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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of model grid zooming on tracer transport in the 1999/2000 Arctic polar vortex" *by* M. M. P. van den Broek et al.

Anonymous Referee #1

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This paper looks at the effects of various resolutions on the transport of HF and CH4. The model is driven with ECMWF winds. On average, the paper is fair. It is clearly written with pretty good referencing and figures that illustrate the author's main points.

On the positive side:

1) The paper reveals the impact of various resolutions in chemical transport models (CTMs). Namely, all resolutions produce a reasonable mid-latitude simulation, while only the finer resolution models are adequate for simulating the Arctic polar vortex.

2) The paper shows that ECMWF descent rates may not be particularly good. This suggests that the community needs to re-evaluate the numerous seasonal to inter-annual transport studies that are driven by analyzed winds derived from assimilation systems. While assimilated observations may provide excellent synoptic to planetary

scale views of the vortex, small systematic biases may confound longer term transport studies. Isentropic models (i.e., SLIMCAT) seem to be able to do reasonable seasonal simulations, so this suggests that the divergence fields in the assimilation fields may be incorrect.

On the negative side:

1) HALOE does not really sample the polar vortex very well. Hence, initialization with HALOE is fundamentally flawed. The model simulations need to be re-run with a better initialization. It seems that the discrepancy in all of the runs could be a result of a poor polar initialization.

2) It seems rather obvious that a 6°x9° model doesn't represent the vortex particularly well. We know that the edge is fairly sharp from satellite, balloon, and aircraft trace gas observations and standard PV distributions, so a 6° latitude resolution is clearly inadequate. From the model output comparisons to the data, it is apparent that the model output are more consistent with one another than with the observations.

3) The vertical advection problem has its greatest affect on the 6°x9° model (Fig. 7a). I assume that if the vertical velocities were better, than the comparisons for all of the model output to data comparisons shown in Figs. 3-6 would also be improved. The poor vertical advection in the model may be exacerbated by the coarse resolution. The authors need to explore whether reasonable downward advection would create a reasonable coarse resolution model comparison to data. The root of the poor comparison may be the interaction of the poor vertical velocities and the coarse resolution. Perhaps the authors should consider some model runs with an artificial tracer that has only vertical stratification (or initialized on isentropic surfaces), then compare the differences in this tracer's evolution.

4) In spite of the fact that the model output poorly represents vortex tracers, the authors proceed to estimate vertical advection inside the vortex from these tracers. In fact, ozone loss in 1999/2000 was greatest at an effective potential temperature of about 450

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K. Therefore the model poorly represents vertical advection in precisely the layer where it needs to be most precise. The authors should probably investigate the downward advection a bit more carefully since they conclude that the descent is probably the root of their problems. The descent shown in Fig. 7 should be examined as a function of proximity to the vortex edge in addition to altitude resolution.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2261, 2003.