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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Continental Scale Antarctic deposition of sulphur and black carbon from anthropogenic and volcanic sources" *by* H.-F. Graf et al.

**Anonymous Referee #2**

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The manuscript by Graf et al. reports on modelling of the concentrations and deposition rates of sulphur and black carbon from pollution sources in Antarctica. Pollution sources in Antarctica considered in this study are ship traffic, emissions of research bases, air traffic and the Mt. Erebus volcano. The emission rates of these sources are not discussed in this manuscript but in a separate paper which recently appeared in ACP. This manuscript focuses on a short description of the chemistry climate model used in this study and the presentation of results largely illustrated by concentration or deposition flux maps covering the Antarctic continent and the surrounding South Polar Ocean.



Too my knowledge this is the first time a study has been aiming at quantification of the effect of local emissions of pollutants (sulphur and BC) in Antarctica. The subject is therefore well suited for publication in ACP.

What is written in the manuscript in its present form I find largely well written and presented. However, given that the pioneering work of compiling an emission inventory for Antarctica was published in a different paper, I find this manuscript incomplete and falling short in validation and interpretation of the model results. Therefore I would like to encourage the authors to invest some more work into this manuscript.

My main concerns regarding the present manuscript are the following:

In terms of the atmospheric sulphur budget the authors mention initially that the biogenic (DMS) source of atmospheric sulphur in Antarctica will be the dominating source, but they consider the knowledge of this source as so uncertain to not include it at all in their model study. I wonder if the uncertainty in estimates of the source strength of this oceanic source is indeed any higher than the estimates for the pollution sources and would like the authors to expand on this.

In any case, even if one argues that the modelling in this study is confined on purpose to the pollution sources mentioned above, the discussion of results and the conclusions urgently need to be set much more clearly into context of the dominant role of the DMS-derived sulphur. I find here Figure 4 and the associated discussion in particular misleading and think a mere comment in the Figure caption ("excluding DMS contribution") is not appropriate. The relevance of ALL source contributions should be addressed.

In terms of black carbon I find that the results are hardly discussed in the manuscript. Even if difficult to quantify, the long-range transport of BC from lower latitudes to Antarctica should be mentioned as it very probably forms the "background" of BC in the Antarctic troposphere. Are the authors aware of the Pereira et al. paper (2006, J. Geophys. Res., 111, D03303, doi:10.1029/2005JD006086), which discusses BC measured on the Antarctic peninsula.

I would like to see a more consequent presentation and discussion of the results in terms of seasonality. Partly this is done, partly it is omitted. Figure 1, for instance, only presents summer results. Why? The ground-based measurements of atmospheric concentrations of sulphate and BC show considerable seasonality in Antarctica (the authors already have some references to papers discussing this included) and to address the seasonality aspect helps to structure the discussion.

This modelling study is not presenting any attempt to validate the model results. As someone working in the experimental field I always wonder about the value of a model in such a case. The authors mention only in one sentence (end of 1st paragraph of Section 3) that they have compared with meteorological fields. What does it mean to have "good agreement"? This is entirely hidden from the reader and should be corrected in a revision. For instance, the paper model includes calculations of wet deposition. In that case, I guess, it would be possible to compare calculated and observed precipitation rates over Antarctica. Does this fit within a reason or not? From that one could get some feeling on the correctness of wet deposition rates of a particular species.

Furthermore, the model should be able to calculate aerosol sulphate concentrations, I guess. The authors focussed on presenting gas phase SO2 concentrations, but if they would look (also) at sulphate concentrations this would offer much more possibilities to compare with actual observational data from Antarctica. Even if the DMS-derived sulphate would continue to be not included in the modelling, one could get a much better feeling for the model results in comparison with measurements.

More specific comments:

BC should be defined as black carbon at first occurrence in the text.

SO4 is used many times in the manuscript. While it is clear what is referred to, it is not correct. Either write "sulphate" or use the correct chemical description indicating the



ion character "SO4ˆ2-" (can't do it properly in plain text here).

Please use sulphate/sulphur (or sulfate/sulfur) consistently throughout the text including figure captions.

Page 26579, line 5. "downwind" — should this not read "upwind"?

Page 26579, line 12. Brackets are not correctly set at the reference.

Page 26579, lines 24-26. I happen to know the measurements at Neumayer fairly well and the statement here with respect to the Minikin et al. reference is misleading. The sulphate measurements at Neumayer are not affected by the penguin colony close to Neumayer (and the given reference does not suggest this as well) because the colony is same direction as the main station and sampling is stopped if wind is from that sector.

Last paragraph of Section 1. Long-range transport of aerosols and trace gases via free and upper troposphere should at least be mentioned here as well.

Section 3. How is the model dealing with the topography of Antarctica?

Section 3, first sentence. "contribution to total sulphur" is misleading at this point, as DMS-derived sulphur is not considered (as explained some sentences later).

Section 3, 2nd paragraph. This is an example why I mentioned before that I feel BC is not well discussed in the paper. A paragraph corresponding to this one on SO2 could be written for BC as well, I guess. Average life time (in the model), concentration maps etc.

Page 26582, line 25. Please explain how weather conditions influence SO2 build-up.

Page 26583, line 1. "crater Island" — is this a correct expression?

Page 26584, lines 12-13. This is an example where I think some more interpretation/explanation of the model results should be given, not just a description. There are more statements like this. In this case: Why is the interior unaffected according to the model? I guess it can be explained by the average wind field, but this deserves a sentence more than a mere description of the result.

Similarly, page 26585, line 7-8.

Conclusions. This section is too hurried. Is Mt. Erebus also a source for BC as the first sentences seems to imply? I suggest to divide the conclusions on S and BC results. The conclusion should definitely address the aspect of the DMS-derived sulphur components dominating the budget (see my comment above).

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26577, 2009.