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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global stratospheric hydrogen peroxide distribution from MIPAS-Envisat full resolution spectra compared to KASIMA model results" *by* S. Versick et al.

Anonymous Referee #3

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General Comments –

The paper presents new retrieval results of global H2O2 distribution in the middle atmosphere from MIPAS instrument on board Envisat. Considering the importance of HOx chemistry in the middle atmosphere and the role of H2O2 as a reservoir species for HOx, the unique H2O2 data set from space-borne instrument helps provide valuable information about the hydrogen budget. The discussions of H2O2 diurnal and intraannual variations based on the comparison of model results and MIPAS data are also interesting. Such topic should be of interest to the science community of Atmospheric Chemistry and Physics. I recommend it to be published if the authors can address the

following comments carefully.

Specific Comments –

1) P33516: about the spectral range and the microwindow selection The spectral range in the text (Line 6) is 1210 – 1320 cm-1, while the range shown in Fig 1 is 1220 – 1300 cm-1. Suggest change the Fig to make it consistent. Also, the microwindows shown in Fig 1 have various widths, some may include several spectral lines, while the others may be tiny and hard to see from the figure. I think the authors should show at least one or two examples of such microwindows and add them as additional panels or "closeup" modes of Fig 1.

Since the signal to noise ratio if very low, it would be very important to see the retrieval residual and compare with the H2O2 spectral lines to ensure that the residual is random in nature.

2) P33519, Line 8-16: about the negative values in the profile Does the negative feature present in both daytime and nighttime profiles of MIPAS H2O2? Does the negative feature change with latitude or change with time of year? These may be useful information for the reader as well. Also, it may help evaluate whether it is a consistent bias of the measurement.

3) P33521, Line 8-10: Is the "daily mean vmrs from MIPAS data" here refers to daytime zonal mean only? If so, please specify it and please mention the satellite overpass time at the corresponding latitudes as well. A few pages later, when discussing the diurnal variation, the authors do mention the 10am and 10pm times, but here the description is not clear.

4) The comparison in Fig 7: The MIPAS profiles does not show the uncertainty (or error bars). I understand that the error bars might be very big and dominating. But this is important information to be included in the comparison plots (or at least mentioned in the caption and the text). Also the model results should include some uncertainty

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ranges as well. A good way to do it is to use the recommended uncertainty ranges of the key chemical reaction in the several kinetic references. This uncertainty in reaction rate should produce a corresponding uncertainty in the model results. With these "error bar" information included in Fig 7, the discussion will be more meaningful.

More importantly, the authors didn't address the different vertical profiles of H2O2 before MIPAS and various model calculations at the near polar regions in both southern and northern hemispheres. The shapes, not only the values, are different. And I suspect that such difference in profile shape can not be explained by adjusting one simple chemical reaction rate.

5) Fig 9, the annual variation Since the H2O2 mostly follows the overhead sun, it's the best to overplot the corresponding SZA (solar zenith angle) so that it can be easily shown.

6) P33525, Line 6: The authors avoid discussions on the discrepancies at polar region in Fig 9 by saying that it may be affected by the "energetic particle precipitation". But the "energetic particle precipitation" does not penetrate as deep as 30 km to affect H2O2 in Fig 9. It's influences are mostly in the mesosphere. I think the discrepancy here is again related to the unresolved/undiscussed discrepancy between modeled and retrieved H2O2 vertical profiles (as shown in Fig 7). It is fine that if the authors can not make any solid conclusions on it. But it is worth mentioning it in the conclusions.

7) Fig 11 about the diurnal cycle The authors only showed 30km data comparison. I think it is worth to show another altitude, for example 50km, where both model and MIPAS H2O2 show difference between day and night.

Technical Corrections / suggestions –

P33515, Line 7: "measurements before 2005" is a little confusing. The earlier paragraph mentioned that the spectral resolution changed in 2004 and the data discussed here are only data before that. So please include the specific date or at least the month

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of the cutoff date and make it consistent throughout the text.

P33523, Line15: “whole time period of MIPAS” Again, the authors mentioned earlier that they only use data before the resolution change in 2004. So the statement here is confusing. Please explain.

“3d-Chemical Transport Model” in a number of places in the text: I assume it means “3 dimensional”. Suggest to use “3D (3 dimensional) ” when it’s first introduced.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 33511, 2011.