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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Key aspects of stratospheric tracer modeling" *by* B. Bregman et al.

B. Bregman et al.

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Reply referee #1

We thank the referee for the constructive comments. Below we give our reply.

General comments

We have restructured our manuscript considerably and used consistent labeling of the model experiments. The model experiments have been explained more clearly by reorganizing the section Experimental set-up and together with the adjusted labeling the experimental overview is now improved. We also divided the passive tracer results into different subsections and added a subsection 'Diffusivity and model grid resolution' to improve the readability.

We have added a section Discussion where parts of the introduction have been moved as suggested by the referee.

With permission of the referee we have added the results of an important additional dispersion calculation demonstrating the great sensitivity of air parcel dispersion to the starting altitude of the back trajectory calculations and discuss this issue in the section Discussion.

Bullet 1:

This sentence has been changed and the part between brackets has been removed.

Bullet 2:

The manuscript does not mention explicitly the existence of a model experiment using three-hourly instantaneous winds. Line 25 says: ’Using the default setup we used 3-hourly instead of 6-hourly winds’. Nevertheless, we realize that confusion may arise and adjusted the text accordingly to the adjusted labeling.

The referee concludes that 3-hourly instantaneous winds should be used to justify the conclusions on p 4389, lines 17-21. However, we feel that these conclusions are valid based on the current set of model experiments. The differences in the results between the model experiments 6-hourly instantaneous and 6-hourly interpolated already indicate the effect of time interpolation. The differences in the results between the model experiments 6-hourly interpolated and 3-hourly interpolated indicate the effect of update frequency.

The effect of using instantaneous or time interpolated winds is rather marginal. We have stated this more clearly in the text and stressed the importance of the update frequency which yields a much stronger improvement.

Bullet 3:

We have revised the manuscript accordingly and provided a clear separation between model approach experimental set-up and divided the experimental set-up section in

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three subsections, each describing the specific diagnostics applied in this study.

Additional points made by the referee:

The referee questions the usefulness of the back trajectory experiments and the differences with the Scheele et al. results. Scheele et al. do not address the 3-hourly interpolated winds. We have emphasized this more clearly in the text. The reason to include some of the old Scheele et al. results is to provide a clear comparison and to avoid forcing the reader to examine a separated paper. The referee is indeed right by concluding that 3-hourly interpolated 3DVAR is better than 6-hourly interpolated 4DVAR. We stressed in the text that this is an important message for modelers using ERA40 data which is 3DVAR.

We haven't applied 3DVAR in our CTM studies since it is well known in the modeling community that the OD data is generally of better quality, especially in the polar regions, so that we do not add useful information by extending the CTM experiments with ERA40 data. We have stated this more clearly in the Introduction section. In the section Conclusion and Discussion we additionally stressed that the findings from our back-trajectory experiments may lead to promising results using re-analyses data when combined with improvements in the data assimilation system.

We agree with the referee that the Antarctic is a very interesting region. There are however several reasons to choose the Arctic region.

(1) There have been previous model evaluations of the Arctic 1999-2000 winter using the same observations so that the results can be compared straightforward so that potential model improvements will be observed more clearly.

(2) The effect of using different assimilated winds for the Antarctic has been extensively discussed in another paper (Manney et al., 2005) which is mentioned in the manuscript.

(3) The Arctic polar vortex is much more dynamical, providing more challenge to the models.

(4) An additional evaluation of the Antarctic would further increase the length of the manuscript.

We have added more information to the text from the van den Broek et al. (2003) study.

The referee believes the comparison with the methane profiles is not sufficient for the model validation. The main reason for using these balloon-borne observations is to compare the CH4 distributions from the model updates with those from the previous model versions using the same diagnostics and not to provide a complete and extensive model validation of the Arctic region. We have emphasized this more clearly in the Introduction section. compared to the previous model validation (Van den Broek et al.) we extended our equivalent latitude diagnostics.

In combination with the model experiment labeling we have adjusted and extended the description of these experiments accordingly. We also omitted the '30L' model experiment since we have no sufficiently available in-situ observations at altitudes higher than 10 hPa. Moreover, the model profiles show no negligible differences with the '6-hourly instantaneous' model experiment at lower altitudes, and we further believe the readability of the corresponding figure will be improved when omitting this profile. Finally, we are cautious by examining the profiles at high altitudes where the chemical life time of CH4 becomes much shorter, affecting the distributions on too short time scales, even in early spring. We therefore restricted the text to the statement that the calculated profiles are not sensitive to vertical resolution in the lower stratosphere and stressed that sensitivity integrations give negligible differences below 10 hPa.

The referee suggests to adjust the title. We agree with the referee that the study may not cover all aspects of stratospheric modeling. We have extended the title with: "using assimilated winds". Nevertheless, some aspects (wind variability) also apply to climate models.

Specific comments.

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We have adjusted/corrected the text and figures according to the referee suggestions. We have added additional explanations where required by the referee for all comments. Below we give additional reply for some of the comments.

We have explained more clearly the advantage of a sigma-pressure coordinate in the presence of clouds, but have skipped convective processes, since is it not straightforward that this coordinate has more advantage over a sigma-theta coordinate.

The introduction section has been reduced and the sections experimental setup and model description have been re-organised as mentioned above. In addition, some parts in the introduction have been moved to the new section Conclusions and Discussion.

More explanation about the experiments is given in the new sub-divided section Experimental setup.

There is no difference between update time intervals and update frequency. The text has been adjusted so that one consistent naming is used.

With wind fluxes is meant ‘mass fluxes’. The text has been corrected accordingly.

The advantage of using hybrid-sigma pressure coordinates is valid when clouds are present. We agree with the referee that this advantage is not straightforward for convective processes and we have therefore omitted the latter. By using theta as vertical grid definition the vertical transport is calculated on basis of heating rates, which are not trivial to calculate in (active) cloudy environments, introduction uncertainty in the vertical transport. We have stated this more clearly in the related section.

Grid zooming can only be performed with first-order (slopes) advection, because of the two-way communication between the zoom and the parent regions. Additional communication (halo) grid cells have to be introduced when adding more moments in the advection. This requires a fundamental and serious update of the model transport modules for which there has been no time available so far. We have included this

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explanation in the text.

We have added more information in the sub-divided section Experimental Setup about the balloon-borne methane profiles, their location and how the model profile interpolation is performed.

We have added the “slopes’ results. We also added the “slopes” results in the mean age of air calculations.

We believe the referee points to the results from the zooming experiments when asking about the differences between the new and the Van den Broek et al. results. We have added that when applying the same grid averaging in the polar region as in the model with coarser grid resolution there will be no difference in tracer distribution when increasing the resolution.

Figure 4: We have re-organized the figures containing the methane profile comparisons and included 2000-02-27.

The number of data points in Figure 7 is not reduced when applying polar grid reduction, since the grid reduction is performed over the longitudes and not the latitudes. We have stated this more clearly in the Model description section. Each data point represents a longitudinally averaged model value for each latitude.

The referee suggests adding a zonal mean tropopause in figure 12. Considerable efforts were required to include these and we believe that by adding a ‘mean’ tropopause may mislead the reader, since many individual particles could be located in the stratosphere, but in the troposphere according to the ‘mean’ tropopause. Moreover, the purpose of this figure is to show the differences in the dispersion in the lower stratosphere, rather than showing details in the separation between stratosphere and troposphere. For the latter figure 13 is helpful, since this figure focuses on the stratospheric air parcels only.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4375, 2006.