Atmospheric Chemistry and Physics Discussions

Interactive comment on "Implementation issues in chemistry and transport models" *by* S. E. Strahan and B. C. Polansky
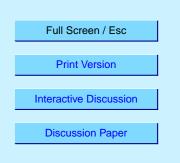
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Most or all of the referees had suggestions for material to add, while simulataneously suggesting shortening the paper (although no specific suggestions were given regarding what to leave out). I too prefer papers that are concise. I have done my best throughout the paper to eliminate any duplication and to reword for maximum impact without eliminating the important scientific results of the paper. I have added explanation where requested by the referees while shortening the paper by about half a page.

Referee #1

This referee has 2 major comments and a number of minor comments. The first major

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comment is about the lack of comparison of the model results with in situ aircraft data. This comment refers to the satellite data used in the diagnostic that evaluates mixing and transport across the edge of the Antarctic vortex.

The purpose of this diagnostic is to evaluate the behavior of the model vortex on seasonal time scales; it does not purport to diagnose all aspects of vortex processes. On seasonal time scales, the diagnostic examines the integrated or net effect of smaller scale processes such as filamentation. This is not to say that filamentation isn't important, only that the diagnostic that one can derive from the HALOE observations cannot address this process. HALOE does not have the spatial coverage to do sensibly look at day to day mechanics of vortex dynamics. Instead, HALOE observations are extremely useful because the low interannual variability in the Antarctic allows one to sensibly combine the 12 years of measurements in a statistical analysis, producing a picture of the seasonal evolution of the vortex.

The referee believes aircraft data should be used in a comparison. Unfortunately, there is no aircraft data set with sufficient coverage in space and time to create an adequate model diagnostic. I have authored of several papers using Arctic and Antarctic aircraft data to examine vortex structure and evolution. I can tell you from experience that the most one can get out of the aircraft data is a monthly mean picture of the structure. There is insufficient data to derive a climatology of filamentation. An aircraft data set does not give you the 3D (latitude-longitude-time) data set necessary to quantify small scale processes around the vortex for an entire season.

Thuerefore, this paper cannot diagnose small scale processes in the atmosphere or the model. However, the HALOE-derived diagnostic can draw conclusions about the overall seasonal representation of the model vortex and how this may affect chemical containment.

Another major comment was that the referee would like to have seen a recommendation of the optimal vertical resolution and/or aspect ratio for resolving important trans-

port properties. The inherent transport properties of the met fields come from the resolution of the general circulation model that produced them, not from the resolution of the CTM. The question of aspect ratio is not relevant to a CTM. This paper addresses the use of met fields in a CTM and thus the topic of optimal aspect ratio in a GCM is far outside the scope of this paper. The question posed in this study regarding vertical resolution is: are you compromising transport by running the CTM at a vertical resolution less than the original resolution of the met fields? This is a sensible question to ask in a CTM framework and that has been addressed.

Minor comments:

1) Too much 'discussion of the GMI CTM'. I've searched through the text and every mention of the GMI CTM involves a reference to a relevant GMI study, for example, where a diagnostic had been previously used or described. Studies that diagnose transport in the GMI CTM are also occasionally referenced because, as pointed out in the first mention of the GMI CTM, its experiments uses the same advection core and very similar wind fields, and thus its transport characteristics will be very similar to those of the experiments used in this study.

2) Recommendation to shorten the intro and put Section 3.1 into it. I have given this serious consideration but have decided not to. The paper's intro is on the need for CTMs in the world of chemistry-climate models (CCMs) and why we must carefully consider how we use CTMs if we are to get meaning from their results. Section 3.1 is a far more specific introduction to the sensitivity studies here and is closely related to Section 3.2 which discusses how and why a few key species can be used to make useful diagnostics. It makes sense for these sections to be together, and for them both to come immediately before the evaluations.

3) Explain 'large transients in vertical velocity'. 'Large transients' means extrema in vertical velocity occurring on short time scales. By 'short' we mean less than the 6-hr averages of u and v that are used to run our CTM (or the daily averages used in

the Rasch et al. reference). Averaging u and v eliminates their extrema. Thus, w calculated from their averages (in the CTM) may be much less than the 'effective w' experienced by constituents in an online calculation. The text on p. 4 is changed to "This was attributed to using daily-averaged winds that eliminated the large transients in vertical velocities that may be responsible for significant vertical transport on short time scales."

4) explanation of differences between online and offline calculations. It makes sense to reorder the sentences as you suggested, as it gives me a chance to better explain what I mean about the effect of faster photochemistry. See p. 9/top of p. 10 for changes.

5) The level spacing in the two models is very close to the same but is not precisely equal. This is actually of no consequence and the text was confusing. Figure 4 has been changed to show the 100 and 10 hPa levels and the mention of different spacing has been eliminated.

6) Explain the impact of level spacing on transport in term vertical diffusion. I have added a sentence stating that this might be the explanation, but the cause might also be an artifact of the method used to map the u and v fields to lower vertical resolution.

7) I have added a line to the model description (Section 2, p. 6) stating that there is no upper boundary condition for CH4.

Referee #2

General

The issue of data resolution with respect to model resolution: The reference noted in this comment (Khosrawi, 2005) uses 'different model setups' because they are running a trajectory model. Our model is not a trajectory model, it is a 3-dimensional advection calculation with chemistry. A trajectory model has no inherent diffusive mixing and because of this, the Khosrawi study had to tune their model to get a tracer distribution (pdf) that looked like the observations. There is no tuning of diffusion in an advective

CTM such as ours.

The issue of aspect ratio is important for a general circulation model that is generating the wind fields used in the CTM. The 'aspect ratio' of the CTM has no effect on the information content of the GCM wind fields (i.e., on the scale of the forcings in the wind fields, which depend only on the resolution of the GCM). Optimization of the residual circulation in a GCM is far outside the scope of this paper. This paper deals strictly with the implementation of met fields within a CTM.

I have considered the general request to reduce the figures, but I find them all essential to demonstrating the results of the CTM experiments.

Specifics I agree with the suggestion the change the title. It is now: "Meteorological implementation in chemistry and transport models".

This question - which model is closer to reality - is not actually relevant to the subject of this section. The general subject of the paper is, how are constituent distributions affected by making compromises to a more thorough CTM calculation. For example, you can run a simulation faster by doing the chemical calculation in an offline model than if you do it online in the GCM. Thus the first question you must ask is, how different is the CTM calculation to the online chemical calculation? That is the kind of question addressed here. To study a question of climate or atmospheric composition, yes, we would want to know which model is more realistic.

Request for more quantitative comparisons. There are already quantitative comparisons where I feel they may be useful (e.g., percentages differences in CH4 fields). I don't think the qualitative comparisons made on the pages cited by the referee will be any more illuminating if I attach a number to them. Perhaps if the referee could point to a specific comparison and say what quantity would be useful.

On Section 5.4. As I noted above, I am changing the resolution of the CTM and this is different from changing the resolution (or aspect ratio) of the GCM that produced the

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met fields. This section does state that the results shown are specifically for the Lin and Rood advection core used here. I have not worked with any other advection cores so I cannot say how a more diffusive one might respond to these tests. The abstract does state the conclusions apply to the Lin and Rood CTM, but to make it clearer, a sentence (near the beginning of the abstract) has been modified: "In this paper we present a series of sensitivity experiments on a CTM using the Lin and Rood advection scheme, each differing from another by a single feature of the implementation."

Details p. 10220. in the tropical lower stratosphere (added to text). p. 10221. This is the terminology of author of the advection code (S.J. Lin). Not wishing to make the text any longer than necessary, I refer the referee to the cited references. p. 10222. Global temperature distributions, polar winter temperatures, tropical midlatitude separation, some aspects of upper stratospheric transport,Ě.quite a number of results from those papers are relevant to this study. In trying to keep the paper shorter, I think it is best for the interested reader to go to the cited references. p. 10222. This information can be found within the reference given, but I will add this information (Russell and Lerner, 1981). p. 10224 & p. 10228 - wording changes. Done.

Referee #3

Only minor comments p. 10221. Did we repeat the studies with assimilated met fields? No. We wanted to focus on met fields that had a decent residual circulation that would be useful for long-term (multi-decadal) studies, for example, of ozone recovery. Assimilated fields have too many problems with their residual circulation for long-term simulations.

p.10222. The age tracer doe not have a vertical distribution. It is released at the bottom two layers of the model (the boundary layer and just above). It is released for a period of 1 month, and after that, age tracer mixing ratio is forced to zero at the ground. This is effectively a surface deposition. This additional description has been added to the text.

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p. 10223. I know and I agree. I added a sentence noting the nondiffusive nature of the SOM scheme comes from the higher order moment info that it carries.

p. 10226. Actually, it is a mass flux conserving method that we use to lower the vertical resolution. I rewrote this sentence to note that the method conserves mass flux and divergence. The way we choose the number of levels for the reduced res is somewhat arbitrary and not that exciting. I don't want to waste time in the paper nonessential on this detail.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10217, 2005.

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