e.g., how much EC or TC deviated in the test aliquots) to support the conclusions.

We are happy to include more detailed information for interested readers. The text now reads:

“We added $\text{NH}_4\text{H}_2\text{PO}_4$ to a limited number of three types of samples (ultrapure water to be considered as blank; sample containing mineral dust; sample not affected by mineral dust) and compared it to untreated aliquots to assess the applicability for our background site ($n=5$ for each sample type and treatment). Addition of the coagulant led to a decrease in pH (ultrapure water: 5.6 to 4.4; sample containing mineral dust: 8.2 to 4.6), which can alter the sample composition, e.g., by removing carbonate carbon. We observed changes in the signal of the flame ionisation detector in various OC and EC fractions (positive and negative) and obtained blanks with doubled TC loadings (untreated: $2.5 \mu\text{g cm}^{-2}$; $\text{NH}_4\text{H}_2\text{PO}_4$: $4.8 \mu\text{g cm}^{-2}$). Addition of $\text{NH}_4\text{H}_2\text{PO}_4$ led to a decreased TC loading for the sample containing mineral dust (untreated: $114 \pm 2 \mu\text{g cm}^{-2}$; $\text{NH}_4\text{H}_2\text{PO}_4$: $80.5 \pm 1.75 \mu\text{g cm}^{-2}$), presumably dominated by carbonate carbon. The removal of substances leading to a broad shoulder in OC4 was comparable to filters treated with HCl, which will quantitatively remove carbonate carbon. The untreated sample without mineral dust showed comparable TC loadings for filters loaded with untreated sample and addition of $\text{NH}_4\text{H}_2\text{PO}_4$ ($17 \pm 3 \mu\text{g cm}^{-2}$ and $19 \pm 2 \mu\text{g cm}^{-2}$, respectively), while only the untreated aliquots showed EC ($0.23 \pm 0.11 \mu\text{g cm}^{-2}$). In agreement with Kuchiki et al. (2015), no influence of an aqueous solution of the substance pipetted on a clean filter on the laser signal was observed. Still, no automatic split point could be set for any replicate of the sample not containing mineral dust treated with $\text{NH}_4\text{H}_2\text{PO}_4$, while the untreated sample showed EC.”

Line 14, 'Using the interference introduced by mineral dust, we identify mineral dust layers and find very good agreement with a complementary method based on calcium concentrations and the pH.' I would like some numbers to show the degree of agreement e.g. correlation coefficient. Please present these in the Abstract. Is there an accepted standard for the 'right' value, to which these two methods could be compared? What do the differences between the results of the two methods tell us about the methods or the samples?

As described in a previous comment, Cohen's Kappa was calculated to quantify the agreement of the two methods. While this showed substantial agreement of the complementary methods, it does not conclude whether layers are correctly classified as containing mineral dust. However, the substantial agreement of the two methods based on different mineral dust tracers (IC approach: pH, Ca²⁺ concentration; TOA approach: change in optical properties of Fe containing compounds) is evidence for a correct classification. There is no accepted standard for a "right" value.

In the Abstract, the text now reads:

"Using the interference introduced by mineral dust, we identify mineral dust layers and find substantial agreement with a complementary method based on calcium concentration and the pH."

Overall, I suggest adding more specific quantitative findings to the Abstract (e.g., "EC median = 11.1 ng g⁻¹; mineral dust deposition up to 2100 mg m⁻²").

In addition to the quantitative findings already given in the abstract (e.g. averages, standard deviations and upper ranges), we use the opportunity to shed more light on the results of the single layers. We included the maximum concentrations of WinsOC and EC as well as the highest mineral dust concentrations in the Abstract. While the latter was already given in the text in Section 3.3.3, we added the WinsOC and EC concentrations of single layers in Section 3.2.2.

The corresponding text in the Abstract now reads:

"Average concentrations for elemental carbon and water-insoluble organic carbon were 11.1±2.5 and 458±215 ng g⁻¹, respectively, and ranged up to 86.3 and 3260 ng g⁻¹ in single layers, respectively."

"Based on thermal-optical analysis and an average iron fraction in mineral dust of 4 %, the approximated mineral dust concentrations ranged up to 25 µg g⁻¹ in single layers. The approximated mineral dust input during the accumulation period agrees well with gravimetric results and ranged up to 2100 mg m⁻²."

We added the following text in Section 3.2.2:

"For single layers, WinsOC and EC concentrations ranged up to 3260 and 86.3 ng g⁻¹, respectively."

Samples are analysed for the period 2016 to 2024. The data is presented. But I would like more commentary to understand the significance of the results. Are there meaningful trends in the data? How do the data from these years fit into what has been found for the preceding years, are there patterns, trends, anomalies? I think there is a lot more context to the measurements that could usefully be added.

We agree that trend analysis would be interesting. However, the limited number of years covered so far (2017-2024 for EC, 2016-2024 for mineral dust) is insufficient for a robust statistical evaluation. As no earlier quantitative data for LASI deposited in the seasonal snow cover at the site is available, an extension to preceding years for further evaluations is not possible. For contextualization, an overview of EC and mineral dust concentration and deposition data from other studies (line 219 - 238 and 320 - 334, respectively) is already presented in the text. At present, we are happy to provide a starting point for future trend analysis with our data.

Lines before 283, do you think the decrease in Fe fraction is due to preferential deposition of particles with a high iron content (density? size?)

This is an interesting question. Dumont et al. (2023) observed decreasing deposited dust mass, decreasing particle size distributions and decreasing total elemental Fe mass along the dust plume path from the Pyrenees to the Swiss Alps. Kandler and Scheuvens (2019) report a tendency to downwind-fining with largest particles close to the source and a quick depletion of quartz (mass) from the aerosol due to its particle size. Kandler et al. (2007) describe variations in the mineralogical composition of mineral dust sampled in Izaña, Spain, for different size ranges. Based on these and other studies, changes of the dust composition during transport are evident.

However, these investigations exceed the scope of our work and are not accessible through the data we collected. We did not make modifications in the manuscript regarding this topic.

I like the discussion lines 302 to 310. But when you say 'the uncertainty of the mineral dust approximation is 65%', which result does this refer to? It is unclear, and rather than uncertainty, I think you may mean variability?

The uncertainty of 65% results from the variability of the share of Fe in mineral dust (25 ± 3) and the spreading of data points around the fit to calculate Fe loading from $ATN_{700-450}$ (lines 295-297). We agree that the inserted information about the two parameters may be confusing but cannot offer an alternative text with increased comprehensibility while keeping all the information we want to give. As the result is a mixture of different variabilities and describes the resulting uncertainty within which the actual result is expected, we did not change the term.

line 324, why can they only be underestimated?

This referred to the incomplete sampling of the snow cover in 2019. If mineral dust layers occurred in the unsampled part of the snow cover, the reported deposition will miss them and an underestimation results. On the contrary, the concentration in this case can be positively or negatively biased, depending on the occurrence of dust and the water equivalent of the unsampled layers as discussed in line 316 to 317. To clarify, we modified the text in Section 3.3.3, which now reads:

“As mineral dust deposition in 2019 may be underestimated due to incomplete sampling of the snow cover, it may be the second highest or highest year regarding mineral dust input.”

Line 416, 'Data used in this work will be uploaded to TU Wien Research Data and the doi will be added here.' I think that at this stage in publication, it is time to upload the data and make the doi. For readers, reviewers, etc. The data must now be in final form if you have analysed it and written the paper, correct?

Yes, the data is prepared. Obviously, we misunderstood that the data should be uploaded before the review. We thought the uploaded file should already include the input of referees. The data was uploaded to TU Wien Research Data and the text in the Data availability section changed to:

“Data presented within this work are available at TU Wien Research Data (<https://doi.org/10.48436/yb10b-yfc83>).”

(2) Technical

Advisory bodies (SI; IUPAC) advise that the symbols used for variables should be italicized. Please apply this convention at multiple locations e.g. 'n', 'R²' and so on.

We are thankful for the remark and applied the changes to the manuscript at several positions for the symbols “n”, “R²”, “λ”, and “ $ATN_{700-450}$ ” in the text and the figures.

There are a lot of abbreviations. LAI, LASI, LAP, GOK, GAW, TOA, WinsTC, WinsOC, EC, OC, ICP-OES, IC, WMO, FLK. They are often a barrier to understanding for non-experts. Some are used rarely. Many (ICP-OES, LAI, LAP, WMO and RF) are only used once. TOA is defined at the fifth use, not the first use. RF is undefined. Recommend to use best practice - define an abbreviation on first use and only define an abbreviation if it is used multiple times. Some (WinsOC & WinsTC) are defined twice. Avoid abbreviations in Abstract whenever possible since it should function as a standalone summary.

We agree that the use of abbreviations may be a barrier to understanding the text. We critically reviewed the text. If an abbreviation is only used once, it was only kept when the abbreviation is better known than the written-out word. This includes analytical techniques as ICP-OES or organisations as GAW or WMO. We defined these abbreviations to explicitly give their meaning but kept the abbreviation for best comprehension. The use of different nomenclature for light-absorbing impurities or particles (in snow) - LAI, LASI, LAP - is inconvenient and hinders literature search for the topic. Thus, we included all forms suitable for our analytes including their abbreviations.

We now define TOA and IC at their first use. The corresponding sentence in Section 2.1 now reads:

“At the end of the winter accumulation period (end of April or early May), snowpacks were sampled in increments of 20 cm for carbonaceous compounds (quantification via thermal-optical analysis (TOA), referred to as TOA-profile) and 10 cm for Ca²⁺ concentration analysed via ion chromatography (IC) and pH (referred to as IC-profile).”

We are not sure what the last sentence in the comment refers to. In the Abstract, no abbreviations were used. In case the referee referred to the Conclusions section: We agree that it should function as a standalone summary. Thus, we defined abbreviations again here, risking double definition of WinsTC, WinsOC, EC and TOA throughout the manuscript. All abbreviations are used more than once in the Conclusions section.

There are times when the meaning is unclear, please rewrite to improve clarity. Examples:

Line 131, 'Since the temperature range given previously was not always reached for our set of samples,'

We agree that a more detailed description is beneficial, especially because both referees noted it. To clarify, we added an equation for the calculation of the parameter and adapted the text, which now reads:

“As described by Kau et al. (2022), the transmitted laser signal ($\lambda=660$ nm) was evaluated in the calibration phase. At this point, the actual analysis of carbonaceous compounds is already finished. Oven and filter sample are just cooling down, but data is still logged to record the calibration peak. Kau et al. (2022) evaluated a temperature range between 700 and 400°C. Due to small adjustments of the insulation material, the lower temperature, i.e., 400°C, given previously was not always reached at the end of the calibration phase for our set of samples. Thus, we now evaluated the change in transmittance observed between 700 and 450°C using the transmitted laser signal I_{700} and I_{450} , respectively. Converted into a dimensionless temperature dependent attenuation ($ATN_{700-450}$, see Eq. (1)), samples that exceeded a value of 4.0 were classified as containing mineral dust. For clarification, $ATN_{700-450}$ corresponds to $ATN_{700-400}$ defined in Kau et al. (2022), however, using the intensity of the transmitted laser signal at 450°C.”

$$ATN_{700-450} = 100 * \ln \left(\frac{I_{450}}{I_{700}} \right) \quad (1)$$

Line 153, 'Considering layers with coloured filters would match the two approaches in 2018,'

We changed the text for better comprehensibility. It now reads:

“For 2018, the two approaches show the same number of mineral dust layers when the layers with coloured filters are considered. Here, the mineral dust layer observed within the IC-profile is divided in two samples in the TOA-profile.”

Line 197, 'EC concentrations for single layers and changes for layers including mineral dust are shown in Figure 3 exemplary for 2020 and 2024.' -- 'exemplary' is unclear, used in a way that doesn't align with it's meaning -- do you mean 'EC concentrations for single layers and changes for layers including mineral dust are shown in Figure for 2020 and 2024, chosen to exemplify the

The text now reads:

“EC concentrations of single layers and changes for layers including mineral dust are exemplified in Figure 3 using the data of 2020 and 2024.”

Line 75, 80, 119 and elsewhere, 'filtrated' is not a word, use 'filtered'

We thank Matthew Johnson for the correction and changed the text accordingly.

Line 79, replace 'Contrary,' with 'On the contrary' or 'In contrast'

Thank you again, we changed the text to “In contrast”.

Check x-axis label in Figure 4, remove '()'

$ATN_{700-450}$ is a dimensionless quantity. To prevent confusion, we adapted the suggestion to leave out the previously given “()” in Figure 4 and added the information of the parameter being dimensionless in the text in Section 3.1:

“Converted into a dimensionless temperature dependent attenuation ($ATN_{700-450}$, see Eq. (1)), samples that exceeded a value of 4.0 were classified as containing mineral dust.”

We want to point out additional changes, which were implemented independent of the referees' comments:

For a more accurate description, we changed “share of Fe in mineral dust” to “Fe mass fraction in mineral dust”.

The given WinsTC concentration of the snowpack collected in 2016 slightly differed from the actual concentration (385 and 345 ng g^{-1} , respectively) due to a calculation error. We apologize for the mistake in the submitted version of the manuscript and corrected the average and standard deviation values of WinsTC given in Table 1 (455 and 207 ng g^{-1} instead of 459 and 204 ng g^{-1}). We also updated the WinsTC concentration of 2016 in Figure 3. The WinsTC deposition of 2016 and any conclusions made in the submitted version are not affected.

In Section 3.3.2, we noticed an error in the classification of the dust-laden samples to snow and rime. Previously, 7 snow and 1 rime sample were reported, while it should read 5 snow and 3 rime samples. We are sorry for the error in the submitted version of the manuscript and corrected it. This change does not affect any conclusions made.

The legend in Figure 1 previously only showed “ATN” instead of “ $ATN_{700-450}$ ”. We clarified the parameter used here.

References

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