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Geoscientific
Model Development
Discussions

Interactive comment on "The Lagrangian chemistry and transport model ATLAS: simulation and validation of stratospheric chemistry and ozone loss in the winter 1999/2000" *by* I. Wohltmann et al.

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The paper describes the chemistry part of the model ATLAS including initialisation and particle sedimentation. The general ideas of the model ATLAS are identical to those of the model CLaMS. The conceptual differences to CLaMS seem to be rather minor to me: A different numerical solver, slight differences in the handling of "particle parcels" used for sedimentation, improved meteorological analyses. It is emphasized that the model ATLAS has been coded independently from CLaMS. Although this is not a value on its own, it demonstrates the reproducability using the same concept. To my first

impression, the results of ATLAS seem to be of similar quality that those of CLaMS.

Shown are model results for the winter 1999/2000. For this winter, a CLaMS simulation is published only in a very preliminary version of CLaMS. Thus the paper shows the first simulation for the winter 1999/2000 for such kind of a Lagrangian model. The paper is well written with a sufficient levels of detail. A crucial point for these simulations is the initialisation, that seems to be done very well and is well documented.

Even though the innovation with respect to the model concept is limited in comparison to the existing concept of CLaMS, I would recommend this paper to be published in GMD, since it contains new aspects of the simulation of the winter 1999/2000. Also, a publication of the model description to the present, sufficient detail is valuable also for the understanding of possible future publications of this model. However, some clarifications and improvements of the text are needed prior to publication that are described in detail below.

Major points

1. Heterogeneous chemistry: 773.24ff, 776.11f, 778.6-10
   It is said that NAT and ice particles are formed directly when temperatures fall below T_{NAT} and T_{ice}, respectively. This is in contrast to well known observations (e.g. Dye et al., 1992) and would also lead to no growth of the NAT particles responsible for denitrification. Later, on page 776.11ff it is then explained, why some super-saturation needs to be allowed (if I understand this correctly). I suggest to describe the model allowing NAT super-saturation is the “standard configuration" and a rewrite of the relevant parts in the text of the paper in a consistent way.

2. 781.25ff: A detailed Comparison with ER-2 and OMS observations and the HALOE climatology is given in the electronic supplement. These plots are valu-

GMDD
3, C144–C148, 2010

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able, since they contribute to the model validation and demonstrate the good performance of the model and that not only one best example of comparison is shown in the paper. However, the plots is not commented and therefore the reader needs to get the interpretation on its own. It would be desirable to get some interpretation like at least some average deviation between model and observation. Otherwise the reader must have special knowledge to understand how to interpret and evaluate the differences between model and observations.

3. 782.14ff, Determining ozone loss in the model:
   There are two principal differences, how ozone loss in the model could be determined. The classical way is to show the difference of model ozone to a passive ozone tracer as it is shown here. The Match and vortex average methods estimate ozone loss from observations assuming no transport of air through the vortex edge by integrating the vortex-wide ozone loss rates. This problem is in detail described by Grooß et al. [ACP, 2008]. In the model ATLAS, both methods for determining ozone loss could be applied. As also the model includes the transport of air through the vortex edge it would be valuable to know whether the differences in March occur due to under-estimated ozone loss in the model or due to transport of air through the vortex edge.

Minor points

1. 771.25ff, remark about denitrification:
   It is probably a greater model challenge to simulate denitrification for winters with intermediate temperatures, but of course, the availability of observations gives good arguments for choosing this winter.

2. 772.9: Is there a separate description of the chemistry module? If so, give the citation.

GMDD
3, C144–C148, 2010

3. 777ff: Model setup
   Nothing is said about how the lower and upper boundary is handled. It is probably “open" meaning no flux of chemical compounds entering the model domain from below or above. Please clarify.

4. 777.10: I assume that the heating rates are used to derive the vertical velocity. It should be mentioned here.

5. 777.25: The number of N atoms cannot be changed by the chemistry, but as N_2 and N_2O is not part of NO_y, NO_y is indeed changed by chemistry (even though minor) e.g. by reactions including N_2O.

6. 780.4: please note that the ACE-data are observations from different year(s)

7. 782.23: What is the altitude resolution of the current model run?

8. 783.6: rephrase to clarify, e.g. “...the comparison with ... indicates a slight over-estimation of ozone loss by the model"

9. 783.26: “The vortex-average method is not applicable to the upper peak..." Please explain this argument shortly. Also note, the NO_x-catalyzed ozone loss peak is well below 35km here.

10. 783.24 “the general circulation":
    If the authors mean the Brewer-Dobson circulations, then one would need a longer simulation time. Please clarify.

11. 785.25ff “Differences...": Although the tracer simulation seem to be reproduced well using ATLAS and ERA-interim, the given confidence is not a proof. Therefore the authors should rather write as “Differences ... are therefore likely/rather due to..." or similar

GMDD
3, C144–C148, 2010

12. 787.1ff, state of maturity of all CTMs: Conclusions can only be given for ATLAS as no comparison is given with other CTMs here. I would be surprised if Eulerian CTMs with similar resolution would also show similar comparisons with the observations. It is true that some processes are easy to model since they are in a kind of saturation (e.g. denitrification and chlorine activations) Remaining uncertainties could be tested best non-saturated for cases, e.g. winters with intermediate temperatures.

13. supplement, fig53d: I would use ClOx instead of ClO since the day-night sampling is not clear...

Interactive comment on Geosci. Model Dev. Discuss., 3, 769, 2010.