

Interactive comment on “Chemistry of hydrogen oxide radicals (HO_x) in the Arctic troposphere in spring” by J. Mao et al.

Anonymous Referee #1

Received and published: 30 April 2010

This manuscript presents a detailed model analysis of HO_x observations in the Arctic atmosphere. The model - a global 3D chemical transport model - is used to reproduce the observations and to calculate the HO_x budget in a region that is still not well known. The paper is well written and the subject is fit for Atmospheric Chemistry & Physics and presented in a clear way. Therefore I recommend publication following a few - minor - clarifications.

GENERAL COMMENTS

Section 3 should be a little more quantitative in describing the differences between the model and the measurements, especially for OH, NO_x and HNO₄. The discrepancy should always be mentioned, whether as a factor or as a percentage. Besides, it is not entirely true that satellite observations of NO are consistent with the measurements

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and the model, because the model clearly overestimates the measurements by several factors above 5 km. Please clarify or correct.

It is not very clear how the uptake of gases on aerosol is calculated. The authors should be more explicit than simply stating "a general representation for first-order uptake". In addition, how is the aerosol surface area determined? Since they say it was not measured, I assume it was calculated by the model or estimated in some way. Was the measured aerosol composition used to calculate the uptake coefficient, or was an average composition calculated and used? On the same subject, it does not seem that some basic sensitivity study was done. Given the uncertainties in the aerosol characteristics and in the HO₂ uptake coefficient itself (and the unknown role of the organics), and the importance of this process for the paper conclusions, it seems important to see how the model would respond to some changes in these parameters.

The model underestimates HCHO and NO in the lower troposphere and overestimates NO in the upper troposphere. NO₂ also seems to be slightly overestimated at higher altitudes. These discrepancies are significant and the authors should at least address them: how would a better agreement between modelled and measured NO_x affect the model results? Better, worse or no improvement, compared to the measurements? What would be the impact on the model results of 2-3 times higher [HCHO] and how would this affect the paper conclusions? Could the HCHO problem be an instrumental/inlet problem? I think the paper would greatly benefit by a short discussion of these questions.

SPECIFIC & TECHNICAL COMMENTS

page 6958, line 12 and elsewhere: "radials" instead of "radicals"

page 6959, line 5: maybe mention reaction with OH and explain how peroxides are converted to water.

page 6967, line 14 and page 6972, line 1: there is space missing after "HO₂"

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page 6970, line 21 and elsewhere: don't use "second simulation" or similar expressions, it is confusing: just say which model is being discussed (e.g.: "model with uptake of HO₂" or something like that).

page 6971, line 2: A change in [OH] of 58% is quite large and it would appear that it actually made the agreement with the measurements better. This is in contrast with the "no significant bias" statement on page 6965.

figure 1: adjust the x-axis of CH₃OOH, NO₂ and HNO₄ so that the lines are clearly visible: now they are squeezed together and this might suggest there is a better agreement than there actually is.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6955, 2010.