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Atmospheric Chemistry and Physics Discussions

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.

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The reviewer's and the editor's comments were helpful for improving the manuscript in several respects. Both were right that the original manuscript provided a rather weak linkage between the two introduction (1, 2) and the sensitivity and modeling sections (3-6). The high variability and inconsistency in published ozone flux data from the literature and the complexity of chemical processes affecting ozone in the snow and in the surface layer made us wonder how these discrepancies could possibly be explained, and which of the published data would be the most valid for a description of the large-scale polar tropospheric ozone chemistry. One could suggest to go out and perform new, and possibly better measurements, but a remaining question is if the discrepancies seen in the literature data may possibly also result from a high spatial and

temporal variability in ozone fluxes. Capturing these variations with ambient ozone flux experiments would be a tremendous task. Therefore, the sensitivity analysis with the model was chosen with the goal to narrow down the range for representative, large-scale ozone uptake rates to be recommended for use in atmospheric models. New text was inserted at the end of section 2 to better explain these connections. Furthermore, several sentences were added to the conclusion section that tie together conclusions from the literature review and from the sensitivity analysis to derive recommendations for model improvements.

A goal of the manuscript is to investigate the significance of processes contributing to ozone deposition to snow. And, at this time, the descriptions in models are limited to physical uptake mechanisms. The title does not name any chemical processes but, deliberately, is chosen to provide a more general description of the studied topic.

The model was initialised with fields from a climatological run of MATCH (von Kuhlmann et al., 2003) starting in October 1999 and allowing the model to spin up for three months presenting hourly ozone outputs for the year 2000 and monthly mean results for January 2000. These details are provided in the manuscript.

Since the lifetime of ozone is estimated to be $>$1 month for polar, wintertime conditions, atmospheric ozone levels are predominantly determined by large-scale effects, which reduces the sensitivity towards local, small-scale site heterogeneity and mixing conditions. Therefore, the differences between the model grid size and the measurement footprint are expected to be less important than in lower latitude situations.

The reviewer pointed out a valuable further interpretation, which we added to our discussion: We added some text in chapter 6 and in the conclusion section to point out the noted relatively low importance of local, springtime halogen-ozone depletion chemistry on the seasonal, large-scale Arctic ozone budget. Also, following the reviewer's suggestion, the listing of "unanswered questions" was moved to the end of section 2 and some further discussions on the remarkable surface layer ozone chemistry in Antarc-

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tica were added. Other errors pointed out by the reviewer were corrected. Figure 1 was improved, several references were updated.

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