

## ***Interactive comment on “The role of ozone atmosphere-snow gas exchange on polar, boundary-layer tropospheric ozone – a review and sensitivity analysis” by D. Helmig et al.***

**Anonymous Referee #1**

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First of all this manuscript provides a brief review on ozone atmosphere-snow interaction in polar regions. In addition, a sensitivity analysis based on state of the art model calculations is presented. The goal of this evaluation is to assess the impact of ozone dry deposition on the tropospheric ozone budget in snow covered polar regions. In comparing their model results with measured tropospheric ozone data from different Arctic regions, the authors demonstrate that ozone dry deposition on snow surfaces should be less efficient as previously assumed and corresponding literature values seem to be overestimated. This finding has important implications for interpretation of the tropospheric ozone budget in polar regions. As for this point, the methodology is

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sound and assumptions are clearly and conscientiously identified, although there are some obscurities (see specific comments below). On the whole, I am confident that the evaluation presented is of high quality and sufficient to support the drawn conclusions. My main concern is, that the paper lacks of a clear outline. I think it is not a good idea to couple a review about “ozone atmosphere-snow exchange” with modelling efforts in this way. The model calculations are virtually restricted to only assess the role of ozone dry deposition and this is also clearly stated in the manuscript. More complicated processes like (photo-) chemical production/depletion at the atmosphere-firn interface can not be described with the present model layout and are beyond the scope of the analysis. Therefore, model output and measurements are compared for January, when these processes are assumed to be negligible. Consequently, at least the title of the manuscript should be changed, because the present title suggests that these physico-chemical processes are assessed in the sensitivity analysis. Nevertheless, the results and conclusions drawn from the model analysis are highly interesting and clearly worth to be published! Hence, my recommendation is: Why not focus the paper on the model analysis, which is a self-contained topic and adequately shorten the review part?

Specific comments:

1. Page 764, chapter 3: Did you use ECMWF reanalysis data to drive tracer transport? What was the temporal resolution of the reanalysis data in your different model runs?
2. Page 772-773 as well as figures 6-7: The agreement of model results with (local) measurements is really amazing, even on a hourly timescale (figure 7)! Please describe in more detail, how the model was initialized in these runs. As for me it is hard to accept that a global model with its inherent relatively moderate spatial resolution could provide such a close match with locally measured surface ozone data. Another point is the good reproduction of the annual cycle on the whole, except for sites where bromine induced surface ozone depletion is at work. This fact provokes the conclusion that snow pack (photo-) chemistry is of minor importance in determining the budget of tropospheric ozone in the snow covered Arctic. Could this point be another important

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outcome of your work? I think this aspect is worth to be discussed here.

3. Page 773, line 19: "... Summit mostly receives lower stratospheric (not tropospheric) air ..."

4. Page 774: The Conclusions chapter is not the right place for the listed "unanswered questions", because they can definitely not be derived from the presented analysis and should be removed here. A better place would be the end of chapter 2. Alternatively, some speculations about the situation in Antarctica may be advisable.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 755, 2006.

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