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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The multi-seasonal NO_y budget in coastal Antarctica and its link with surface snow and ice core nitrate: results from the CHABLIS campaign" *by* A. E. Jones et al.

Anonymous Referee #1

Received and published: 16 April 2007

REVIEW of

MS-NR: acpd-2007-0091 Title: The multi-seasonal NOy budget in coastal Antarctica and its link with surface snow and ice core nitrate: Results from the CHABLIS campaign Author(s): A. Jones, and CHABLIS NOy budget team

This paper describes the interpretation of data taken at the British Antarctic Survey station Halley. The NOy budget is discussed as it changes with seasons; the budget is derived from measurements of the individual NOy species. Using these data, two additional questions are investigated: the possible sources of NO3- to the snowpack and the source strength of NOx emissions from the snowpack.

The real novelty of this work is the fact that 13 different NOy species / groups of species were measured individually using 6 different techniques over several seasons. This documents in detail the seasonal changes of these species and their contribution to the budget. It is a major improvement over past efforts by various groups to investigate the Polar NOy budget using the 'NOy converter'.

This work, however, is not a measurement paper, and therein lies one of the major problems: Section 2 on observations and data averaging refers to five measurement papers, that are all referenced as 'in preparation'. In my opinion the papers describing the methods and/or the original results should be published first. The current paper discusses results that have not undergone critical evaluation by the community, and it gives no possibility to assess the quality of the data. I recommend strongly not publishing this paper until all of the 5 data papers are at least submitted (if they are submitted to an on-line discussion journal) or published. Otherwise I see a potential for creating a circular reference, which would be a problem for the scientific community and the validity of the peer-review process. Of course, the authors may choose to include all relevant details on their measurements (methods, sampling methods and errors, 3sigma; detection limits, etc) in the present paper.

Further, as detailed below in the specific comments; I do not see that the findings and discussion of this work warrant publication as a separate paper. I see very little new science in this work. The authors state in the discussion that their next step is a 'full 3-D model assessment'. I recommend that the present work not be published separately. In shortened and revised form it would make a good introduction to such a modeling paper.

An additional general comment: the authors do not always give proper credit to related work and do not clearly indicate their own new/original contribution.

Specific comments;

Results 3.1.1 / 3.1.2

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The measurements at Halley do not seem to tell us anything new. This paper seems to mostly confirm previous work in the Arctic (without always explicitly referencing such work) and Antarctica.

The seasonal dominance of PAN and organic nitrates in winter and the photochemically produced NOx and HONO are a features of polar atmospheres documented for many years in the Arctic. Granted, the mixing ratios are much lower in Antarctica, and there are surely regional nuances, but overall figures 1 and 2 are exactly what one would expect to see. Jones et al. mention a few select publications on this in their introduction themselves. They state that PAN in the Arctic is driven by long range transport, but fail to state (in this section) whether this would be different in Antarctica. If PAN is the dominant NOy species in the winter it would be interesting to see what its lifetime and sources are.

(In the introduction Solberg et al. is quoted as discussing PAN as NOx source. Their work actually showed local formation of PAN from NOx.)

Results 3.1.3

What is the benefit of this section? Records from different sites that were taken in different years with different instrumental approaches are compared here. The last sentence of this paragraph states that the Halley measurements “capture the dominant NOy species”. This, however, implies that the NOy converter does capture them in the first place. It seems that this section is included to show that the NOy converter, despite all its known and published shortcomings did indeed in the previous years measure NOy completely. Since the experimental approach at Halley was much more superior to the NOy converter approach I feel the authors do themselves a disservice with this paragraph. Also, one needs a good measure of good-will to see in Figure 3 anything more than that the points were more or less in the same region.

Results 3.2.1

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A number of authors have addressed the question of where NO3- in the snow comes from with different approaches and results at Alert, Summit, and Spitsbergen in the Arctic. Yet, no field measurements are referenced in this section.

Judging from their titles, none of the 5 measurement papers 'in preparation' seem to talk about snow. A few words on snow sampling protocol, locations, analysis, errors, etc seems appropriate here. Was the snow uniform? Which layer was sampled? Was there a lot of fresh snow? Was it wind packed or remobilized by wind?

Sources of NOx 4.

Over the past 7 years a number of authors have made direct flux measurements of NOx and HONO at various Arctic and Antarctic locations. None of their papers are referenced. Their results might be a useful check whether the numerical exercise presented here is realistic.

Snowpack NOx emissions 4.1.3

A word of caution: Last year Beine et al. published some model results using a TUV model and compared them to their HONO measurements. It seemed that the model could not reproduce the complex chemistry in the various snowtypes and generate fluxes accurately. I suggest bringing the discussion on the uncertainties (4.3) to the front of the section so that the reader is aware of all the assumptions that go into this modeling exercise.

Outcome 4.2

I have never heard before that "NOxĚ is sometimes expanded to include HONOĚ" It would be useful to reference this statement.

If the authors can model NOx emissions from the snowpack, surely they can also model HONO emissions? Without this important link the whole approach seems simplistic. I like the way the individual contributions are shown in figure 6; this whole discussion would be much strengthened if snow pack HONO sources were included.

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It might be useful to state that the modeled NOx emission rates (in January) are much higher than those measured by Oncley et al. at South Pole [2004]; They are similar in magnitude to those measured in the Arctic.

Summary 6

The summary for the most part simply re-iterates the findings of the paper and should be shortened.

Typos etc.

P 4138, par 2, lines 4 and 9; pptv, not ppbv for HONO and HNO3.

Table 3b: end-september: the text states 1.22 E +6, not +7

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4127, 2007.