Reply of the *Anonymous Referee #1*'s comments on "Modelling the Effect of Denitrification on Polar Ozone Depletion for Arctic Winter/Spring 2004/05" by W. Feng et al.

This paper presents model calculations on the effect of denitrification on Arctic ozone depletion. It is shown that a model with a detailed microphysical PSC scheme produces HNO3 (and by inference denitrification) in better agreement with observations than a model with a simpler equilibrium scheme or even a model without denitrification at all. The paper is in general well written and I recommend publication in ACP after consideration of a few mostly minor comments.

We thank the reviewer for his/her review and the positive comments.

General comment:
The result that denitrification affects Arctic ozone depletion is not new at all. I suggest to include a reference to Waibel et al., Arctic ozone loss due to denitrification, Science, 283, 2064-2069, 1999.

OK. We have added the references of *Waibel et al., 1999* and *Rex et al., 1997* in the revised paper.

Clearly the detailed microphysical scheme in the model results in a much better agreement with observed HNO3 than the simpler equilibrium scheme. However, the impact on modeled chlorine activation and ozone seems to be rather small (conclusions, page 3869: "...with a small effect on Arctic ozone loss"). So I don't understand why one of the conclusions is that "simulation of the impact of denitrification on ozone depletion...requires a detailed microphysical scheme" (abstract, p. 3858, l. 17-19 and conclusions, p.3869).

This small effect on chlorine activation is likely due in part because the two experiments (EXP_A and EXP_B) use the same surface of the liquid aerosol for the chlorine activation. We have changed the sentence as:
"Even though the modelled HNO_3 field is significantly improved when using the DLAPSE microphysical scheme (EXP_B), the differences of the simulated O_3 at 456 K from the SLIMCAT full chemistry experiments (EXP_A and EXP_B) are not significant, possibly because the same surface area of the liquid aerosol are used for the chlorine activation in these two experiments (EXP_A and EXP_B). This requires further detailed investigation."
Based on this we have removed the sentence of "simulation of the impact of denitrification on ozone depletion...requires a detailed microphysical scheme" in the abstract and conclusion.

Secondly, it is stated in the abstract that "use of the DLAPSE scheme improves the simulation of Arctic ozone depletion compared with the inferred partial column ozone loss from ozonesondes and satellite data". From the data in Table 2, the difference between EXP_A and EXP_B again seems rather small and I wonder if this difference is significant, given the large scatter of the inferred ozone loss from various observations. In fact, the discussion on p. 3867 appears to be much more cautious than the statement in the abstract.

We have updated Table 2 which now includes more references about the studies for Arctic 2004/05 ozone loss based on the Reviewer 2's suggestion. We also include the partial column or total column ozone loss. The ozone loss difference at 456 K between EXP_A

and EXP_B is small maybe due to the same liquid PSC surface areas used in these two experiments (see reply above). However, based on the inferred partial/total column ozone from satellite measurements (ACE-FTS, SAGE III, POAM III) and ozonesondes, the calculated partial column ozone from EXP_B has a better agreement compared with inferred from Match technique by Rex et al. (2006). Based on these, we have revised the discussion in the paper.

Specific comments: p. 3860: "A feature of the MLS technique...": I believe this applies to ASUR as well?

We have added ASUR here.

Table 1 lists the different treatments of denitrification in the different model runs. Are the PSC surface areas as used for the chemistry the same in all model runs?

Yes, we have already mentioned this in the paper.

Figs. 1 and 2: Are the horizontal bars on the ASUR curves error bars? If so, what kind of error do they represent? And why is the ozone error apparently so large below 25km and suddenly so much smaller above 25km?

Yes, the ASUR error bars are shown in Figs. 1 and 2. Based on Dr. Armin Kleinböhl's comment, we have changed the ASUR O3 original data uncertainty up to 12% in the plots. We have also added the following sentences "The total typical retrieval error is ∼20 % for HCl, ∼10% for ClO, and less than 15% for N_2O and 12% for O_3 (e.g., von König, 2002; Bremer et al., 2002; Kleinböhl, 2004; Kuttipurath et al., 2007)." in the paper.

Fig. 3: I guess it would be much more straight forward to show all differences with respect to the observations. I.e., (EXP_A-ASUR)/ASUR*100, (EXP_BASUR)/ ASUR*100, etc. In the current form the large differences to EXP_C (no denitrification) are highlighted, but it is more difficult to judge how good EXP_B agrees with the observations.

Thank you for the suggestion. The previous plot was to highlight the denitrification effect. We have now changed the Fig. 3 to show the differences with respect to ASUR observations. Here we removed the relative difference of HCl plot because of negative HCl values from ASUR, but we added a ClO plot for 31 January 2005.

p. 3868, l. 6: can you give numbers for EXP_A here as well, i.e. by how much the modeled ozone loss improves using the detailed scheme? This information (10DU) is only found in the next paragraph, if I understand this correctly. I suggest to combine the discussion of Figs. 7 and 8 and not to treat this as two independent pieces of evidence.

Yes. In the revised paper, we now give numbers for the EXP_A. We also combine the discussion of Figs. 7 and 8.

p. 3868, l. 26: "too strong chlorine activation": compared to what? ASUR / MLS data? The difference in chlorine activation (or more precisely modeled ClOx and HCl) seems small between EXP_A and EXP_B. If you believe this is a conclusion from this study, it would be good if you could show by how much chlorine activation changes / improves between EXP_A and EXP_B. (See my

comment to Fig.3 above.)

We apologies for the confusion. The model has too strong chlorine activation compared with MLS V1.5. Now we use MLS version 3.3 data and the modelled ClO profile compares quite well with MLS v3.3 by the end of Jan 2005 (see figure 1.). Therefore, we have removed this sentence and put in the following: "The model captures the temporal evolution of observed HCl and ClO from MLS measurements, but the model with full chemistry (EXP_A and EXP_B) seems to have too much chlorine activation from late February to mid-March 2005 which is also confirmed by Santee et al. (2008)." in the discussion of Figure 4.

p. 3869, l.8: "The simulations are QUITE SENSITIVE to the PSC schemes used in the model, with a SMALL EFFECT on Arctic ozone loss": To me this sounds selfcontradicting. Do you mean "quite sensitive" with respect to NOy, with "a small effect" on ozone loss?

We have changed the sentence as "The simulations of HNO3 are quite sensitive to the PSC schemes used in the model...."

Technical corrections:
p. 3859, last line: what is the meaning of "thereafter" here?
p. 3861, l.9 remove "as" before "vertical coordinate"
p. 3865, l.16: include "the" before "model"
p. 3866, l. 12: remove "which" before "consistent"
p. 3868, l. 22: "a three-dimensional chemical transport model" or "the three-dimensional chemical transport model SLIMCAT"

Thank you for these. We have corrected the text.