Referee report on Kau et al. ‘Thermal-optical analysis of snow samples – challenges and perspectives introduced via the occurrence of mineral dust.’

General comments

This manuscript presents a new approach in dealing with the interference of mineral dust (hematite in particular) that might occur during thermal-optical analysis of filter substrates. The paper would be highly welcomed in the literature, due to the reoccurring phenomenon of mineral dust disturbing OCEC-analyzes. Since it is a novel approach, the text is well suited for AMT. In its current form, however, the paper needs some substantial work. Currently, the full potential of the manuscript has not been explored, but rather, corners appear to have been cut, resulting in several sections that either lack or provide minimal information. As an example, the approach is tested for high alpine PM10 filter samples, but only two samples are available. More data would be needed to provide a more robust basis for the authors to discuss the results on that topic. Is there data available data from another site (other than Sonnblick?). Similarly, the snow samples from Sonnblick could be compared with snow samples from another location. This would mean more work, but in the end, it would only and built-up the discussion/conclusions, and ultimately, strengthen the manuscript. With that said, I would reiterate that major revisions are necessary for this paper that could have great potential breakthrough in this area of science. Please see more comments below.

Major comments

Section 3. The different instrumentation used in the study seem applicable and impressive. Still, it is difficult for a reader to see their place and role in the different steps of the approach. It could easily be misinterpreted that the instruments were just randomly chosen when the authors clearly thought about how to best utilize their instruments for this method. One suggestion is to provide an introductory section before each instrumentation is expanded upon. In this introductory text, it should be explained why the instrument is used. In a way, it should end up as a flow-chart explaining the different steps of the analysis, providing future readers with a roadmap of the approach enabling them to easier follow along.

The aims of the paper should be scrutinized. Currently, there is a mismatch between the aims (denoted as interests in the introduction) stated in the introduction and once they are re-stated in the conclusions. The aims should be clearly reviewed and revised in the introduction. On the whole, the conclusions that are discussed particularly in lines 298-308 are not supported by the data currently presented in the paper (e.g. recommending a rerun of analyses; that the approach was successfully applied to PM10 filters also, see additional comment on this below).

Section 4.2 and Fig.2. For the samples contaminated with the reference hematite, was it one batch of filter samples? In other words, was this step ever repeated, with another independent set of hematite contaminated filters? In order to test whether the same pattern would be repeated in the filter samples this is a suggestion of work to be done. Similarly, what about conducting experiments with reference filters

containing Fe originating from a different source than hematite? This could further confirm the claims made that this method works well for hematite by not other types (which currently is not strongly supported by much data; only the tunnel samples).

Section 4.3. It is understood that there are only two PM_{10} samples from Sonnblick. Yet, two samples are not enough to support the claims the authors want to make in the text (concerning the applicability of the fit for PM_{10} samples). The authors need to find a way to expand with more samples for the PM_{10} or majorly down-play the importance of only two samples in the claims they are making.

Minor comments

Title. As it currently reads the title of the paper suggests that *challenges* and *perspectives* are to be discussed. The use of such wording would be more appropriate if this was a review article. I would encourage the authors to critically think through the title and modify it to reflect the paper better.

Line 17-18. I would advise the authors to transform this sentence into a more informative sentence. In other words, do not mention what you are discussing in the manuscript without adding something about it, but rather, be specific and give highlight details of the results and conclusions that can be made concerning this method. It would not necessarily need to make the text much longer, but all the more informative for future readers.

Line 21. In-text references, it is neither alphabetical nor chronologically, please pick something and be consistent throughout the manuscript.

Line 30. This is the wrong reference to Schwarz et al. and his work on rBC in snow. The 2006 paper deals with atmospheric BC. I would recommend the authors to look for his 2012 paper, that deals with rBC in snow samples.

Lines 46-48. It is difficult to understand what the authors mean with this sentence. How is it not the main point? (or do you simply mean that it is not the point in the paper to differentiate between WinsOC and OC?). How can the concept be applied to the analysis of both types?

Line 53. Adjusted the method accordingly how? Please provide the reader with what was done previously (even if only brief).

Lines 56-57. Please be more specific in this sentence. This is very broad and not very descriptive of what is addressed in the paper.

Line 65. It is stated that snow samples were collected near the GAW station. Please be more precise. Overall, any more info that could be provided on the snow samples could potentially be useful for future work. One suggestion would be an informative table (which could easily be put to the supplement) containing relevant info on the snow, e.g. fresh/old, density, sampling depth, etc.)

Line 76. Please define PM10.

Line 96. What is method 3052? Please inform (even in brief).

Lines 116-131. The text along these lines can easily by moved to an introductory (or background section), as these results are not specific to this study, but rather has been known from previous work.

Line 117. TOA of snow samples. Obviously, the actual snow sample is not analyzed in the TOA, but rather the filter with collected particulates from the snow. Please adjust this throughout the manuscript to be as concise as possible.

Line 118-119. Does it not depend on what type of minerals are present on the filter? Some minerals will not change color, nor darken the filter substrate. If there is hematite present on the filter it is evident that the transmittance is affected, but not all MD. It is stated further down in the manuscript (line 226) that hematite was a minor contributor to the MD. Please be specific in the text.

Line 132. How was the Fe content determined? And also, what do these numbers translate into for MD concentration for the snow samples? Please include this as a guide for future readers who might have a MD concentration and would want to compare that to the range of samples where the approach here is applicable to.

Line 150. What is meant with high background of the filter matrix? If it refers to hematite, how did it get into the background of the filter?

Lines 180-183. One could argue that this information is not novel to the study here, but has rather been known from previous work, and is actually part of the motivation for this work. Thus, it should be moved to the introduction/or alternatively in a background section after the introduction.

Line 199. Please remove the equation from the text, it should be inserted on a separate line (as well as any other question from the text).

Line 210-211. Unloaded filters at room temp I_0? Previously, it is stated that I_0 is for 400°C. Please clarify.

Lines 217-218. It is mentioned that such high hematite loadings are rarely found. What type of concentration does it equal in the snow? Please provide numbers on this, is it above ppm?

Line 225. SEM analyses confirmed such differences. Please be more specific by including some results.

Lines 225-226. Hematite was larger for the reference than the snow originating particles. Any idea of how much larger? Please include this information.

Fig. 4. The figure would benefit if both axes would be modified to more accurately reflect the range of the sampling points. In other words, the sampling points would be more centered in the figure.

Lines 278-279. How do we know of the different originating Fe, as in where is the evidence for this claim? Visual inspection also?

Line 285. Please add that it is during the calibration stage during TOA.

Line 290. Have the authors actually tested this for the other protocols? It should be the case, but if it was tested, this sort of statement could be made with greater confidence.

Data availability. This does not comply with current AMT standards on data availability.