Comment on acp-2022-156
Anonymous Referee #1

Referee comment on "Measurement Report: Black carbon properties and concentrations in Southern Sweden urban and rural air – The importance of long-range transport" by Erik Ahlberg et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-156-RC1, 2022

REVIEW Preprint acp-2022-156

The work presented by Ahlberg and coauthors investigates the variability of black carbon properties at an urban and rural site in southern Sweden. This manuscript fulfils the requirements of a "Measurement report" since it treats specific aerosol measurements and processes confined in a restricted area and time. However, the manuscript is structured as a full "Research Article" and falls a bit short on certain aspects of data analysis and interpretation. The final consequence is that the motivations, methods and goals are not always clear. I recommend major revision and resubmission. I hope that the major and specific comments listed below will help the authors in the rebuttal process.

MAJOR COMMENTS

First, there is an evident problem with sections and subsections. The article is organized without subsections; thus the assimilation of the scientific message becomes particularly complicated. The structure should be modified including subsections in the "methods" and result "sections".

The overall motivation behind the paper is unclear. The authors mentioned climatic and
health implications, but these are described very generally without a local (Swedish) perspective. The manuscript does not provide enough measurement time to address climatic issues but could draw a nice, even if very short, picture of air quality. I think this should be the redline of the entire manuscript and should include as motivation: the health impact of aerosol emission in Sweden (e.g. death per year), the history of Swedish reduction strategies and subsequent effects.

There is nothing that can be practically done about this, but the fact that the urban and background measurements are not simultaneous is the weakest point of the manuscript. The authors should convince the reader that the background aerosol population do not change drastically from July to October. Till that point, the results shown in Figures 1,2,3 could be affected by many atmospheric processes such as precipitation, and changes in emission large scale circulation. Potentially due to this problem, the goal of the authors is not always clear.

Technically speaking there are some additional soft spots. It is very much not clear how the absorption data were treated and corrected, as a consequence the data are quite questionable. See specific comments. Moreover, the Aethalometer data are not essential to the scope of the paper since the SP2 provides mass concentration, size distribution and mixing state proxy.

It must be described more clearly when ACTRIS data (absorption and chemical composition) were used. The reader realizes only towards the end of the manuscript that a year-long dataset was used. No description or introduction to these data is ever given.

SPECIFIC COMMENTS

L17: health effect is mentioned also in the introduction, but it is never really explained.
It is an odd way to start a paper. I would remove the first sentence since it might apply to all atmospheric species.

This is the motivation of your work and also the first sentence of the abstract. Still, I have zero ideas about how BC affects the climate and human health.

I would provide an example for all properties or not report any example. Listing only the lensing effect does not add any relevant information since absorption enhancement is not a topic of the paper.

It appears like the reference for transport and deposited dose is missing.

Health and toxicity are mentioned a couple of times, but it is not yet explained how fundamental properties are connected to health.

Main take-home message is summarized here. I think it does not belong to the introduction. Potentially makes more sense as a final statement of the abstract.

Please provide some evidence supporting your statement. July September is a long-time gap. I expect different dilution due to boundary layer height (I imagine lower temperature in October), different precipitation and washout, or different chemistry due to shorter sunlight duration...

SP2 is not yet introduced. Move this last sentence to the instrumentation section. Since you mention turbulent flux. Can you estimate a loss fraction?

This chapter is extremely long and complex. I would add a table listing the deployed instruments and measured variables at the two sites.

is good practice to provide the name, city and country of the manufacturer. Missing everywhere in the manuscript.

is this the saving rate of the SP2? A person not familiar with the SP2 would not understand what was does it mean. I think is not so important to be mentioned.
L90-91: What do you mean by “not satisfactory”? Was the reason connected to a wrong sizing of the DMA or a decrease in the performances of the SP2 (decreasing laser power, misalignment)? Are then the data valid?

L98: 62 nm is a very small diameter, but those particles will not contribute significantly to the total mass. My issue with this choice is that most, if not the totality, of previous SP2 paper, reports rBC particles starting from 80-90 nm...which I might consider the safe side. Considering that you do not provide any counting efficiency, I do not understand why you chose such a low cut-off.

L104: the coating is not directly measured by these two detectors. I would use caution with these statements since people might think that the SP2 directly provides coating thickness of BC-containing particles, which is well far from reality.

L106-111: I am genuinely confused by this explanation. Are talking of delay-time or LEO-fit. To me, it appears like a mixture of the two. Please rewrite it. If the position-sensitive detector was not used is not worth mentioning, since it adds confusion.

L114: since you mention Weingartner...what Cref value was used? Was it calculated for this specific aethalometer or taken from Weingartner or Collaud-Coen or Zanatta? These papers are based on AE31 though and not AE33.

L125-133: Please add the chemical species identified by the SP-AMS. If all presented rBC mass is derived from the SP2 the calibration for refractory material is even described? In what sense the SP-AMS at the urban site did not provide robust results (instrument malfunction, wrong calibration)? Is this the reason why all AMS graphs at the urban site are plotted with arbitrary units? I have the feeling that this issue and the SP2 calibration problem (L90) undermine the credibility (accuracy, reproducibility) of the dataset and, as a consequence, the full manuscript. The authors should explain in more detail why and how the SP2 and SP-AMS data are still reliable despite the technical issues.

L145: just a comment to point out that 64 nm of electric-mobility diameter does not correspond to 64 nm of mass equivalent diameter.

L171: back trajectories are not shown

RESULTS AND DISCUSSION
L183-191: There is no context to your observation. Up to me, these are low concentration for being an urban site.

L186: it is already clear from its concentration that the curbside is not extremely polluted. At what percentage difference you would define extreme pollution? And based on what process?

F1: provide error bars. Does the analysis include the weekends?

L191: MAC of BC is not the only reason for the difference in AAE. Different AAE might be caused by a change in the chemical composition of absorbing aerosol or a change in the relative concentration of aerosol absorbing more light at the lower wavelength. If this is the case, MACbc will remain the same while AAE of total aerosol will increase.

L200-212: As it is also pointed out in the text, the rBC number fraction mostly depends on the diameter quantification limits of both the SP2 and especially DMA rather than on aerosol properties. So, I am not sure what should I retain out of this subchapter.

L213-223: Here several factors must be considered and I strongly believe that the SP2 size cut is not the problem. Even if the SP2 size range could be easily accounted for by fitting a lognormal to the size distribution. Anyhow, no details are reported about the correction used for the AE-33 and I believe the problems are more connected with absorption calculation rather than the SP2 detection range. I recalculated the MAC from the values reported in Table 1. I recalculated Babs by multiplying Mebc by 7.77 m2/g. Then I calculated MAC as the ratio of Babs and MrBC. So, I obtain MAC values above 25 m2/g. This value is very similar to the mass attenuation coefficient used in the past to convert directly attenuation coefficients to eBC in the old AE31. So, I cannot say what happened here exactly, but I think that no Cref or correction was applied. I think you need to do a bit of extra thinking here and reconsider the relevance and accuracy of eBC measurements.

L2017: was the 10-20% calculated or is it just a gentle guess? If it is calculated why is not applied to the measurements? Lensing effect cannot be excluded, but I hardly think that this is the main reason behind the eBC-rBC difference: You are using a MAC 1.6 times smaller than Swedish ambient values (Martinsson), while an additional 10-20% is coming from size cut. There is too much uncertainty to speak about absorption amplification.

L220-223: so why not use the Martinsson MAC? There is no explanation behind the choice of 7.77 m2/g.
L234: the fact that aerosol diameters are affected by cloud processing is true. I wonder how this is relevant to your study. If this is a general statement, please provide at least some references.

L242: very few people know what is the broadband channel. Since it is not essential information to interpret your result, I would remove it in the result section.

L245: these effective density measurements are interesting. You could show the mass distribution for all selected diameters…it would help to understand your text. I might have missed this info, but did you measure effective density at the rural site? Do you see this bimodal distribution?

L271: At what altitude the back trajectories are passing over Malmo?

L275: No actual description is given for the one-year-long dataset. It is very confusing, especially without a dedicated subsection with numbering and title.

L300-320: did you use the one-year-long dataset for the malmo influence? Not clear to me. Specify the use of the 1year dataset. If not, I am not very persuaded that your 15 days measurements could show or not show any systematic impact of Malmo emission on background aerosol concentration.

L331: ok, but why?

L339: first-time HR-ToF-AMS is mentioned
L349-352: what are regional traffic sources? You clearly show that the urban environment is impacted by traffic; so, emission reduction policies will benefit the urban population. But it will have a less evident impact on rural locations. I find this final statement a bit confusing, I suggest rephrasing.