

Interactive comment on “Simulation of stratospheric water vapor trends: impact on stratospheric ozone chemistry” by A. Stenke and V. Grewe

Anonymous Referee #4

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Summary

This paper analyses the impact of water vapour perturbations in a coupled chemistry-climate model. The authors are focusing on the chemical effect only by deliberately excluding the feedback of the changing atmospheric constituents on atmospheric dynamics. One interesting result is the asymmetry between the Arctic and Antarctic, where enhanced water vapour seems to have a larger effect over Antarctica, subsequently leading to higher ozone destruction there. This seems an interesting result and I believe the paper should be published after revision. I do not think that we require

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proof that the results are “correct” (whatever reviewer 1 wants to imply with this word) to assess if the paper should be published or not.

General Comments

Being late in writing this review (I was called in as a replacement reviewer) I have the advantage of being aware of the already existing review 1 and the associated reply. Even though I agree with many comments made by reviewer 1, I tend to feel less strongly about most.

In general I feel the paper would benefit from a section describing model deficits and strengths (lower upper boundary, trend in equivalent water, overestimation of water, etc.). I think we all have to accept that our chosen model systems are compromises, but that we can gain useful insights from these models when we asses them with their respective weaknesses in mind. If I understand the reply correctly, this will be part of the revision.

Specific Comments

Page 6562:

The Randel et al., JAS, 2004 study should be mentioned and discussed here, in conjunction with the 35% weaker trend modelled.

Page 6565:

The notation OH-S should be changed.

The number of chemical reactions could be reduced, but it could be useful to keep some key reactions like R10 and R11.

Page 6572:

I am slightly worried by the statement regarding the numerical effects leading to an unrealistic increase in ClONO_2 . Maybe it would be useful to remind the reader about the numerical solver used in ECHAM4.L39(DLR)/CHEM to calculate the chemistry at this point.

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Page 6575:

Are the ~ 190 K just a typos or are there any other reasons for this surprisingly low temperature – I would have expected something in the range of 192–203 K.

Page 6576:

Some re-phrasing of the second paragraph would improve readability, I believe.

Page 6578:

Similar to reviewer 1, I believe that the asymmetry between the two Polar Regions is the most interesting result of this study. Therefore I would suggest that the conclusions section should be restructured with a stronger focus on the hemispheric differences and a discussion of possible reasons and implications: Why does it happen in the model? Could the “real response” in the atmosphere be the same? I appreciate that part of the new discussion may be speculative, but carefully phrased I wouldn't object some speculations in a conclusions section.

Page 6579:

I would not stress the linear relation between ozone response and stratospheric water vapour too strongly, given that there are only two points and a coordinate system origin. I don't think Figure 14 is needed here.

Summary

I suggest the publication of this paper in ACP after revision. As mentioned earlier, I think it would be beneficial for the paper to stress the differences between Arctic and Antarctic more (in the main text and in the summary). In addition, I think it would be beneficial to have a small (extended) discussion of model properties relating to the water vapour (weaknesses/strengths). In the light of the one numerical problem mentioned in conjunction with figure 9 it would be useful to have a small reminder of the approach utilized to solve the chemical reactions equations. Judging from the authors reply to reviewer 1, I have no doubt that they can provide the requested information/restructuring of the paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6559, 2004.

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