

Interactive comment on “Characterization of long-term and seasonal variations of black carbon (BC) concentrations at Neumayer, Antarctica” by R. Weller et al.

Anonymous Referee #2

Received and published: 6 November 2012

Referee Comment

General comment:

The manuscript presents interesting results on Antarctic aerosols and their physical properties. The dataset is unique and the analysis is done in an appropriate way. The authors honestly discuss the problem of data comparison using two similarly functioning, but different instruments. Even when a comparison between two instruments failed, the dataset is worth publishing and parameters can be published by the discussion of individual time series originating from one and the same instrument. Figures and tables are easy to read and figure and table captions are also clear. In addition,

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a thorough discussion on detection limits of presented parameters is given. Also, the introduction gives a good overview of what has been published before. All in all, the manuscript is worth for publication in ACP, when a few particular comments are addressed.

Detailed scientific comments:

2 Experimental techniques and data evaluation methods

Page 25362, Line 13 Hourly averaged MAAP data went rarely to negative values (12.8% of the data), whereas for the AE10, even the 4-h samples fluctuated around zero (26.9% of the data). Comment: What kind of averaging was needed for the PSAP to have no zero values. Did they also fluctuate around zero for weekly values. Why have you been choosing the average time you went for in the final analysis?

Page 25362, Line 27: The accuracy of BC measurements by aerosol absorption photometers depends on the validity of the specific BC attenuation cross section (QBC) used for the filter material, which was $14\text{ m}^2\text{ g}^{-1}$ for the AE10 for Pallfex filters (note that a factor of $19\text{ m}^2\text{ g}^{-1}$ was recommended for often used quartz filters) and $6.6\text{ m}^2\text{ g}^{-1}$ for the MAAP. Comment: Please report why you have been choosing the BC attenuation cross section for the two instruments. Did you do a comparison with chemical sampling and determination of BC with a different method? As I understand, the attenuation cross section is a filter based constant for specific wavelengths, but when the filter gets loaded, it will also depend on the load material, so being dependent on the type of aerosol collected. The cross section you choose, clearly defines the BC mass values you get. I propose to argue at this point why you have been choosing for the values you took. Report also your estimate on uncertainty on the BC attenuation cross sections.

Page 25364, Line 1-3: In order to assess the impact of BC on the optical properties of the aerosol, BC concentrations have to be converted to particle absorption coefficients $\alpha_{\text{p}}(\lambda)$ (unit: $1\text{ Mm}^{-1} = 106\text{ m}^{-1}$) by using the specific BC absorption on the filter mate-

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rial (QBC). Comment: Further on, on page 25364, line 1 – 13 you start the discussion again, calling this parameter the specific BC absorption coefficient. This sentence is wrong, it is rather more the other way round if you use optical techniques. You receive from these instruments absorption coefficients, which have to be converted to BC mass concentrations. It is very important that you name your parameters in a clear, appropriate, and consistent way. Here, I do not understand that you discard AE10 BC absorption coefficients. On page 25362 you write $14\text{m}^2\text{ g}^{-1}$ instead of $19\text{m}^2\text{ g}^{-1}$ was actually used. So if you received BC mass concentrations from the Aethalometer and have a reliable specific BC absorption coefficient, why can you then not talk about absorption coefficients received from the AE10. Please clarify this section!

Page 25363, Line 23: Apart from the contamination screening procedure an additional data selection was necessary for the AE10: Comment: Please explain your contamination screening procedure!

3 Data presentation

Page 25365, line 25 ongoing. We tried to homogenize both data sets using this regression by rescaling the AE10 data with the calculated slope and intercept. Unfortunately, it emerged that this approach did not really improve the situation, most probably due to the poor correlation between both data sets. Thus a reliable homogenization of both time series emerged unsuccessful and we preferred a separate trend analysis on both data sets. Comment: It is reasonable that you discuss the comparison of the two datasets. It is - of course - unsatisfactory that the comparison does not show a correlation. Nevertheless I think it is good to discuss datasets separately as you do this in the following, this way trends in the individual datasets can be observed. But I do not agree on the paragraph starting with line 25 and ongoing. From a mathematical point of view, it is clear that this kind of homogenization cannot be successful as it does not improve the dataset by using this method based on a bad correlation found in the original data. Please shorten this section.

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Comment: I would discard Figure 2. It does not add any new value to the study. The facts can be observed from Figure 1.

4 Discussion

Page 25367, line 17, following paragraph. Comment: For this paragraph, please state in each comparison of BC mass concentration, which technique was used. It is a bit confusing here to see if it was atmospheric or ice core data.

Technical and language comments:

Page 25367, line 1: assumptions Comment: assumption

Page 25367, line 6: except some Comment: except from some

Page 25367, line 22: than annual Comment: than the annual

Page 25367, line 25: BC concentration Comment: BC concentrations

Page 25368, line 20: during storm Comment: during storms or stormy conditions

Page 25368, line 27: maximum Comment: maxima

Page 25370, line 14: BC concentration Comment: BC concentrations

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25355, 2012.

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